Jens Høyrup

Selected Essays on Pre- and Early Modern Mathematical Practice

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Part I
Main Roads and Odd Corners

Chapter 1
Introduction

Generalities

The present book grew out of an overrating of my compeence. In September 2017 I was asked by Springer to give commentaries and make a report about a submitted manuscript dealing with Cauchy. I had to tell that I knew too little about Cauchy, and even less about research dealing with Cauchy and his mathematics, to be able to do that. I was then asked whether I could instead propose anything from my own hand, and I suggested an anthology containing mostly articles I have written within the last two decades. That was accepted, and later my selection was accepted by referees.

I decided not merely to republish facsimiles of the old articles, which had been formatted according to a variety of house styles. Instead, the material is presented in a unified layout and style. I choose to use the opportunity also to make small stylistic corrections silently, and to insert a few cross-references and further observations (and a single correction of an error) – all of these clearly marked in ⟦…⟧. Apart from that, the substance of the original articles is untouched.

Many of the original articles were initially presented to conferences or workshops and addressed the particular themes of these. Since I often draw on the same primary material, and since the various audiences could not be presupposed to be familiar with this background, a number of basic explanations are given in more or less identical form in several articles. The rare reader who reads everything at a stretch will hopefully learn to slide over the repetitions; for the reader who concentrates on a single article it should be convenient that necessary background is provided there.

The bibliographies for the single articles have been brought into a common format but not collected into a single bibliography. In general I use the author-date system, though sometimes standard abbreviations such as MKT or PL occur; all bibliographic references stand as […]. The final index is not split into a name- and a subject index – it seemed awkward to me to list, for instance, "Euclid" in one and "Euclid, *Elements*" in the other.

The two parts

The subtitle of Part I is "Main Roads and Odd Corners", that of Part II "Oblique Glances and Birds-Eye Views". In the words of one referee, the former "contains mainly articles in history of mathematics in the narrow sense", and the latter "articles related more to historiography and cultural aspects of the mathematics". This corresponds on the whole to my own idea, although I would hardly have dared to express it so sharply. In both, the underlying view throughout is that mathematics has to be investigated as *a practice* (whether this word is mentioned or not), not as some abstract entity located in

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Popper's Third World, nor as an extension of pure logic. This should be a more broadly accepted view today than when an early work on mine about Jordanus of Nemore was blocked from a proceedings volume by "a famous Finnish logician" (as I was told by the editors of the volume) – and then kindly published as [Høyrup 1988] by Jean Paul Van Bendegem.

It goes by itself, I suppose, that a genuine practice is much more complex than can be reflected in any analysis. I therefore do not pretend to deal with more than a fews aspects of the matter; nor are the aspects that are taken up identical from one article to the next.

The single articles

As quoted, Part I may be claimed to contain "mainly articles in history of mathematics in the narrow sense".

The first of them, *I.1, "On Parts of Parts and Ascending Continued Fractions"*, indeed deals with a seemingly narrow problem, namely two specific ways to speak about fractions. One of them, "parts of parts", for example "two-thirds of one fourth", seems so straightforward that it has (as far as I know) never aroused much interest. The other, the "ascending continued fractions" (such as "three-fourths, and two-thirds of one fourth") has been taken note of in various contexts. It is well known that Leonardo Fibonacci borrowed them from Arabic mathematics, where it is in regular use, and even borrowed a notation for them from Maghreb mathematics. In [1982], Kurt Vogel followed their further appearances in European mathematics until Clavius; several Assyriologists have observed them in texts from various periods of Babylonian history, but without being aware of their presence in Arabic mathematics. It has regularly been surmised that the Arabic fractional idiom descended from the (totally different) Pharaonic use of unit fractions, while the extremely rare occurrences of the two types of composite fractions in the Pharaonic mathematical corpus have gone unnoticed. So has the presence of a distorted version in a specific subset of the arithmetical epigrams of the *Greek Anthology.*

Closer analysis of all this (and a bit more) allows a number of observations concerning the relation between scribal and vernacular mathematics in the Near Eastern Bronze Age cultures; it also allows the formulation of plausible hypotheses concerning the origin of the Pharaonic unit fraction system; finally, it leads to the framing of a scenario for the restricted diffusion of the idiom from Semitic to other language areas.

The short *I.2, "A Note on Old Babylonian Computational Techniques"*, also deals with a problem belonging to the history of mathematics narrowly understood. As early as [1863: 33], Moritz Cantor suggested the Babylonians to have made use of a reckoning board. He repeated the idea in his *Vorlesungen* [1907: 42], emphasizing however that this was nothing but a reasonable assumption and not supported by any kind of positive evidence. After Denise Schmandt-Besserat's discovery of the Near Eastern "token system"

it seemed an even more reasonable assumption, but what had looked as positive philological evidence turned out to be an illusion.

The article analyzes calculational errors found in two Old Babylonian texts, showing that multiplications were performed in a medium where intermediate steps disappear from view once they have been performed (as on an abacus, but not in our paper algorithms), and suggesting that a counter meaning *1* (in any sexagesimal order of magnitude) might be displaced to a neighbouring order of magnitude but would not easily become *10* in the same or a neighbouring order of magnitude. When Christine Proust received the preprint of the article, she immediately argued [Proust 2000] from other errors that the calculational device represented four or five sexagesimal orders of magnitude (which means that calculations going beyond this range had to combine operation on two or three devices, giving rise to the errors; and that the device was called a "hand". The latter suggestion has now been confirmed by evidence reaching from the 26th to the 5th century BCE.

I.3, "On a Collection of Geometrical Riddles ...", was first presented in 1997 to the workshop "Transmission of (arithmetical/geometrical/recreational) Problems", Mathematisches Forschungsinstitut Oberwolfach. It takes as quasi-theoretical foundation my earlier work on the distinction between apprenticeship-based and school-based pre-modern mathematical practice – the former being of oral, the latter of scribal and hence literate cultural type – and on the importance of "neck-riddles" for the formation of professional identity in the former.

Combining sources from many cultures – Old and Late Babylonian, ancient Greek, medieval Islamic, Latin and Italian – the article identifies a handful of geometrical riddles apparently carried by environments of non-scribal surveyors at least from c. 2000 BCE to the end of the first millennium CE, first giving rise to the creation of Old-Babylonian so-called "algebra", later taken up in ancient Greek pseudo-Heronic and Indian Jaina geometry, submitted to critique in *Elements* II, and used by al-Khwārizmī to justify the rules for solving mixed second-degree problems.

I.4, "Mahāvīra's Geometrical Problems", was presented 1998 at the symposium "History of Mathematics: Mathematics in the Americas and the Far East, 1800–1940", Mathematisches Forschungsinstitut Oberwolfach (with special permission, since it missed the time limit by a millennium). The first section recapitulates the matters that are presented in depth in article I.3, while the second elaborates what is said in a few paragraphs in the section "Adoptions IV" of that article.

In its full form, *I.5, "Sanskrit-Prakrit Interaction in Elementary Mathematics ...",* originated in 2012 as a contribution to the "International Seminar on History of Mathematics", Ramjas College, Delhi University, but some of the ideas are already sketched in [Høyrup 2007: 59–62] – not quite correctly, at the time I still accepted the Euclidean interpretation of al-Khwārizmī's approach. The theme is indeed something which

had occupied me and about which I had collected observations for at least a decade before 2012.

The way this article traces the diffusion of the rule of three is to some extent a parallel to how article I.3 follows the life of the surveyors' riddles. An important difference is that the evidence we have for the existence of these riddles, though rich, is never fully direct: in contrast, the way scholarly sources refer to the vernacular formulation of the rule of three seems to be very close to how merchants themselves would be trained – actually, how Italian merchants were trained in the abbacus school. Another obvious difference is evidently that the surveyors' riddles were mathematically fruitful and fecundated a number of "high" mathematical traditions. The rule of three, on its part, did not stimulate mathematical theory although it remained an essential tool in commercial practice for long, earning itself the name of "golden rule"; but once those who used it had learned basic proportion theory or elementary first-degree algebra it was left behind, having nothing but calculational convenience to offer – and that only on the condition that it was continually trained. None the less, its diffusion offers information about social interaction and sharing of knowledge and techniques around the Indian Ocean and the Mediterranean during the Middle Ages.

I.6, "Geometrical Patterns in the Pre-Classical Greek Area", was published in 2000, but in essentially the same form presented in 1992 as a contribution to the Symposium "Early Greek Mathematics" in Athens; the basic ideas struck me during a visit to the Archaeological Museum in Athens as early as 1983.

At the Athens symposium I started by admitting that much of what I was going to speak about might not be considered Greek; whether it was mathematics could be subjected to doubt, and that was indeed my point; "but it is certainly early".

The article aims at establishing tentative tools allowing to distinguish such geometric decorations of pottery or other artefacts whose regularities go beyond what immediately impresses the eye from such as have no such pretensions. The former type can reasonably be regarded as "mathematical", the latter not; I propose the term "geometrical impressionism".

The decorations of the "Old European" culture from the sixth to the final third millennium BCE move gradually from a crude eclecticism to an often refined impressionism; in the ensuing half-millennium, the style is dissolved in the Cycladic culture into living patterns (in an interaction with Minoan styles which is mentioned but not investigated). The Mycenaean court culture of the mid-second millennium – the first culture in the area carried by a Greek language – presents two very different faces. One, expressed on various non-ceramic artefacts, apparently unfolds a "native" style Mycenaean style, and reaches a high level of geometric regularity beyond the immediately visible; the other, found on pottery, borrows the living Cycladic and Minoan style but gradually becomes more rigid while remaining "impressionistic". During the Submycenaean decline of courtly

wealth and the ensuing Protogeometric period, this gives way to often rather crude abstract decorations, which occasionally suggest the survival of facets of third-millennium ways; in the Geometric period (ninth–eighth century), the abstract decorations give birth to impressive combinations of figurative and abstract but always impressionistic painting.

This "Geometric" style, however, had disappeared long before the rise of Greek theoretical geometry; and whatever "geometry" presented by the visual arts of the epoch when Oenopides and his contemporaries made the ruler and compass canonical had long left the line and the circle behind. The delight which many eyes take in regularity at the level of immediate perception does not lead by any necessity to mathematical geometry, whether provided with proofs or not.

I.7, "Broad Lines; A Forgotten Geometrical Ambiguity, deals with what may look like a trifle – definitely an "odd corner". So odd, however, that lack of understanding of a conceptual structure that differs from what we are accustomed to has led generations of Plato scholars into the wilderness, and resulted in serious mis-appreciation of Babylonian mathematics. Apart from that, a small anecdote may elucidate the import of the observation that many pre-Modern genuinely practical surveying traditions have measured land in length units, presupposing a standard width: When I sent the article to the journal, its editor told me a few days later that he had had to make a lecture in a middle school (grade 7, 8 or 9, I suppose). He choose as his topic my article, and – the students were hilarious to discover that mathematics could be thought in a way that differed from that of the teacher. That, of course, fits the concluding paragraphs of the article.

Even *I.8, "Concerning the Position of 'Heron's formula' "*, concerns an odd corner, and an apparently minor question – the relation between Heron's two presentations of the formula for the determination of a triangular area directly from the sides: the one in the *Metrica,* the other in the *Dioptra.* However, the analysis leads to insight in Heron's way of producing the *Metrica* and in the relation between more genuinely practical geometry and Heron's work.

I.9, Heron, Ps-Heron, and Near Eastern Practical Geometry, was first presented in 1996 in "Arbeitskreis Antike Naturwissenschaft und ihre Rezeption", Universität Trier, and to the "Conference on Ancient Mathematics" held in Delphi. It deals again with the relation between Heron's *Metrica* and the pseudo-Heronic collections known as *Geometrica,* but in a different and wider perspective that also includes the practical geometries of the Islamic world and Fibonacci's *Pratica geometrie* (and, peripherally, Old Babylonian scribal geometry and Columella's *De re rustica*). Central to the approach is the investigation of the different parameters that can be used in the establishment of links and contrasts.

I.10, "Which Kind of Mathematics Was Known and Referred to by Those Who Wanted to Integrate Mathematics in «Wisdom»" was originally presented in 2007 as a contribution to the meeting "Science and Philosophy in Antiquity" in Budapest. It shifts the interest to another facet of ancient Greek mathematical culture: those philosophical and philosophizing currents that regarded mathematics as a road to higher wisdom – Plato, and those badly delimited currents which I characterize collectedly as "quasi-gnostic" – Platonists, Neoplatonists, Neopythagoreans. Initially it argues that nothing forces us to believe that the kind of mathematics which Plato's philosopher-kings are supposed to know in the *Republic* is the kind of mathematics we known from Euclid and Archimedes, and that available evidence speaks against this identification. After that, it looks at what was available for such quasi-gnostics as did not understand Euclid (disregarding "Liberal-Arts mathematics" of Nicomachean type, which cannot be regarded as wholly external to the gnosticizing tradition).

Beyond notions familiar from Liberal-Arts mathematics, what we find are sometimes traces, sometimes clear references to mathematical knowledge belonging to one or the other kind of practical mathematics or their "recreational" outgrowth; borrowed piecemeal, however, out of context and without any coherence.

I.11, "The Rare Traces of Constructional Procedures in 'Practical Geometries'" was presented in 2006 as a contribution to the workshop "Creating Shapes in Civil and Naval Architecture", Max-Planck-Institute for History of Science, Berlin – once again with permission, since it deals with neither civil nor naval architecture. It starts from the observation that what normally goes under the name "practical geometry" in the history of pre-Modern mathematics is mostly a scribal or administrative concern. It takes shapes and measures for granted, as something that has already been created or procured – seemingly by others, advice is never given about measuring procedures.[1] On the basis of such already given measurements, they tell how to find areas, volumes – and even measures such as the height of towers that are not directly accessible. However, the workshop question about the *creation of shapes* allowed me to search for the exceptional traces of genuine constructions.

One of these traces is a quaint formulation of the rule for finding the perimeter of a circle from its diameter. Old Babylonian as well as Greek texts (the *Geometrica,* but also Heron when reporting what is commonly done) take *the triple* (the Greek texts regularly with a post-Archimedean supplementary seventh) instead of applying the multiplication they generally use. The explanation turns out to be that they refer to a simple

[1] Fibonacci's *Pratica geometrie* is an exception, but indeed more than a normal "practical geometry". The section about "the dimensions of fields that lay in mountains" [ed. Boncompagni 1862: 107–110] thus gives advice about how to be sure the measurements are made correctly.

geometric *construction* of the perimeter – a construction which is described in a late 15th-century Gothic builder's manual and in an Old Icelandic manuscript from the same epoch; this construction – slightly modified after Archimedes's time – must hence have survived, probably among master builders and architects, from 1800 BCE to 1500 CE, without leaving other traces in the written record than this terminological curiosity.

Another trace is the side-and-diagonal-number algorithm for approximation of the ratio between the diagonal and the side of a square (also discussed in article I.10), which is described in several Platonizing sources from classical Antiquity and is likely to have been known in the Old Babylonian epoch. It is probably derived from a way to construct a regular octagon that can be seen to have been in use at least during classical antiquity. Whether there is also here continuity with Old Babylonian practice is a guess – multiple discovery seems quite possible.

The concluding remarks point to a final instance of weak interaction between scribal and surveying geometry that is reflected in three sources from 13th-14th-century France and Provence.

I.12, "About the Italian Background to Rechenmeister *Mathematics"*, was originally presented in German in 2008 to the colloquium "Die Rechenmeister in der Renaissance und der frühen Neuzeit – Stand der Forschung und Perspektiven", Munich. The other participants in the colloquium were all engaged in the German *Rechenmeister* ("calculation master") tradition that arose on the basis of Italian abbacus school mathematics, and my purpose was to provide them with historical background. The article therefore gives an extensive overview of the Italian development, showing first that it did *not* have Fibonacci's *Liber abbaci* and *Pratica geometrie* as its starting point. As far as mathematical substance is concerned, the article concentrates on the character and development of abbacus *algebra.* Many of the particular points made briefly here are elaborated in later articles (I.14, I.15, II.11, II.12, II.13 and II.14).

I.13, "The Unknown Heritage'…", deals with what Euler called a question of a quite particular nature, which therefore deserves attention – but has not received any by historians of mathematics. Euler turns out to be right, however, not only from his own mathematical point of view but also from that of the historian.

The name "unknown heritage" refers to a specific, strongly overdetermined problem, which in a simple form involving only integers circulates widely in late medieval and Renaissance Romance and Byzantine areas as a recreational problem. The first occurrence, however, is in Fibonacci's *Liber abbaci,* which also offers much more sophisticated versions, one of them correctly solved but accompanied by equally correct rules for the solution that do not correspond to Fibonacci's own calculation.

Two Arabic authors present problems which are clearly derived from the simple version of the "Christian" problem but changed into a normal "nested-box" problem that

can be solved step by step; none the less they are solved as the original version, leaving no doubt about the inspiration.

Close analysis of the whole range of occurrences suggests that the simple version was invented in Greek late Antiquity or in Byzantium. As regards the sophisticated version, the origin is more enigmatic. Because of an overlooked Castilian instance of the simple version, the original version of the article suggested an origin in Provence, though with great hesitation – it is indeed impossible to find neither a Provençal mathematician nor any trace of a Provençal mathematical environment in the 12th century possessing the requested abilities. An addendum, based on other work in which I have been engaged in the meantime, suggests instead that this is one of several instances of advanced arithmetic produced in 12th-century al-Andalus which (much as the works of ibn Rušd, but even more radically) failed to be transmitted to the Arabic world but did reach Latin mathematics though in this case only anonymously.

I.14, "A Diluted al-Karajī in Abbacus Mathematics", was presented in 2010 as a Contribution to the "Tenth Maghrebian Colloquium on the History of Arab Mathematics" in Tunis. It confronts some facets of 14th-century abbacus algebras with corresponding facets of al-Khwārizmī, Abū Kāmil and al-Karajī. (*Fakhrī* as well as *Badi'*) and comes to the conclusion that the deviations of abbacus algebra from what we find in al-Khwārizmī and Abū Kāmil point to al-Karajī. On the other hand, al-Karajī's more advanced insights and methods have left no traces in the abbacus treatises. What has influenced these thus can hardly be al-Karajī's works but only such lower-level Arabic works that had taken over some of al-Karajī's simpler results: a *strongly watered-down* al-Karajī, not the al-Karajī that had inspired al-Samawʾal.

I.15, "'Proportions' in and around the Italian Abbacus Tradition" was presented in 2008 as a contribution to the meeting "Proportions: Arts – Architecture – Musique – Mathematiques – Sciences", at the Centre d'Études Supérieures de la Renaissance, Tours. The paper was published in *Physis,* as it proved too long for the proceedings. In these, instead, appeared an expanded version of section 2 [Høyrup 2011].

The "proportions" of the title are to be understood in agreement with the usage of the time; that is, they encompass what we would call "ratio" (understood as a relation between two numbers, not as a fraction) as well as our "proportion", the equality of two ratios. The rule of three (dealt with in article I.5) is only touched at peripherally.

The article is a quite detailed overview, but two results of general significance emerge. Firstly, Chapter 15 Part 1 of the *Liber abbaci* turns out to contain as its main constituent a systematic treatise on the classical Greek means, which however are not mentioned, even though Fibonacci can be supposed to have known them from Boethius. For each of these, except the trivial arithmetical mean, it is shown how any of the three numbers involved can be found if the two others are known; this treatise is shown by stylistic features to have been borrowed wholesale by Fibonacci. At the time of writing the article

I did not connect it to the sophisticated version of the "unknown heritage", nor obviously to the *Liber mahameleth,* which I had not yet worked through (none of the editions had yet appeared). Now I discover that the three are related, being indeed the "instances of advanced arithmetic produced in 12th-century al-Andalus" which I referred to above.

Secondly, analysis not only of what was written by authors with scholarly ambitions – Fibonacci, Antonio de' Mazzinghi, Benedetto da Firenze, the contemporary compiler of Ms. Palatino 573, and Pacioli[2] – reveals that even the most elementary proportion theory was an intruder in the abbacus tradition proper. Within this tradition, proportionality was dealt with solely by means of the rule of three. Even the scholarly ambitious writers do not apply proportion *techniques* beyond the product rule (that is, if $\frac{a}{b}$: $\frac{c}{d}$, then $ad = bc$), except in Fibonacci's borrowings in *Liber abbaci* Chapter 15. Apart from the product rule, they sometimes use the proportion language in the *statement of problems* – for instance, to find numbers "in the same proportion as 3 and 4" as an alternative to "one being the same part of the other as 3 is of 4", and "to find numbers in continued proportion" which fulfil other conditions. From the perspective of university mathematics of the time (whether engaged in spherical trigonometry or just teaching the Boethian terminology for ratios) this will have seemed primitive. From our point of view it can be seen to prepare what became the norm in later mathematics.

I.16, "Archimedes – Knowledge and Lore" was written in 2016 when I was asked to write an article on Archimedes in the Renaissance for an encyclopedia based on existing secondary literature. Since that literature did not exist to the full, I had to produce what was lacking – and since the Renaissance was familiar with the Archimedes figure almost exclusively through Latin authors until 1500, I had to begin with what Latin authors knew and told about the hero.

The outcome of my search for references to Archimedes in every piece of Latin literature I could lay my hand on from Antiquity to the Central Middle Ages surprised me. I was prepared to find little, but finding only two suggestions that Archimedes was somebody who made mathematical *theory* was unexpected. Even more unexpected was that both of these suggestions were made by Berber authors (Apuleius and Augustine). It amply confirmed Cicero's statement that "we Romans" had reduced mathematical theory to what is useful.

Italo-Latin authors instead knew Archimedes for his war machines and death by distraction while drawing some kind of diagrams; for other mechanical feats; for his model of the planets; for his exposure of the goldsmith's fraud in the crown story; and as an astrologer. Even this was almost forgotten in the Early and Central Middle Ages. Of more than ten million lines in the part of the *Patrologia latina* that covers the years 550 to 1200,

[2] These are the only authors considered in [Bartolozzi & Franci 1990].

only four refer to Archimedes. The Latin scholarly culture of the time, though totally dependent on Latin Antiquity, was evidently quite selective.

The 12th-century translations brought some knowledge of Archimedes the mathematician, and scholastic writers of the late 13th and the 14th century were aware of the identity of the "Archimedes" known from the Latin authors and "Archimenides" the geometer known from the Arabic. That did not affect 14th-century Humanism, however; Petrarca seems to be the only 14th-century Humanist of standing who had some interest in Archimedes – but only in the Latin Archimedes. This Archimedes he praises in wholly unplatonical way for making the heavens visible to the eye and not only to the intellect, while censoring him (against contrary to all Platonisms) for that immoderate absorbtion in the intelligible world that led to his death. Clear evidence, we may say, that the first century of Humanist adoration of Plato was pure ideology not based on any understanding.

With Ficino and later 15th-century Neoplatonism that was evidently to change. But Neoplatonism was not what caused the initial discovery of the mathematician among Humanists. That started, on one hand, where Humanism, art and military engineering met. On the other, at the interface between Humanism and University mathematics. In the former group, Archimedes was primarily a higher artisan, his mathematics was technical; in the latter we find beginning interest in his mathematical *texts,* at first slowly, after 1500 at accelerating pace. The 16th century is also the time where mathematicians like Maurolico and Commandino start working actively in Archimedean style.

This Archimedean turn of Humanism (evidently touching only a very small minority of Humanists) took place in Italy. In northern Europe, Humanism hardly existed before 1500. Northern Humanism, mostly grown in the university garden, at best understood Euclid; Archimedes arrived late. *Archimedism,* the adulation of Archimedes the mathematician, is first expressed by Ramus, who was not at the level where he could understand the hero's works. The first northern mathematician to *use* Archimedes was probably Viète, who used and cited but did not praise. Soon after 1600 (not covered by the article), as mathematics took new ways, this split between use and praise continued, though for a different reason.

Part II, "Oblique Glances and Birds-Eye Views" begins with *II.1, "Existence, Substantiality, and Counterfactuality ...".* This article grew out of a solicitation to speak about Greek Antiquity at a meeting on "Existence in mathematics", held at University of Roskilde, Denmark, in 2000. Since the organizer was head of my own working group, it turned out to be impossible to decline politely. Even though the section on Aristotle enters a closed territory in which historians of philosophy have been active for long and neither I nor historians of ancient mathematics ever, I believe my particular starting point has allowed me to draw some new and worthwhile conclusions.

This starting point was the observation that three of Euclid's postulates in *Elements* I simply are not true according to the standard cosmologies of the epoch. In Aristotle's

finite cosmos, a line cannot be produced as much as wanted – and not at all if it has already reached the outermost sphere; a circle cannot be drawn with a diameter larger than that of the cosmos; and lines in the same plane need not fulfil the parallel postulate. More precisely, my starting point was amazement that nobody seems to have discussed this difficulty, neither modern historians of mathematics nor ancient writers discussing *or attacking* the fundaments of geometry. Closer scrutiny of this question presupposed understanding of what status Aristotle ascribed to mathematical object like numbers, lines or ratios. Do they *exist,* and if so, in which of the many senses of that word? Are they forms (no!)? Are they properties – or are they, in the likeness of forms, *properties aimed at?* These questions, and what they presuppose from Aristotle's general ontology, is what occupies the first section of the article.

The last section turns to the question whether the ancients considered mathematics as a whole, or its single constituent branches, as something "existent". The inherent answer to a never-asked question appears to depend on philosophical viewpoint. In a Platonic or post-Platonic perspective, such existence (as well as the counterfactual postulates) seems to be unproblematic. In certain Aristotelian works, the answer appears to depend on which discipline raises the question, in others the answer would seem to be indubitable negative. In all cases, however, nothing suggests that a *different mathematics* might exist, and in that sense we may claim that mathematics as a whole *existed.*

The formalist view of mathematicals as existing if only the axiomatic network that defines their relations is without contradiction seems to be wholly inadequate when ancient mathematics is discussed, and is therefore disregarded. Equally disregarded is H. G. Zeuthen's view that Euclid's constructions were meant as existence proofs – it takes for granted that we know what would have been meant by "existence"; the article shows that this latter question allows no simple answer.

II.2, "Conceptual Divergence – Canons and Taboos – and Critique" was first presented in 2002 to the "Sixth International Conference on Ancient Mathematics" in Delphi; yet apart from using as examples discussions of ancient Egyptian, Babylonian and Greek mathematics it was not determined by the conference theme (which was anyhow too open to determine much). In general it grew out of my uneasiness with explanations of ancient (or otherwise unfamiliar) ways of thinking or expressing thought through postulated cognitive inability or postulated incompatible modes of thought.

At first, an easier parallel is examined: Aristotle's view of the nature of time and place/space. These have regularly been ascribed to the inability of pre-Newtonian thought to grasp time and space abstractly: the former as a condition for motion not needing in itself any moving body to exist, the latter as an empty receptacle prior to any contents. Who takes the trouble to read the sources will see that Aristotle knows these concepts very well, speaking of them as "what most people think"; but he argues that these

metaphysical abstractions make no sense, in a way that is not too different from what Einstein did in 1905 in the beginning of “Zur Elektrodynamik bewegter Körper”.

This leads to a request that a mode of thought (mathematical or otherwise) should be understood though analysis of how its concepts are *used as tools,* that is, through (in case, mathematical) practice; as illustrations I take (1) the apparently negative but possibly merely subtractive numbers of late medieval abbacus treatises, and (2) Diophantos’s apparently general fractions. I argue, on the other hand, that there is no simple one-to-one correspondence between practices with their appurtenant material tools and objects, and the conceptual structures which they engender – neither when mathematics is considered nor in general. So, conceptual divergence is a phenomenon that must be taken in consideration; but it is no straightforward matter to characterize it precisely; moreover, a possibility that has to be taken into account *as a possibility* need not explain all differences of mathematical idiom and notation – *vide* Aristotle on space.

The following section explores a famous case, namely the ancient Egyptian unit fractions. The system seems clumsy to us, and none the less the Egyptian calculators stuck to it for two millennia. Was this, as maintained by Alan Gardiner and others, because the Egyptians were only able to conceptualize, for instance, “a seventh” as number 7 in a row of seven equal parts of something, of which of course only one can be *the* seventh? Or was it just, as maintained by Eric Peet, an instance of conservative notation for something which the Egyptians understood as we do? From slips first pointed out by Vogel in 1929 (noticed already by Peet, who however drew no consequences; and overlooked by all workers since then) it is argued that no cognitive shortcoming is involved but instead a conscious stylistic choice, a *canon,* called forth by the historical development of the fraction system in the context of the new scribe school of the Middle Kingdom.

Old Babylonian mathematics presents us with other instances of canon development, but also with a different phenomenon: *critique,* investigation (in Kant’s words) of “possibility and limits”. Normally, Babylonian mathematical writers would mention addition before subtraction, just as we do. But there is an exception: Sometimes a magnitude is subtracted before it is added. This has been interpreted as a result of primitive thinking, unfit for abstraction. Closer attention to when and where it happens refutes this explanation: early texts are perfectly “abstract”, using the phrase “add and subtract”,[3] and then state the outcome of the two operations in this order. In some later text groups (not all), the precedence is given to subtraction – but only in cases where it is clearly *the same* entity that is first removed, then added. It appears that some teacher at some moment, whether due to his own second thought or to critical questions, has discovered that *strictly speaking* it is impossible to add something which is not available, and has started “speaking strictly”.

[3] Since this is of no importance here, I allow myself to disregard the multiplicity of distinct additive and subtractive operations. These are presented in article II.7.

Critique was also involved when Old Babylonian teachers came to see "broad lines" as problematic and first introduced a terminological distinction allowing them to add measuring numbers instead of the concrete entities themselves, and later invented various tricks that could transform a line meaningfully into a area. So, what the texts do not say or do not do must be explained, not in terms of what they *could not think* but as evidence of what they *found it professionally unfitting or incoherent to say* or (as suggested by slips) at least to *write down.*

None the less, taboos when respected have consequences; outlawed concepts cannot contribute to the creation of new knowledge. Cognitive proscription at one moment may create conceptual divergence in the next phase.

II.3, "Tertium non datur, or, On Reasoning Styles in Early Mathematics", was originally a contribution to the symposium "Mathematics as Rational Activity", Roskilde University, held in 2002. As other contributions to the symposium, this one aims at illustrating that reasoning – even mathematical reasoning – is not one thing. The provocative title refers to the conclusion that as soon as we go beyond mere routine there is no such thing as "purely empirical" mathematics, mathematics not built on some kind of reasoning; if we do not find traces of this reasoning, the reason is *either* our own failing understanding of the sources, *or* the insufficiency of surviving sources as reflections of educational practice and its basis – *tertium non datur. Teaching* of mathematical techniques may of course have been by pure drill.

Before starting the argument, however, its necessity is argued from two representative scapegoats. One is Morris Kline, who sees no organized reasoned mathematics before the Greeks; the other George Ghevarghese Joseph, who denies the Greeks any particular honour but does so with arguments that at closer view seem to vindicate Kline.

The positive part of the argument starts by looking at Old Babylonian so-called "algebra", discussed also in article I.3, and shows that its geometrical cut-and-paste procedures are reasoned but "naive": as in the case of modern equation transformations, we "see" immediately that the operations are correct and lead to the requested result, but there is no explicit discussion of *under which conditions.* Instead there are a few texts that explain *why*, not by deductive proof, however, but by building up a conceptual network. Genuine critique though not totally absent is rare, touching only two specific points (as told in connection with the above presentation of article II.2).

This is confronted with *Elements* II.6, which is shown to be a "critical" transformation of an Old Babylonian standard procedure. Euclid does not cut and move around, instead he constructs and proves equalities of areas to the best standards of Greek axiomatic geometry.

Axiomatic Greek geometry was not born full-fledged, of course, and the following section presents a sketch of its development. First came definitions, already to be predicted (by us, obviously, and thus *a posteriori*) from such things as Hesiod's conceptual

clarification of “strife” by dichotomy in *Works and Days.* Mathematical definitions were known to Aristotle, repeatedly in the very shape we find in the *Elements.* But the clarification provided by these resulted in the discovery of the need for *postulates,* only one of which was known to Aristotle (actually he predicts the need of something like the parallel postulate, but in a way that shows that it did not yet exist). Critique was thus an aim for Greek geometrical demonstration already in the fourth century BCE, but is was still in process and only reached its (temporarily) final state in the third.

“Greek geometry” as a term normally refers to the critically demonstrating kind we known from Euclid, Archimedes, etc. But Greek geometry was more than that. Much of what was done in such mathematical practices as surveying and architecture we only know from its results; accordingly, it cannot contribute to the present discussion. But at least we have the pseudo-Heronian collections known as *Geometrica.* Much of the material here is presented solely in the shape of rules not telling their origin, but sometimes there are arguments, which invariably are then “naive” (occasionally also naive in the everyday sense). “Naive” in character are also the mathematical arguments that are occasionally found in “gnosticizing” writings (cf. the presentation of article I.10 above). Critique and axiomatization, though setting the norms for much later mathematics, was the business of a small minority even among those who were occupying themselves with mathematics during classical Antiquity.

So, is all mathematics reasoned? The closing section argues, in particular from the rule of three and from the double false position, that this conclusion is precipitate. Routine practice, if only so much of a routine that the practitioner is not in risk of forgetting, may well, and was for long, trained with success without the difficult reasons behind it being told. Yet who would believe that the rule for the double false position was the outcome of pure guessing or trial and error? Those who invented it must have based themselves on an argument. In this sense, thus, *tertium non datur.*

II.4, “Embedding: multi-purpose device for understanding mathematics ... ” was first presented in 2000 to IX Congreso de la Asociación Española de Semiotica. It was accepted for the proceedings of that meeting, but these were never produced; the version which I finally published in 2016 (the present version) was somewhat updated, mainly the section “Embedding and spatiality”.

Being invited as an outsider to the congres I took the opportunity to explore an idea located somewhere between philosophy and history of mathematics, linguistics, and psychology – namely that the linguistic phenomenon of embedding might elucidate certain features of numeral systems (in particular place-value systems) and of algebraic symbolism; and perhaps also have to do with the assumed absence of revolutions in the development of mathematics, old theories and results being supposedly always integrated and thus conserved within new theories.

The sections “Numerals” and “Symbols and other symbols” are able to conclude that a linking to the general notions of embedding and recursivity as known in particular from generative grammar is meaningful. “Embedded theoretical domains?”, on the other hand, concludes that other mechanisms are at stake here. One could be a kind of objective truth in mathematics which does not coincide with the single theory but conditions theories (related to what would explain Eugene Wigner’s “unreasonable effectiveness of mathematics in the natural sciences”); the other being simply an expression of conservative professional ideology, according to which mathematics is supposed to “change in order to conserve”.

The last section, “Embedding and spatiality”, attempts to combine the results obtained concerning number systems and symbolization with neurolinguistics and other linguistic theory. It concludes that the “embedding” of sentence structures, numerals and symbols is a calque of real spatial embedding, going back to the recycling of spatial understanding in syntax; and further that mathematical embedding is a parallel to and not derived from linguistic embedding – in agrement with a point of view which Noam Chomsky had not enunciated explicitly in 2000 but has formulated in the meantime.

II.5, “What is “geometric algebra”, and what has it been in historiography?” came into being in 2016 as a contribution to the session “Histoire de l’historiographie de l’algèbre” of “Séminaire Histoire et Philosophie des Mathématiques”, CNRS and Université Paris Diderot. It confronts one of the main pillars of what Viktor Blåsjö [2014] speaks of as the “modern consensus in the historiography of mathematics”, namely a conviction that the notion of geometric algebra is historiographic nonsense and pure anachronism. Quotations at hand, it argues that there is no such thing as *the* notion of “geometric algebra”, that those who used the words after 1930 (primarily Otto Neugebauer, but also B. L. van der Waerden) did not care about how they had been meant by Zeuthen, Paul Tannery and Thomas Heath, nor those who wrote after 1950 about what Neugebauer had actually said. Moreover, those who attack the idea (Arpád Szábó, Michael Mahoney, Sabetai Unguru) have mostly done so on the foundation of things their targets have never said nor meant; one gets the suspicion they never read the publications they refer to. Those who today suppose it to be proven that the idea of a “geometric algebra” is historiographic nonsense and pure anachronism also have not undertaken the task to find out what has been meant by its various friends and foes.

That, as historical experience tells, is not an unusual way to establish – not a mere consensus but a genuine orthodoxy. The article is an exhortation to think twice.

II.6, “State, ‘Justice’, Scribal Culture and Mathematics in Ancient Mesopotamia” was presented in 2008 at Ghent University as a Sarton lecture – expected, I was informed, to “appeal to a broad audience of non-specialists” though not as broad as the Sarton Chair lecture (included here as article II.15). Accordingly, it provides general background to the following four articles.

The article traces the development of mathematics in its interaction with the Mesopotamian "state" from the very beginning in the later fourth-millennium BCE until the collapse of the Assyrian empire shortly before 600 BCE. In the beginning, as argued, the emergence of statehood in Mesopotamia around the great temples of Uruk was inseparably involved with accounting (thus with mathematics), and writing was created with the sole purpose of creating context for the numbers and measures. As, after a collapse and the emergence of a system of competing city states, a stratum of *scribes* arose, this stratum expressed its professional pride in the creation of the first literary texts as well as "supra-utilitarian" mathematics – mathematics which in shape looks as if it corresponded to what scribes might have to deal with in their professional life but is indeed more complex that what would ever be encountered in scribal practice. This coexistence of managerial and supra-utilitarian mathematics survived until the beginning of "Ur III" around 2100 BCE: in this extremely bureaucratized state, mathematical management reached a high point, but all mathematics that might require independent thought appears to have been eliminated from the education of rank-ad-file scribes – not only supra-utilitarian mathematics but mathematical problems in general. At the same time, mathematically precise management appears to have still legitimized the state as an expression of "justice".

After the collapse of even this state (Mesopotamian empires lived no longer than the Spanish and British world empires in later times), a new culture unfolded in the mature "Old Babylonian" period (c. 1850 to 1600 BCE). Within this new cultural formation (new at least at the elite level of scribes and rulers), supra-utilitarian writing and mathematics became more important than ever, while the last traces of "mathematical justice" legitimizing the state disappeared. The high level of supra-utilitarian mathematics (known as "Babylonian mathematics") did not survive the breakdown of the Old Babylonian social structure around 1600 BCE, and when scribes returned to court in the Assyrian empire in the earlier first millennium BCE, they did so as specialists in omen interpretation (including astrology), exorcists, and the like. Some of them developed a mathematical astronomy (which survived the Assyrian empire, and only reached its highest level some 500 years later); but it was created as a tool for astrology, and its creators never define themselves as anything like "mathematicians" or "astronomers". Mathematical administration was left to less learned professions. Mathematics, once a driving force in the emergence of the state and important as a legitimization of state power for some 1300 years, was reduced to a mere tool in Old Babylonian times, and retained that status for the last 1300 years of the period considered – and beyond that.

II.7, "how to Educate a Kapo", was presented to the conference "Under One Sky: Astronomy & Mathematics in the Ancient Near East", London, in 2001. It tells about a discovery that had shaken me a couple of years before: how scrutiny of something as apparently otherworldly as Babylonian mathematical terminology can reveal an unknown facet of the extremely oppressive world created under the Third Dynasty of Ur (known

simply as "Ur III"). It felt like digging in the garden in order to plant potatoes, and then uncovering a mass grave.

The matter is summarized in the previous article. That article was written years later than the present one, which presents the underlying evidence.

This evidence consist in a classification of the Old Babylonian mathematical vocabulary, distinguishing between terms for mathematical operations and terms used to describe the mathematical structure of problems or belonging with the metalanguage. The classification is coupled, on one hand, to observation of whether the terms belonging to the different groups are written exclusively or almost exclusively with logograms of Sumerian descent – exclusively in syllabic Akkadian – or sometimes in one, sometimes in the other way; and, on the other hand, to a survey of third-millennium (Ur-III as well as pre-Ur-III) terminology.

We know mathematical problems from the pre-Ur-III school, and we have many from the mature Old Babylonian period. From Ur III we only have model documents, no problems. What the Old Babylonian terminology teaches us, is, firstly, that there is some continuity with the pre-Ur-III period, but partly re-Sumerianized, suggesting that the semantics has survived in Akkadian; secondly, that the Old Babylonian teachers were aware of innovating when introducing (actually, re-introducing) problems in the curriculum. According to this evidence, the absence of problems from the immense Ur III corpus is not the result of archaeological accident – the absence of evidence can really be taken as evidence of absence.

No other school which has taught mathematics at the level of place-value arithmetic (at least none I know of) has done so without presenting that kind of stimulating challenge to the intellect of its students which is offered by problems. That the Ur III school did so, even though problems had been proposed in Sumerian scribe schools for half a millennium, can only have one explanation: no stimulation of the creative intellect of rank-and-file students – the future overseer-scribes – could be tolerated, they had to be trained in automatic routines and docility.

The closest historical parallel that comes to my mind is the instruction in *studia humanitatis* of young noblemen during the 14th and 15th centuries in Humanists' schools – cf. [Grafton & Jardine 1986]. Here, the *disputation,* so central in university teaching of the epoch, was eliminated. Future courtiers were not to nourish critical thought of their own, they were to serve their prince with skill, elegance and docility, and nothing more.

II.8, "A Hypothetical History of Old Babylonian Mathematics", originated in 2012 as a contribution to the "International Conference on History and Development of Mathematical Sciences", Maharshi Dayanand University, Rohtak – a context which evidently left my hands very free.

The starting point is the observation (the reader is welcome to see it as a lament) based on two characteristic examples that general histories of mathematics do not

understand that Mesopotamian mathematics had *a history* covering more than 3000 years and cannot be reduced to an undifferentiated concoction of Old Babylonian and Seleucid techniques and results if we wish to learn more than isolated details hanging in free air.

Those who first made Old Babylonian and Seleucid mathematics accessible to us – primarily Neugebauer, François Thureau-Dangin and Abraham Sachs – of course knew better already in the 1930s and 1940s. However, before c. 1975 too little material from the fourth and third millennia was known to allow the integration of this period in a general history, and bits elucidating the 1300 years separating the Old Babylonian from the Seleucid period only turned up even later.

Today, a long-term history *can* be written, at least in the general terms of article II.6; it is recapitulated even more summarily in the section "What was known to those who care …" of the present article. Many details still need to be filled out – and mostly this can only be done hypothetically, in a way that may at least guide further research and the formulation of more adequate questions.

The period which seems most promising for such a task is the Old Babylonian period. Admittedly, we have few sources speaking about mathematics "from the outside", and none which do so with precision; mathematics "from the inside" is mostly represented by texts bought at the antiquity market, thus identifiable as Old Babylonian on the basis of palaeography – the exceptions being a large collection of elementary training texts from Nippur (and a few similar texts from elsewhere); a handful of well excavated tablets from Eshnunna; some more from 19th-century Ur; and a batch of text from Susa, these latter badly excavated but at least known to be from Susa. However, close attention to orthographic and terminological peculiarities allows us to divide the corpus into a number of identifiable groups; further attention to the general historical setting makes it possible to construct at least a hypothetical history. This hypothetical history is presented in the last two-thirds of the article.

II.9, "Written Mathematical Traditions in Ancient Mesopotamia", was presented in 2011 to the conference "Traditions of Written Knowledge in Ancient Egypt and Mesopotamia", Frankfurt am Main. Though written slightly earlier, it is a complement to the previous article, looking in agreement with the conference theme at the whole stretch of ancient Mesopotamian history from the formation of the early state until the Seleucid conquest 3000 years later – analyzing, we may say, the *longue durée* and change within it.

As formulated in the subtitle, the article deals with [our] "knowledge, ignorance, and reasonable guesses". Since many of the guesses concern the question how to interpret the occasional appearance of what looks as traces of traditions that were otherwise oral or at least have not left any surviving continuous written traces, I could not obey the conference theme to the full but has also to consider non-written traditions as well.

As it turns out, the basic complex of numbers and measures that arose in the late fourth millennium BCE survived with adjustments and accretions until Ur III, where is was absorbed into a larger structure governed by the place-value system. This structure is what we may observe in action in the Old Babylonian period. We see how it was taught in the elementary training texts from Nippur, and it is sometimes tacitly, sometimes explicitly presupposed in the sophisticated texts. After the Old Babylonian collapse, it was carried by professional groups with too little prestige to produce surviving texts (probably making their calculations on ephemeral supports), but now in combination with newly created metrologies. These (seed measures, and the like) responded better to the needs of a practical management of agriculture that was no longer responsible toward a centralized state. For these metrologies, the sexagesimal metrological tables were no longer useful, and it appears that the "scientific" Ur III system survived in reduced form only – but at least Assurbanipal could claim in the seventh century BCE to be able to find reciprocals (find them in the table, we must presume) and to perform (place-value) multiplications. In Seleucid and earlier Late Babylonian sophisticated mathematical texts, as well as astronomical procedure texts, we also find faultless sexagesimal place-value arithmetic.

As regards the more sophisticated level, it is much more difficult to point to anything that deserves the name of a "tradition". The pre-Ur-III problem culture, as we see in article II.7, vanished during Ur III – and how much continuity there is from the schools of the 26th century BCE to those of the 23rd is not easy to decide. During the mature Old Babylonian period we may speak of a general creation of a problem culture (re-creation, indeed, but much more developed), and of an aspiration to create canonical formats; but precisely the diversity of the formats we encounter suggests the existence not of a single tradition but rather of a multiplicity of short-lived schools responding to a general climate – short-lived not least because of political changes like the conquest and destruction of Eshnunna, and the fall of southern Iraq to the Sealand.

The breakdown of the Old Babylonian state and the dissolution of its socio-cultural fabric entailed the interruption of all these schools. Omen science, medicine and literature survived in inner emigration within a closed environment of scribal families, but sophisticated mathematics did not. When a small number of sophisticated mathematical problems turn up in the fifth century BCE, and again around 200 BCE, it is obvious from the terminology that there is no continuity within an environment of scholar-scribes. Instead, a new start was made from problem types that had been transmitted by non-scribal practitioners with at most rudimentary familiarity with Sumerian – or, rather, two new starts, since these two episodes are unconnected and do not represent a single tradition.

We may just take note of this discontinuity of traditions – which is what the article does. But we may also observe that metrology and place-value computation served broader social interests; so did medicine and omen science, maybe even what we see as "literature". Knowledge of how to find the sides of a rectangle from the area and the sum of the sides

(to mention but the simplest of all the sophisticated problems) had absolutely no use outside the particular (probably very restricted) scribal environment where such problems were used, and *within* this environment their only role was to be solved and thereby to contribute to status consciousness. Such traditions could therefore be forgotten without loss.

We may compare with the fate of mathematical astronomy. It started in Assyrian times, survived and flourished under the Achaemenids and even more impresively under the Seleucids and Arsacids – the latest evidence postdating the beginnings by some 800 years. In itself this kind of knowledge also seems to be of no interest except for those who practised it. But under the Assyrians mathematical astronomy served as support for omen science, which was of supreme state interest; under later rulers this service may have been less important outside the environment of temple scholars, but until the end omen science remained an identity-providing field for the group of temple scholars as a whole.

II.10, "Mesopotamian mathematics, seen 'from the inside'... and 'from the outside'" was presented in 2013 as a contribution to the workshop "Historiography of Mathematics in the 19th and 20th Centuries", Bergische Universität Wuppertal. Accordingly, it shifts the focus from the history of Mesopotamian mathematics to the history of the historiography of Mesopotamian mathematics.

However, in order to make clear the leap that was made in the mid-19th century the article starts when modern historiography of mathematics can be claimed to have begun – in 1758, with Jean-Étienne Montucla. Neither Montucla nor Abraham Gotthold Kästner or Joseph-Jérôme Lalande could offer more than Enlightenment common sense combined with a critical reading of material from classical Antiquity – and that is what they offered.

No more could be made before the cuneiform script had been deciphered and a relatively large text corpus made available. How that happened is told in the next section, "The beginnings of Assyriology". The initial steps were slow. In 1621 Pietro della Valle had described the Persepolis rock inscriptions, but only around 1800 first Friederich Münter, and immediately afterwards Georg Friederich Grotefend succeeded in identifying one of the scripts as alphabetic and Old Iranian – Grotefend also with greater success than Münter identified some sound values. Münter, on the other hand, showed that the script was the same as was used in Babylon, and supposed it to be of Mesopotamian origin.

The next phase, the one which was to give access to Mesopotamian civilization, was the decipherment of the Akkadian version of the inscription and of other Akkadian texts. The first successful steps were made by Henry Rawlinson, Paul-Émile Botta and Edward Hincks from 1846/47 onward, who also identified the number system and came to a first understanding of the place-value system before 1855.

Over the next 80 years, Assyriologists came to a fair understanding of Mesopotamian mathematics as applied in economic and other practical contexts. However, what they produced did not contribute much to the elucidation of questions which might occupy

historians of mathematics, and what these latter wrote on the topic was not convincingly profounder than what had been written by Montucla, Lalande and Kästner.

The first step toward understanding of properly mathematical texts was taken by the young Ernst Weidner in 1916, followed by a few other Assyriologists (Heinrich Zimmern, Arthur Ungnad, Cyril John Gadd). As a result, a few technical terms were understood before Otto Neugebauer initiated the "golden decade" in 1928.

During this decade, my distinction between the approaches of Assyriologists and of historians of mathematics makes no sense. The two main workers were Neugebauer and Thureau-Dangin. The former was a mathematician and historian of mathematics, who had already acquired some competence in hieroglyphic and cuneiform philology; the latter was beyond doubt the most mathematically competent among Assyriologists. Other participants in the process also had mixed competence. First in friendly interaction, then in not exactly friendly but still polite competition, they brought to light what became known as "Babylonian mathematics". In 1937–38, they had worked through and interpreted the whole corpus of texts to which they had access, and they directed their interest in other directions (Neugebauer toward astronomy).

In 1945, Neugebauer and Sachs published a volume containing texts in U.S. collections to which Neugebauer had had no access in the previous decade; in 1961 a collection of text from Susa appeared; and from 1950 to 1962 a small batch from Eshnunna was published. On the whole, however, Assyriologists supposed that everything to be done had been done, and so did historians of mathematics – with the difference that Assyriologists stopped being interested, while historians of mathematics read Neugebauer (more rarely Thureau-Dangin) and wrote on the topic on that basis (too often not distinguishing Neugebauer's explanations in modern symbolism of why things work from the translation, and almost never looking at the transliterated texts or the hand copies). This situation lasted until the later 1970s.

II.11, "Fibonacci – Protagonist or Witness? Who Taught Christian Europe about Mediterranean Commercial Arithmetic?" was presented in 2010 to the workshop "Borders and Gates or Open Spaces? Knowledge Cultures in the Mediterranean during the 14th and 15th Centuries", Departamento de Filosofia y Lógica, Universidad de Sevilla. It draws on material presented in article I.12 and uses it to elucidate the specific problem of transmission.

The starting point is the widespread assumption that Fibonacci was the channel through which Arabic practical arithmetic reached Europe, and the reasons that this story has to be given up. In order to collect material for an alternative account, the next section looks at the development of abbacus *algebra,* comparing it with the algebra of Fibonacci's *Liber abbaci,* various Arabic algebraic writings (those translated into Latin and others which were not translated) and drawing also on other material from the *Liber abbaci.* It appears that the first beginning of abbacus algebra was inspired by contacts to the Romance-

speaking Ibero-Provençal area, and that some ideas from the Maghreb area seeped in during the 14th century. After that, there are no identifiable traces of Arabic influence.

Algebra, of course, is not the whole of commercial arithmetic – actually it is no proper component at all of this field, however much it is part of the *Liber abbaci* as well as many abbacus books. The following sections therefore look at other areas where identifiable specific material can be found: the use of ascending continued fractions, the rule of three, and the characteristic set phrases used in abbacus writings. What looks like specific Byzantine influence is discussed briefly; influence from Hebrew writings (for example ibn Esra), sometimes suggested, appears to be absent.

The final conclusion sums up that many single elements illuminating the transmission process can be identified; that they do not combine to form an exhaustive and coherent picture; and that too much of such source material as might have allowed us to create a convincing coherent picture has gone lost.

II.12, "What did the abbacus teachers aim at when they (sometimes) ended up doing mathematics?", originated in 2007 as a contribution to the conference "Perspectives on Mathematical Practices, Vrije Universiteit Brussel. The aim of the article is to portray abbacus mathematics as a *particular* mathematical practice and to identify its characteristic norm system.

The starting point is a phenomenon which seems to go against the idea of rule by a norm system: fraud. The false rules for the solution of third- and fourth-degree algebraic cases (discussed also in article I.12) are compared to two famous cases of economic fraud: Ivar Kreuger and Enron. Next, Dardi's sometimes valid rules and Giovanni di Davizzo's use of roots as stand-ins for negative powers are analyzed, the latter as an example of misled intuition. An example of valid intuition is found in Dardi's proof of the sign rule "less times less makes more"; sound use of polynomial algebra in the transformation of higher-degree equations in a Florentine manuscript from the outgoing 14th century is analyzed.

All of this had to do with abbacus *algebra.* Algebra, though a prestigious field, was no central constituent of abbacus mathematics. Moreover, many abbacus writers (and *a fortiori* teachers) did not understand the field. So, in order to read out of the abbacus writings the practice and norm set which they refer to, more basic matters have to be looked at: the rule of three and its recurrent service as a postulate or an axiom; control of exactness and coherence of results; and approximation. As it turns out, the following norms are expressed, directly or indirectly:

- abbacus mathematics should, to the extent authors and users could do it respectively follow it, be argued;
- it should be consistent;
- and it should be exact, unless some real-world application asked for approximation.

This agrees badly with the invention of false rules. That does not mean, however, that the norms were not real. Instead it reveals the flip side of norms: fraud can only be efficient when others believe the norms to have been followed. If Enron had been known to be run by bright gangsters, it would never have succeeded; when given a facade by prestigious accountant firms like PriceWaterhouseCooper and Arthur Anderson guaranteeing its honesty, it did.

Giovanni's pseudo-roots tell us more. They suggest the existence of yet another norm, not directly for abbacus mathematics but for what abbacus masters should strive after: extension of the domain. That norm provides a further explanation for the invention of the false rules: in competitions, those who brandished such rules, mastering problems their competitors did not know how to master, gained more prestige than those who merely knew to answer double-false questions with great speed and accuracy. But this norm did more: it provided a driving force for the authentic expansion of algebraic knowledge that took place between 1300 and 1500.

The abbacus norms look similar to the norms of present-day mathematics, and to those obeyed by, for instance, Viéte, Fermat and Descartes. At closer inspection they do not coincide, however. Precision in numerical calculation was not the same as precision in the realm of ruler and compass, nor was the kind of argument provided by abbacus authors the same as that of authors with Euclidean ideals and experience. But the partial agreement between the two norm systems sufficed to allow the two practices to be seen as related.[4] That permitted Viéte and Descartes to reshape algebra (as they say they do), dispensing them with the impossible task to reinvent everything.

II.13, "Hesitating Progress – the Slow Development toward Algebraic Symbolization in Abbacus- and Related Manuscripts, c. 1300 to c. 1550", was originally written in 2009 as a contribution to the conference "Philosophical Aspects of Symbolic Reasoning in Early Modern Science and Mathematics", Ghent. It follows the development of abbacus algebra (with precursors) from the beginning to the early 17th century, grossly the same period as that covered in the previous article. The perspective, however, is not that of the sociology of norms but that of the development of symbolism (broadly conceived, but with distinction between mere abbreviation and notations allowing formal operation).

Three types of mathematical symbols were created in the Maghreb during the 12th century: the fraction line, notations for complex fractions (most important of which the writing of continued ascending fractions), and a symbolism for polynomial algebra. They were probably created in this order. The *Liber mahameleth,* indeed, knows only the fraction

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[4] We may compare with the rejection of Caramuel's Baroque mathematics by the core of the new science of the 17th century, as discussed in article II.15); the over-arching ideals and norms of Caramuel were too different.

line, the *Liber abbaci* also the complex fractions. None of them, however, use the notation for powers and their composition.

This notation is also absent from the Latin translations of al-Khwārizmī's algebra,[5] and even from the earliest phase of abbacus algebra; what we find traces of as early as 1328 are formal operations on fully written words – we may speak of a syntax of symbolism without symbolic vocabulary.[6]

Well before that, however – probably before 1230 – Jordanus de Nemore had developed a letter symbolism, first in his algorisms, where *a, b,* … stand for unspecified digits. He then used it in his *Elements of Arithmetic* for unspecified numbers, and again in *De numeris datis.* Since this latter work can be seen as an algebra,[7] it has often been understood as an algebraic symbolism, and then been criticized because of its clumsiness. This is mistaken. If Jordanus's notation is a forerunner of anything, it is of the formal description of an algorithm: each letter corresponds to a specific step in the procedure.

The early instances of symbolic syntax all concern fractions with algebraic binomials in the denominator. For a long time, however (and beginning no later than 1334), this interpretation of the fraction line as a symbol for division competes with a purely linguistic understanding as an indication of the ordinal form of a numeral, "the $\frac{1}{3}$" standing not only for the fraction but also, for example, for the third of three men. The denominator was thus understood as a denomination, and $\frac{10}{cose}$ stood for "10 *cose*". This could only obstruct the further development of symbolic calculations, and I do not remember a single manuscript where the two uses of the fraction line are both present.

Around the mid-14th century, schemes for the multiplication of binomials (involving radicals or algebraic unknowns) turn up in some manuscripts, and also the use of abbreviations for algebraic unknowns, root, addition and subtraction; but apart from being sometimes used in the schemes, they are always meant as mere abbreviations; in the 15th century they were sometimes but rarely used in formal calculations. With one exception, *consistent* or almost consistent use at least within the single text has to wait until the very end of the century. By then, however, awareness of the existence of a number of *systems* has developed (in Pacioli's *Summa,* but not only there).

Already during the second half of the 15th century, abbacus algebra was transmitted to southern German area, soon to be known as *Coss.* In the Coss, perhaps because the

[5] A redaction of al-Khwārizmī's algebra from around 1300 presents a set of abbreviations which it does not used and which therefore cannot have been invented by the editor of the redaction. It is different from the Maghreb notation but *may* have been indirectly inspired by it.

[6] "As everyone knows, words, too, are symbols", as pointed out by André Weil [1978: 92]. Symbolic *syntax* is therefore what matters, the use of abbreviations only becomes important when expressions become complex and therefore opaque if written in full words.

[7] It is rather a substitute for or justification of algebra – in Jordanus's term, a *demonstratio*.

Italian terms lost their everyday connotations by being transferred as purely technical terms, systematic use became the rule; apart from that, no great change occurred in the use of symbols. Mid-16th-century French algebra, exemplified by Jacques Peletier, was mainly borrowed from the Coss, and shared its character on this as on other accounts.

Who believes in automatic progress toward *us* may be disappointed. Why didn't the abbacus masters, some of whom were very good algebraists, grasp the potentialities of symbolic algebra, and create it? The answer comes from questioning the term "potentialities". Potentialities are not abstract, they have respect to something. Descartes', Euler's or Abel's algebras were not within the horizon of the abbacus masters. The innovations which the most mathematically gifted among them created had, and could only have, respect to the practice they knew.

II.14, "Embedding: Another Case of Stumbling Progress", was presented in 2015 as a contribution to the workshop "Mathematics in the Renaissance – Language, Methods, and Practices", ETSEIB, Barcelona. It was written just after I prepared the revised version of article II.4, which inspired me to have another look at the development of symbolism.

The starting point is Nesselmann's oft-cited, oft-criticized and oft-misunderstood distinction between rhetorical, syncopated and symbolic algebra. The central point in this scheme, it is argued, is Nesselmann's criterion for an algebra being symbolic, namely that operations can be performed at the level of symbols, with no recourse to spoken or written language except in the function of a metalanguage. Two examples illustrate this – one from Diophantos (one of the extremely rare examples of symbolic operation in his *Arithmetica*), one from a 14th-century abbacus compilation. The latter is an instance of purely formal operation with fractions having algebraic expressions as denominators.

That leads to identification of *embedding* (the "parenthesis function") as a decisive constituent of an advanced symbolic algebra – namely that which allows the manipulation of a complex expression as if it were simple. From the mid-14th century onward, abbacus algebra knows two types of parentheses: those defined by a fraction line between complex algebraic expressions, and the "joined root" (or "universal root") keeping together an succeeding polynomial as radicand. Mostly this polynomial is meant to be a binomial, but occasionally it is not; the parenthesis defined by the joined root is thus ambiguous.

The (very slowly) increasing willingness of abbacus writers to think in terms of embedding is followed through the naming of algebraic powers. Initially, following the manner of Diophantos and Arabic algebra, the powers are seen as *entities*, and their composition is multiplicative. "Cube of cube" thus designates the sixth power (etc.). Emulating this, even names for higher *roots* are given "multiplicatively", "cube root of cube root" being used as a name for the sixth root. This latter habit however, is seen rather early to be problematic, since root-taking is indubitably (in our terms) a function: the cube root of 512 is 8, and the cube root of 8 is two – and 2 is the 9th, not the 6th root of 512. Some authors in consequence start naming even *some* powers according to the embedding

principle, seeing *censo* (the second power) as a function that can be taken of another power (but not, for example, of a binomial); other powers they still name in the traditional multiplicative way. Only in the outgoing 15th century will certain authors (for instance Pacioli) name all roots and all powers as composed by embedding. German Cossic algebra does the same, while Viète sticks with multiplicative composition.

Embedding thus turns out to be a difficult idea to grasp, and is indeed difficult to express unambiguously without a graphic indication of the parenthesis.[8] Generalized use beyond what was done by fraction lines and joined roots may also have been rather superfluous within the usual sphere of abbacus algebra.[9]

Viète not only did not use embedding for the naming of powers; that may after all be a result of his familiarity with Xylander's translation of Diophantos. He also did not possess a general parenthesis function, even though his 17th-century translators and editors sometimes read his texts as if he had. Descartes had it – not enclosed in a pair of brackets but kept together by a brace. This, however, is no important difference. Nor is it very important that Descartes only used his parenthesis sparingly. He used it often enough for his readers to discover, so even Wallis and Newton could and would use it – the former defining it by means of a Cartesian brace (round brackets he uses for explanatory parentheses even within formulae), the latter by a vinculum (a stroke above). Both need it only occasionally and therefore make use of it sparingly, but the important step had been taken.

Once more we may ask why development was so slow, and once more we may observe that nobody engaged in abbacus algebra or Coss – practices had no social role outside the environment of those who lived from teaching mathematics – had much use for a generalized parenthesis. Chuquet and Bombelli provide the demonstration: they created something that *could* have served – but they only used it for composite radicands. Tools are not created for an unknown future practice, and for Stiefel and Cardano and all their predecessors, the 17th-century culture of problems derived from Archimedes, Apollonios and Pappos was an unknown and unknowable future.

II.15, "Baroque Mind-set and New Science: a Dialectic of Seventeenth-Century High Culture" was presented as my Sarton Chair Lecture in 2008 and accordingly, as said above, expected to "appeal to a broad audience of non-specialists". In time it stays where

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8 For joined roots, first Chuquet and then Bombelli in his manuscript introduced an underlining (by Bombelli's printer reduced to the two hooks at the end of the line). They did not use them for other purposes.

9 In a few cases, expressions of type $ax+bx$ could have been interpreted as $(a+b)x$, which would have facilitated the application of the rules for solving algebraic cases. However, those few authors who dealt with so complicated matters were bright enough to be able to operate with mental or implicit parentheses.

the previous article ended, in the 17th century; but it does not deal with what we have been accustomed to consider as the Great Science of that epoch. Instead it asks why this Great Science can apparently be presented and analyzed without reference to another essential cultural current of the same years, the Baroque.

A first approach is through general explanations. The Baroque is traced from its origin as an expression of the Counter-Reformation and through the subsequent transformation of Baroque art, leading not only to appeal to strong emotions but also to emphasis on ambiguity and indirect messages. The reliance on emotions and indirect messages links the Baroque current firmly to the kind of public sphere where truth is demonstrated *ad oculos* to the public (a "public sphere" being the social space for the formation of collective world view and collective will – in short, formation of shared ideology). This had not been too far from the "courtly science" of the 16th century; but the New Science, expression of the Republic of Letters and organized in associations of peers (ideally speaking, at least), was based in the other main type of public sphere (the one which the young Habermas believed "bourgeois"), the type where truth and decision come out of debate and argument. The New Science, moreover, strived after and believed in the possibility of unambiguous truth – or, if that could not be attained, in transparent explication of doubts. On both accounts, the two currents were thus opposed in character.

However, some outstanding characters still had a foot in each camp. The article goes in some depth with Juan Caramuel, and speaks mor briefly about Athanasius Kircher and Olaus Rudbeck. Caramuel was the one of them who engaged directly in formulating the Baroque programme with its emphasis on ambiguity. But he also wrote on mathematics – if we count words (roughly a million), his *Mathesis biceps* from 1670 probably exceeds everything Viète, Descartes and Newton together wrote about mathematical topics.

Yet even here what he did was in contrast with the manners of the New Science. When presenting place value systems with bases 2, 3, 4, …, 10, 1 and 60 (in part with appurtenant algorithms) he did not look for a general system; in his opinion, their possibility demonstrates that arithmetic is not one but plural, chosen between by human free decision, and that none of them is absolutely better than the others. When discussing the Ptolemaic, Copernican and Tychonic planetary system, he rejected the first of these as empirically implausible. One might expect him as a Catholic bishop to feel obliged to follow Tycho, but no: even this is presented as a free decision. And when presenting algebra over a hundred folio pages he has decided that algebra is generalized false position and therefore does not go beyond the first degree. Moreover, when discussing the origin of the name "algebra" he first rejects the derivation from the name Geber (Jābir ibn Aflah) on philologically sound grounds. After giving an explanation of his own (using good premises in a most fanciful way) he moves on to a long sequence of Arabic, Greek an Latin etymologies which the reader can choose between if he does not like what has been proposed. Some are characterized as "audacious".

The echo was faint, and the opinions expressed by more central representatives of the New Science (as reflected in the letters to and from Henry Oldenburg, secretary of the Royal Society) were far from positive. Even later, when giving up the belief in "geometric" exactness of scientific arguments, the next generations of Modern science did not discover anything of value in Caramuel. If a forerunner for anything, then Caramuel was a forerunner for certain 20th-century theories of poetry; the Baroque as a whole, for the calculated use of emotions and indirect messages in propaganda and in the advertisement industry.

This last article of Part II can be seen as a counterpart of article I.6: both contribute to understanding what mathematics *is* by looking at practices which, though related, are *not* mathematics.

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Chapter 2 (Article I.1) On Parts of Parts and Ascending Continued Fractions: An Investigation of the Origins and Spread of a Peculiar System

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Corrections of style (not least for references) made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

See “Introduction”.

Contents

OTTO NEUGEBAUER *in memoriam*

Introduction

The following article deals with two particular ways to denote fractional numbers, one of them multiplicative ("parts of parts") and the other multiplicative-additive ("ascending continued fractions"). They turn up in sources from several cultures and epochs, but as a standard idiom only in Arabic mathematics, where their occurrence has been amply described. In certain other contexts (Babylonia, High and Late medieval Europe) their occasional presence has been taken note of though rarely investigated systematically. Finally, a few scattered occurrences in Ancient Greek and Egyptian sources have not been commented upon until this day.

Widespread occurrence of similar practices raises the question of interdependence versus independent development by accident or in response to analogous situations. Thus also in this case. Asking the question, however, turns out to be more easy than answering it, not least because some of the cultures to be dealt with only present us with utterly few examples of the usage, and only the combination of evidence and arguments of many kinds will allow us to construct a scenario which is at least well-founded if not definitively verified on all points.

As a by-product, the inquiry will cast new light on the origins of the Egyptian unit fraction system.

Islamic and post-Islamic evidence

In chapter V of Leonardo Fibonacci's *Liber abbaci* (second version, 1228) a number of complex writings for fractional numbers are introduced. One of them – the others are irrelevant for the present purpose – is what later has come to be called the "ascending continued fraction" ("Aufsteigender Kettenbruch" in German), which Fibonacci exemplifies by the number

$$\frac{\mathbf{1}\ \ \mathbf{5}\ \ \ \mathbf{7}}{\mathbf{2}\ \ \mathbf{6}\ \ \mathbf{10}}$$

meaning 7 10ths plus 5 6ths of a 10th plus $^1/_2$ of a 6th of a 10th[1] – in more compact writing $\{^1/_{10}\cdot[7+(^1/_6)\cdot(5+^1/_2)]\}$. In general

$$\frac{\boldsymbol{a}_3\ \boldsymbol{a}_2\ \boldsymbol{a}_1}{\boldsymbol{b}_3\ \boldsymbol{b}_2\ \boldsymbol{b}_1}$$

stands for

1 Ed. [Boncompagni 1857: 24]. A number of later Italian occurrences until Clavius are discussed by Kurt Vogel [1982].

$$\frac{\boldsymbol{a}_1}{\boldsymbol{b}_1}+\frac{\boldsymbol{a}_2}{\boldsymbol{b}_1\boldsymbol{b}_2}+\frac{\boldsymbol{a}_3}{\boldsymbol{b}_1\boldsymbol{b}_2\boldsymbol{b}_3}=\frac{\boldsymbol{a}_1+\frac{\boldsymbol{a}_2+\frac{\boldsymbol{a}_3}{\boldsymbol{b}_3}}{\boldsymbol{b}_2}}{\boldsymbol{b}_1}$$

The generalization to four or more levels is obvious. Incidentally, the latter expression demonstrates that "ascending continued fractions' have nothing but an inverted visual image in common with genuine continued fractions.

The notation for ascending continued fractions was not invented by Fibonacci but apparently in the Maghreb mathematical school, probably during the 12th century. They are discussed in ibn al-Bannā' 's 13th century *Talkhīs a'māl al-ḥisāb*[2] though without indication of the way they were to be written. Various commentaries show, however, that standardized notations were in use. In one late commentary, al-Qalaṣādī's *Arithmetic*[3] (1448), it is furthermore required that the denominators in an ascending continued fraction stand in descending order from the right ($b_1 > b_2 > b_3$), as it is actually the case in Fibonacci's examples. Even though some of the examples given by other commentators[4] do not observe this rule, which I shall denote *al-Qalaṣādī's canon* in the following, it was probably not of al-Qalaṣādī's own making. The purpose of the canon may have to do with the value of the first member as an approximation. The error committed by throwing away all members but the first will necessarily be less than $1/b_1$ (a's are supposed to be less than corresponding b's). Choosing b_1 as large as possible will ensure that a_1/b_1 is a good approximation (though not necessarily the optimal approximation, cf. note 28). Thus, in Fibonacci's example, dividing first by 10 ensures that the first member will at most be 0.1 off the true value. If the reverse canon ($b_1 < b_2 < b_3$) had been used, the result had been ${}^1/_2+{}^1/_2 \cdot {}^1/_2 + {}^1/_2 \cdot {}^1/_2 \cdot {}^1/_6$, and the error committed by taking the first member alone would have been ${}^7/_{24} \approx 0.29$.

The invention of *notations* was part of the general drive of Maghreb mathematics, but verbally expressed ascending continued fractions and other composite fractional expressions belonged to the common lore of Arabic mathematics. They had been amply used and discussed in the later 10th century by Abū'l-Wafā' in his *Book on What Scribes, Officials and the Like Need from the Science of Arithmetic*.[5] They are also present in

2 Ed., trans. [Souissi 1969: 70*f*].

3 Ed. [Souissi 1988: Arabic 59*f*, trans. (with left-right inversion of the scheme) 41*f*].

4 Quoted in [Djebbar 1981: 46*f*].

5 See [Youschkevitch 1970; 1976: 25*ff*]; or [Saidan 1974]. My poor Russian has not permitted me to make much use of M. I. Medovoj's fuller description [1960] of Abū'l-Wafā' 's treatise. Nor have I been able to use Ahmad Saidan's Arabic edition [1971: 64-368] of the work.

al-Khwārizmī's early ninth century *Algebra*[6] as well as in the *Liber mensurationum* by one Abū Bakr, translated by Gerard of Cremona into Latin in the 12th century and presumably written in the first place around 800 CE[7]. Among the occurrences in al-Khwārizmī's work are the following (page references to Rosen's translation):

- P. 24, $^{25}/_{36}$ is transformed into "two-thirds and one-sixth of a sixth" [$^{2}/_{3} + {}^{1}/_{6} \cdot {}^{1}/_{6}$].
- P. 45, 1 *māl* is found as "a fifth and one-fifth of a fifth" of $4^{1}/_{6}$ *māl* [$^{1}/_{5} + {}^{1}/_{5} \cdot {}^{1}/_{5}$].
- P. 53: "three and three-fourths of twenty parts" [$^{3}/_{20} + {}^{3}/_{4} \cdot {}^{1}/_{20}$] is transformed into "fifteen eightieths".
- P. 54, a twelfth is expressed as "the moiety of one moiety of one-third" [$^{1}/_{2} \cdot {}^{1}/_{2} \cdot {}^{1}/_{3}$].
- P. 72, as one of several rules for finding the circular area we find the square of the diameter minus "one seventh and half one-seventh of the same.
- P. 88, the third of "nine dirhems and four-fifth of thing" is found to be "three dirhems, and one-fifth and one-third ⟨of⟩ one-fifth of thing" [$3 + {}^{1}/_{5} + {}^{1}/_{3} \cdot {}^{1}/_{5}$].
- P. 99. two-sevenths and two thirds of a seventh of the share of a son" [$^{2}/_{7} + {}^{2}/_{3} \cdot {}^{1}/_{7}$].

The *Liber mensurationum* (which contains mostly integer numbers) presents us with the following relevant passages:

- N° 19 (p. 90), *7 et dimidium septime*.
- N° 89 (p. 107), *43 et due quinte et quattuor quinte quinte*, resulting from the computation of $169\text{-}(11^{1}/_{5})^{2}$. Similarly but in greater computational detail in N° 128 (p. 115).
- N° 113 (p. 112), the root of $^{3}/_{16}$ *census* is expressed as *radix octave census et medietatis octave census*.
- N° 144 (p. 118), the area of the circle is expressed as the square on the diameter minus *septimam et septime eius medietatem*. Similarly in N^{os} 146, 156 and 158 (pp. 119 and 124).

The elementary building stones of the ascending continued fractions are the "parts of parts", the *partes de partibus* as they came to be called in the medieval Latin tradition, that is, expressions of the form "$^{p}/_{q}$ of $^{1}/_{r}$". The extent to which these were natural to Arabic speakers of early Islam is demonstrated in the first treatise of the 10th century *Epistles of the Brethern of Purity*, the *Rasāʾil ikhwān al-safāʾ*. In this exposition of the fundaments of arithmetic great care is taken to explain that the first of a collection of two is called a half, while the first of three is a third, that of four a fourth, and that of eleven one part of eleven; the first of twelve, however, is labeled a half of a sixth, without a single word commenting upon the reasons for or meaning of this composition. Similarly, the first of fourteen is expressed without explanation as a half of a seventh, and that of fifteen as a third of a fifth.[8]

The origin of both the parts of parts and of the ascending continued fractions has been ascribed to a variety of causes, in particular to the peculiarities of the Arabic

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6 Ed., trans. [Rosen 1831].

7 Ed. [Busard 1968]. As to the dating (built on terminological considerations), see [Høyrup 1986].

8 Trans. [Brentjes 1984: 212*f*].

vocabulary. Unit fractions from $^1/_2$ to $^1/_{10}$ possess a particular name of their own while those with larger denominators require a full phrase, $^1/_n$ being expressed as "one part of n or "one part of n parts" *unless* it can be composed from unit fractions with smaller denominators. This might indeed explain that the Arabic authors transformed the $^1/_{14}$ of Heron's (that is, pseudo-Heron's) rule for finding the circular area[9] into "half one-seventh", and that they expressed $^1/_{25}$ as "one-fifth of a fifth".

On the other hand, "the moiety of one moiety of one-third" is somewhat at odds with the hypothesis: Why not "one-third of a fourth", when in the actual case the number 12 arises as 3·4? Or at least "one-half of a sixth", which according to Abū'l-Wafā' is to be preferred to "one-third of a fourth",[10] and which still circumvents the difficulties created by the Arabic language while using only two factors? Al-Khwārizmī, moreover, had no particular difficulty with general fractions, at times with denominators exceeding 10, which abound even in those very calculations where the "parts of parts" turn up. The reason that the reciprocal of $^{25}/_6$ is expressed in the form of an ascending continued fraction on p. 45 of the *Algebra* while another ascending continued fraction is, reversely, reexpressed as $^1/_{80}$ on p. 53 seems simply to be that both reformulations fit the further calculations better. The conventional explanation of the use of composite expressions based solely on Arabic linguistic particularities is apparently insufficient, even if these particularities have evidently tainted the way the system was used.

Classical Antiquity and its legacy

The need for an explanation which goes beyond the peculiarities of the Arabic language is confirmed by certain older sources. One of them is the collection of arithmetical riddles in *Anthologia Graeca* XIV.[11] A study of these gives the fascinating result that the types of fractional expressions used vary with the subject of the problem. Problems which refer to Greek mythology or history make use of unit or general fractions. So do all problems dealing with apples or walnuts stolen by girl friends, with the filling of jars or cisterns from several sources, with spinners', brickmakers' or gold- or silversmiths' production, with wills, and with the epochs of life – none of them make use of "parts of parts". "Parts of parts" and related composite expressions, on the other hand, turn up in all problems dealing with the Mediterranean extensions of the Silk Road (N^os^ 121 and 129), with the legal partition of heritages 128 and 143), and with the hours of the day (N^os^ 6, 139, 140, 141, and 142; N^o^ 141 is connected to astrology). A final "fifth

9 The square on the diameter minus $^1/_7$ and $^1/_{14}$ of the square. *Geometrica* **24**.40 [ed., trans. Heiberg 1912: 442*f*]. Cf. *Geometrica* **17**.4, *ibid*. 332^b, 333^b.

10 [Saidan 1974: 368].

11 Ed., trans. [Paton 1918]. The editor of this part of the *Anthologia* was probably Metrodoros (fl. c. 500 CE), but the single epigrams are older.

of a fifth" is found in N° 137, dealing with a catastrophic banquet probably meant to be held in Hellenistic Syria. It appears that a number of recreational problems belonging to (at least) two different contexts (providing the dress of the problems) have been brought together in the anthology, each conserving its own distinctive idiom for fractions: on one hand the traditional Greek idiom, which makes use of general and unit fractions; on the other, the usage of the trading community and of juridical calculators (and perhaps of astrologers and makers of celestial dials), which is different.

We may list the various composite fractional expressions:[12]

– N° 6 (the hour of the day): "Twice two-third".
– N° 121 (travelling from Cadiz to Rome): "One-eighth and the twelfth part of one-tenth".
– N° 128 (a textually and juridically corrupt heritage): "The fifth part of seven-elevenths".
– N° 129 (travelling from Crete to Sicily): "Twice two-fifths".
– N° 137 (the Syrian banquet): "A fifth of the fifth part".
– N° 139 (a dial-maker asked for the hour of the day): "Four times three-fifths".
– N° 140 (the hour of a lunar eclipse): "Twice two-sixths and twice one-seventh".
– N° 141 (the hour of a birth, to be used for a horoscope): "Six times two-sevenths".
– N° 142 (The hour for spinning-women to wake up): "A fifth part of three-eighths".
– N° 143 (The heritage after a shipwrecked traveller): Twice two-thirds".

We observe that the character of these composite expressions is similar to but does not coincide with what we know from the Arabic texts. Firstly, of course, these do not contain integer multiples of fractions like those of 6, 129, 139, 140, 141 and 143, and they would speak of "three fifths of an eighth", not of "a fifth part of three-eighths". Secondly, the Arabic sources mostly follow the canon made explicit by al-Qalaṣādi, while for instance N° 121 of the *Anthologia* does not – and $^1/_{12}$ they split further, namely into $^1/_2$ of $^1/_6$, into $^1/_3$ of $^1/_4$ or even, as we have seen, into $^1/_2$ of $^1/_2$ of $^1/_3$.

Most likely, the integer multiples of the *Anthologia* are to be explained from the recreational character of the arithmetical riddles: being unusual, the multiples make the riddles more funny or more obscure at first sight – it is hardly imaginable that "two-thirds" would be expressed as "twice two-sixths" for any everyday purpose. The demands of versification may have played a supplementary rôle – but since problems with a traditional "Greek" subject make no use of the stratagem hardly more than a supplementary rôle.

The deviation from "al-Qalaṣādī's canon", however, gives no impression of grotesquerie and can therefore not be an effect of the recreational purpose of the epigrams. It is thus probable that it reflects the daily usage of the practitioners trading in "parts of parts", which will not have respected the later Arabic canon and customs in full.

[12] I follow W. R. Paton's translation, even though a somewhat more literal translation of some fractional expressions could be made. Paton's concessions to English rhythm are immaterial for the present purpose.

Another, Latin, source of interest for our purpose is the Carolingian collection *Propositiones ad acuendos iuvenes* conventionally ascribed to Alcuin of York and dating from c. A.D. 800.[13] Chronologically, it is roughly contemporary with al-Khwārizmī and probably with the *Liber mensurationun*. The material, however, appears to be inherited from late Antiquity, and the Carolingian scholar (be it Alcuin or somebody else connected to the Carolingian educational effort) has only acted as an editor.

A brief exposition of the global character of the collection will serve the double purpose of locating its composite fractional expressions with respect to their background and of introducing some notions concerning the function of recreational problems from which the further discussion will benefit. In general, the collection is highly eclectic, bringing together material and methods from a variety of traditions, combining at times mutually incompatible approximations within the same problem solution.[14] Of particular interest in the present context is the very diverse network of connections behind the arithmetical problems. N° 13, dealing with 30 successive doublings of 1, points back to a very similar problem from Old Babylonian Mari[15] and eastward to the Arabo-Indian chess-board problem and even to China. N^os^ 5, 32-34, 38-39 and 47 all belong to the type of "a hundred fowls" known from earlier Chinese and contemporary or earlier Indian sources[16] and presented by Abū Kāmil as a type of question

> circulating among high-ranking and lowly people, among scholars and among the uneducated, at which they rejoice, and which they find new and beautiful; one asks the other, and he is then given an approximate and only assumed answer, they know neither principle nor rule in the matter.[17]

Other problems too point to the "oral technical literature", the treasure of recreational problems shared and carried by the community of traders and merchants interacting along the Silk Road, the combined caravan and sea route reaching from China to Spain.[18]

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13 Ed. [Folkerts 1978].

14 See [Høyrup 1987: 291 n. 38] ("an/42.9" in line 9 from bottom should read "and").

15 Published in [Soubeyran 1984: 30]. The connection and similarities between the Carolingian doublings and those from other epochs and places (except China) are discussed in detail in [Høyrup 1986: 477-479]. On China, see [Thompson 1975: V, 542] (Z 21.1), or [Høyrup 1987: 288*f*].

⟦**I** have now managed to decipher Thompson's somewhat opaque reference and to get hold of his source, namely [Graham 1978: 278]. It has nothing to do with the matter, and China can be left safely out of the picture.⟧

16 See the survey in [Tropfke/Vogel et al 1980: 613-616].

17 My English translation from [Suter 1910: 100].

18 The classification of recreational mathematics as a parallel to folk-tales and riddles, and thus as a special genre of oral literature, is discussed in [Høyrup 1987: 288*f* and 1990: 74*f*].

The influence of eastern trading routes on the stock from which the *Propositiones* were drawn is also made clear by problems N^os^ 39 and 52, dealing, respectively, with the purchase of animals

Connections to the *Anthologia graeca* and thus to the Greco-Roman orbit are also present. Most significant is probably N$^{\text{o}}$ 35, which is a puzzle on heritages – one of the types, we remember, which made use of multiples of parts. It can be traced back to Roman jurisprudential digests, even though the editor of the *Propositiones* has got the solution wrong.[19]

A final type represented by N$^{\text{os}}$ 2, 3, 4, 40 and 45 seems to bypass what we know from the *Anthologia graeca* and points directly to Egyptian traditions (even though matters may in reality be more complex, cf. below, p. 52 ⟦and, more decisively, note 64⟧). Admittedly, when expressed in algebraic symbolism, the problems in question are of a type identical with the one dominating the *Anthologia graeca*, both being represented by first degree equations. The equations of the *Anthologia*, however, are variations on the pattern

$$x \cdot \left(1 - {}^{1}\!/_{p} - {}^{1}\!/_{q} - {}^{1}\!/_{r}\right) = \mathrm{R}$$

(p, q, and r being integers), while N$^{\text{os}}$ 2, 3, 4, 40 and 45 of the *Propositiones* build on the scheme

$$x \cdot (n + \alpha + \beta) = T$$

(n being an integer larger than 1 and α and β being unit fractions or "parts of parts"). Both types possess analogues in the Ancient Egyptian Rhind Mathematical Papyrus.[20] The former type corresponds approximately to 24-27 N$^{\text{os}}$ and N$^{\text{os}}$ 31-34; these are problems which consider an unspecified quantity or "heap" ('*h*'), and which only differ from those of the *Anthologia* by adding the unit fractions instead of subtracting them. The first-degree problems of the *Propositiones* just spoken of, on the other hand, belong to the same type as Rhind Mathematical Papyrus N$^{\text{os}}$ 35-38, problems dealing with the *ḥeḳat*-measure.[21]

The reason for this lengthy presentation of the *Propositiones* and of a particular group of first-degree problems is that four of the five problems in this group employ "parts of parts":

- N$^{\text{o}}$ 2: *medietas medietatis, et rursus de medietate medietas* (meaning ${}^{1}\!/_{2} \cdot {}^{1}\!/_{2} + {}^{1}\!/_{2} \cdot {}^{1}\!/_{2} \cdot {}^{1}\!/_{2}$).
- N$^{\text{o}}$ 3: *ter et medietas tertii* (${}^{1}\!/_{3} + {}^{1}\!/_{2} \cdot {}^{1}\!/_{3}$)
- N$^{\text{o}}$ 4: *medietas medietatis* (${}^{1}\!/_{2} \cdot {}^{1}\!/_{2}$).
- N$^{\text{o}}$ 40: *medietatem de medietate et de hac medietate aliam medietatem* (${}^{1}\!/_{2} \cdot {}^{1}\!/_{2} + {}^{1}\!/_{2} \cdot {}^{1}\!/_{2} \cdot {}^{1}\!/_{2}$).

(including camels) *in oriente* and with transport on camel back.

19 [Folkerts 1978: 33].

20 Ed., trans. [Chace et al. 1929].

21 Another group from the *Propositiones*, consisting of N$^{\text{os}}$ 36, 44 and 48, deviates from both models but comes closest to the *ḥeḳat*-type.

Composite fractions thus seem to go naturally with this problem type. On the other hand, they occur nowhere else, neither in the problems which point to the "Silk Road corpus", nor those which remind of one or the other group from the *Anthologia graeca*, nor in the inheritance problem. One observes that al-Khwārizmī's predilection for taking successive halves instead of a simple fourth is equally present here, and is even extended to the use of $^1/_2$ of $^1/_3$ instead of $^1/_6$. This is all the more remarkable since the simple terms *quadrans* and *sextans* were at hand,[22] and the composite *quarta pars* and *sexta pars* are actually used in other parts of the text (e.g., N^{os} 8 and 47). It will also be noticed that three of the four cases are rudimentary ascending continued fractions.

Babylonia

Some scattered instances of "parts of parts" and of simple ascending continued fractions can thus be dug out from sources belonging to or pointing back to classical Antiquity though not to the core of Greek mathematical culture.[23] Antecedents for the fuller use of ascending continued fractions, on the other hand, must be looked for further back in time – much further, indeed.

They can be found in the Babylonian tablet MLC 1731, which was analyzed by Abraham Sachs[24] and which dates from the Old Babylonian period (c. 2000 to c. 1600 BCE; the mathematical texts belong to the second half of the period). It presents us with the following examples of composite fractions:[25]

– N^o 1: "One-sixth of one-fourth of [the unit] a barleycorn".
– N^o 3: "One-fourth of a barleycorn and one-fourth of a fourth of a barleycorn".
– N^o 4: "One-third of a barleycorn and one-eighth of a third of 20".[26]

22 In the sense that the use of these subdivisions of the *as* as names for abstract fractions is described explicitly in the preface to the fifth-century *Calculus* of Victorius of Aquitania [ed. Friedlein 1871: 58*f*].

23 This peripheral status of the Greek "parts of parts" is borne out by Ananias of Shirak's seventh-century arithmetical collection [ed., trans. Kokian 1919], a work strongly dependent on contemporary Byzantine teaching. "Parts of parts" are as absent from this work as from the "Greek" problems of the *Anthologia graeca*.

24 [Sachs 1946]. Besides the fractional expressions of that tablet, the article presents and discusses similar usages in other Babylonian tablets.

25 In my translation of Babylonian texts, I follow the following conventions:
- "The *n*'th part" renders the expression "i g i - *n* - g á l".
- Fractions and numbers written with numerals ($^2/_3$, $^1/_2$, etc.; 1, 2, etc.) render special cuneiform signs for these fractions and numbers.
- Fractions and numbers written as words render corresponding expressions in syllabic writing.

26 In all metrological systems, the barleycorn is 0;0,0,20 times the fundamental unit. "20" is thus a shorter way to write "a barleycorn".

- N° 5: ‘Two-thirds of 20 and one-eighth of two-thirds”.
- N° 6: “A barleycorn and one-sixth of a fourth of 20”.
- N° 7: “A barleycorn, two-thirds of 20 and one-eighth of two-thirds of 20”.
- N° 9: “17 bar⟨leycorns⟩, one-third of 20, and one-fourth of a third of a barleycorn”.

All these composite expressions result from the conversion of numbers belonging to the “abstract” sexagesimal system into metrological units. Sachs has convincingly pointed out that the notation in question is used because no unit below the barleycorn existed[27] – fractions could not be expressed in terms of a smaller unit, as done in other conversions to metrological notation. Still, the tablet shows that the parlance of “parts of parts” *was* at hand, and even that there was an outspoken tendency to make use of ascending continued fractions rather than of sums of unit fractions with denominators below 10.[28] We observe that two-thirds is the only general fraction to turn up, while everything else consists of unit fractions and their combinations,[29] and that “al-Qalaṣaḍī’s canon” is inverted – be it accidentally or by principle.

This tablet presents us with the most systematic Old Babylonian use of composite fractions. It is not quite isolated, however, and scattered occurrences can be found here and there in other Old Babylonian tablets.

One instance was pointed out by Sachs: YBC 7164 N° 7 (line 18), where the time required for a piece of work is found to be “$^2/_3$ of a day, and the 5th part of $^2/_3$ of a day”.[30]

In another text from the Yale collection, “parts of parts” (though no ascending continued fractions) occur in all five times: YBC 4652 19-22,[31] problems of riddle-character dealing with the unknown weight of a stone. Here, “the 3d part of the 7th part”,

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27 Except in the system of weights, where $^1/_2$ barleycorn existed as a separate unit – cf. [Sachs 1946: 208*f* and note 18]. Most likely, however, the text is concerned with area units (among other things because the numbers to be converted are obtained as products of two factors, both of which vary from problem to problem).

28 In N° 4, the result could have been given as “$^1/_4$+$^1/_8$” (or as “$^1/_4$+$^1/_2 \cdot {}^1/_4$”). In N^{os} 5 and 7, “$^1/_2$ + $^1/_4$” (or “$^1/_2$ + $^1/_2 \cdot {}^1/_2$”), and in N° 9, “$^1/_4$ + $^1/_6$” would have been possible.

The actual choices of the texts secure that the first member alone approximates the true value as closely as possible. They demonstrate that “al-Qalaṣādī’s canon”, even though ensuring that the first member of the expansion is a *fair* approximation, of course does not guarantee it to be *optimal*.

29 Naturally enough, this reminded Sachs of the Egyptian unit fraction system (as also borrowed by the Greeks): Even there, $^2/_3$ is treated on a par with the sub-multiples $^1/_2$, $^1/_3$, $^1/_4$, etc. He did not make much of the fact that “$^1/_3$ of $^1/_5$” would be no number to an Egyptian scribe but a problem with the solution “$^1/_{15}$”. Nor was he apparently aware that much closer parallels to his notation could be found in the Arabic orbit.

30 [MCT, 82]. Discussed in [Sachs 1946: 212].

31 [MCT, 101].

“the 3d part of the 13th part”, “the 3d part of the 8th part” (twice) and “$^2/_3$ of the 6th part” turn up. We observe that the ordering of factors agrees with “al-Qalaṣādī”’s canon”, and that even a “13th part” is present (Babylonian, in contrast to Arabic, had a name for this fraction).

In the series text YBC 4714, N^o 28, line 10 (and probably also in the damaged text of N^o 27), “a half of the 3d part” turns up in the statement.[32] This is evidently meant as a step toward greater complexity from the previous problems having “the *n*’th part” (n = 7, 4, and 5) in the same place.

A text of special interest is the Susa tablet TMS V.[33] All the way through the tablet, sequences of numbers are used as abbreviations for complex numerical expressions involving parts of parts. Recurrent from section to section (albeit with some variation), 13 times in total, is the following series (the right column gives the interpretation)

a: “2”	2
b: “3”	3
c: “4”	4 (cf. the different meaning in g)
d: “$^2/_3$”	$^2/_3$
e: “$^1/_2$”	$^1/_2$
f: “$^1/_3$”	$^1/_3$
g: “4”	$^1/_4$
h: “$^1/_3$ 4”	$^1/_3$ of $^1/_4$
i: “7”	$^1/_7$
j: “2 7”	2 times $^1/_7$
k: “7 7”	$^1/_7$ of $^1/_7$
l: “2 7 7”	2 times $^1/_7$ of $^1/_7$
m: “11”	$^1/_{11}$
n: “2 11”	2 times $^1/_{11}$
o: “11 11”	$^1/_{11}$ of $^1/_{11}$
p: “2 11 11”	2 times $^1/_{11}$ of $^1/_{11}$
q: “11 7”	$^1/_{11}$ of $^1/_7$
r: “2 11 7”	2 times $^1/_{11}$ of $^1/_7$
s: “$^2/_3$ $^1/_2$ $^1/_3$ 11 7”	$^2/_3$ of $^1/_2$ of $^1/_3$ of $^1/_{11}$ of $^1/_7$
t: “2 $^2/_3$ $^1/_2$ $^1/_3$ 11 7”	2 times $^2/_3$ of $^1/_2$ of $^1/_3$ of $^1/_{11}$ of $^1/_7$

In section 10 we also find

A: “1 $^2/_3$”	1 plus $^2/_3$
B: “1 $^1/_2$”	1 plus $^1/_2$
C: “1 $^1/_3$”	1 plus $^1/_3$
D: “1 4”	1 plus $^1/_4$
E: “1 $^1/_3$ 4”	1 plus $^1/_3$ of $^1/_4$
F: “1 7”	1 plus $^1/_7$

32 [MKT I, 490].

33 [TMS, 35-49]. The tablet has probably been prepared toward the end of the Old Babylonian period.

G: "1 2 7"	1 plus 2 times $^1/_7$
H: "1 7 7"	1 plus $^1/_7$ of $^1/_7$
I: "1 2 7 7"	1 plus 2 times $^1/_7$ of $^1/_7$
J: "2 $^1/_2$"	2 plus $^1/_2$
K: "3 $^1/_3$"	3 plus $^1/_3$
L: "4 4"	4 plus $^1/_4$ (not $^1/_4$ of $^1/_4$)
M: "7 i g i-7"	7 plus $^1/_7$
N: "7 2 i g i-7"	7 plus 2 times $^1/_7$

In all cases, the expressions multiply the side of a square (literally: count the number of times the side is to be taken).

In order to make *his text* as unambiguous as possible, the scribe has followed a fairly strict format, most clearly to be seen in *t* and *N*: starting from the right, he lists (with increasing *denominator*) those fractions which in full writing would be written i g i - *n* - g á l, and which he abbreviates as the integer numeral *n*; next come, in increasing *magnitude*, fractions possessing their own ideogram ($^1/_3$, $^1/_2$ and $^2/_3$). This entire section of the sequence is to be understood as "parts of parts". Then follows an (optional) integer numerator (> 1), and finally an (equally optional) integer addend. As long as the numerator is kept at 2 and the addend at 1, the system is unambiguous. If we violate these restrictions (as in *c* and *L*), however, it stops being so. Inside the text, the systematic progress eliminates the ambiguities: if used as a general notation, on the other hand, the system would lead to total confusion – a fact which is obviously recognized by the scribe, since he introduces ad hoc the sign i g i in *M* and *N*.

These observations entail the conclusion that we are confronted with a specific, context-dependent shorthand, not with a standardized notation for general fractions, as claimed by Evert Bruins.[34] Behind the shorthand, moreover, stick not just general fractions but the system of "parts of parts"; the summation required by the ascending continued fractions, on the other hand, is not visible through the notation.

In the end of the above-mentioned article, Sachs[35] reviews a number of Seleucid notarial documents making use of composite expressions often involving "parts of parts" (all examples apart from N° 15 deal with the sale of temple prebends corresponding to parts of the day):

(1) "A fifth of a day and a third from a 15th of a day".
(2) "A sixth, an 18th, and a 60th".
(3) "A 30th, and a third from a 60th".
(4) "A half from three quarters".
(5) "A fifth from two thirds".

34 [TMS, 36].

35 [Sachs 1946: 213*f*]. In the present case I take over Sachs's translation, except that I translate *ina* as "from" instead of "in".

(6) “Two thirds of a day and an 18th of a day”.
(7) “A sixth and a ninth of a day”.
(8) “A 20th from one day, of which a sixth from a 60th of a day is lacking”.
(9) “A 16th and a 30th of a day”, added to “a 16th of a day”, giving “an eighth and a 30th of a day”.
(10) “An eighth from a seventh”.
(11) “A half from an eighteenth”.
(12) “A third from a twelfth”.
(13) “An 18th from a seventh”.
(14) “A twelfth from a seventh”.
(15) “A half from a twelfth” (as a share of real estate).

Sachs rightly observes that the system seems less strict than the old one. In cases where the number is expressed as a sum, no particular effort is made to assure that the first member is an optimal approximation, nor to follow the strict pattern of an ascending continued fraction.[36] From the present perspective, it may be of interest that all “parts of parts” *except those involving the irregular* $^1/_7$ respect “al-Qalaṣādī’s canon”.[37] The Arabic avoidance of denominators larger than 10, of course, is not observed.

Egypt

Its building stones being unit fractions with small denominators, the “parts of parts” scheme has often been connected to the Egyptian unit fraction system. In its mature form, as we know it from Middle Kingdom through Demotic sources, however, the Egyptian system had no predilection for those small denominators which it is the purpose of the “parts of parts” scheme to achieve. The Egyptians, furthermore, were not interested in such splittings where the first member can serve as a good first approximation, whereas a fair first approximation is a key point in the extension of the “parts of parts” into ascending continued fractions (as we met it already in the Old Babylonian tablet, cf. note 28). Attempts to explain the schemes of “parts of parts” and ascending continued fractions by reference to the Egyptian unit fractions system thus appear to be misguided.

“Parts of parts” as discussed above are not common in Egypt. In fact, I only know of three places where the usage is employed *to indicate a number*[38] (cf. below on other

[36] Thus, N° 2 could have been rearranged as $^1/_5+^1/_{45}+^1/_{60}$ or, alternatively, as $^1/_5+^1/_6\cdot{}^1/_5+^1/_6\cdot{}^1/_6\cdot{}^1/_5$; N° 7 either as $^1/_4+{}^1/_9\cdot{}^1/_4$ or as $2\cdot{}^1/_9+{}^1/_2\cdot{}^1/_9$. N^{os} 3 and 6 only need reformulation and no rearrangement in order to agree with the pattern of ascending continued fractions.

[37] “May be of interest” but need not, at least as far as the history of *mathematical* ideas and notations is concerned. Indeed, in an article discussing some of the same examples and a number of others Denise Cocquerillat [1965] points out that the expressions are chosen in a way which will make the merchandise look as impressing as possible to a mathematically naive customer. The governing principle may thus have been sales psychology rather than any general idiomatic preference.

[38] True enough, as pointed out by a referee, these numbers are no pure numbers: they represent

applications). The first of these is Rhind Mathematical Papyrus (RMP). Problem 37, one of the *ḥeḳat*-problems which were mentioned above in connection with the *Propositiones ad acuendos iuvenes*:

> Go down I [i.e., a jug of unknown capacity] times 3 into the *ḥeḳat*-measure, $^1/_3$ of me is added to me, $^1/_3$ of $^1/_3$ of me is added to me, $^1/_9$ of me is added to me; return I, filled am I. Then what says it?[39]

The second is Problem 67 of the same papyrus,

> Now a herdsman came to the cattle-numbering, bringing with him 70 heads of cattle. The accountant of cattle said to the herdsman, Small indeed is the cattle-amount that thou hast brought. Where is then thy great amount of cattle? The herdsman said to him, What I have brought to thee is: $^2/_3$ of $^1/_3$ of the cattle which thou hast committed to me … .[40]

The third example of "parts of parts" used to indicate a number, finally, belongs in the Moscow Mathematical Papyrus (MMP), Problem 20, where $2^2/_3$ is told to be $^1/_5$ of $^2/_3$ of 20.[41]

The latter example is put into perspective in RMP, "Problem" 61B, which explains the method to find $^2/_3$ of any unit fraction with odd denominator, and uses $^2/_3$ *of* $^1/_5$ as a paradigm.[42] The $^1/_5$ *of* $^2/_3$ which appears as a regular number in the MMP is thus (reversion of factors apart, which was trivial to the Egyptians) not recognized as such in the RMP, N° 61B: a composite expression like $^1/_5$ *of* $^2/_3$ was to be considered a *problem* and no number *per se* (a problem whose answer is $^1/_{10}$ + $^1/_{30}$). The same observation can be made on RMP, "Problem" 61, which is in fact a tabulation of a series of solutions to such problems.[43]

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the value of one quantity measured by another – a *ḥeḳat*-measure gauged by a jug, the toll on a herd of cattle as part of the original herd, the number $2^2/_3$ measured by the number 20. But this is precisely what numbers are mostly used for in daily practice, in Ancient Egypt as elsewhere, and also the way numbers most often occur *within* calculations in Egyptian mathematical texts.

39 [Chace et al 1929, Plate 59]. The grammatical construction used is *$^1/_3$ n $^1/_3$* the indirect genitive, which is also used in expressions like *$^1/_{10}$ of this 10* (RMP 28), $^1/_3$ + *$^1/_4$ of cubit* (RMP 58), *$^1/_3$ + $^1/_5$ of this 30* (MMP, 3), etc. (here as in all transcriptions of Egyptian unit fraction sums I modernize the writing; the original text merely juxtaposes the denominators with the superscript dot meaning *ro*, "part"). This construction should be distinguished from the reverse construction *z n 5*, "persons until [a total of] 5" discussed by Erhart Graefe [1979].

We observe that the sequence *$^1/_3$ and $^1/_3$ of $^1/_3$* suggests the idea of ascending continued fractions (as do the successive *medietates* in the related *Propositiones*-problems).

40 *Ibid*., Plate 67. I have straightened somewhat the opaque language of the extremely literal translation.

41 Ed., trans. [Struve 1930: 95].

42 [Chace et al 1929, Plate 83].

43 $^2/_3$ of $^2/_3$, $^1/_3$ of $^2/_3$, $^2/_3$ of $^1/_3$, $^2/_3$ of $^1/_6$, $^2/_3$ of ½, etc. (*loc. cit.*). Eric Peet [1923: 103*f*] makes a point

A final use of what appears at first like composite fractional expressions α *of* β turns up in the description of reversed metrological computations and conversions (RMP 44, 45, 46 and 49). As an example we may take RMP 45,[44] which connects the two. A granary is known to contain 1500 *khar* and is supposed to have a square base of 10 cubits by 10 cubits (1 *khar* is $^{2}/_{3}$ of a cube cubit), and the height is looked for. The calculation then proceeds in the following steps:

1	1500
$^{1}/_{10}$	150
$^{1}/_{10}$ of $^{1}/_{10}$ of it	15
$^{2}/_{3}$ of $^{1}/_{10}$ of $^{1}/_{10}$ of it:	10

The key to the calculation is provided by Problem 44, which supplies the corresponding direct computation of the content of a cubic container of 10 cubit by 10 cubit by 10 cubit: the volume is first computed as 10·10·10 [cube cubits] and then transformed into $1000 + {}^{1}/_{2}\cdot 1000 = 1500$ *khar*. A solution of the reverse Problem 45 by geometric reasoning would have to go through these steps in reverted order, transforming first the volume of 1500 *khar* into 1000 cubic cubits, and then dividing by the area of the base or, alternatively, by length and width separately. The text, as we see, proceeds differently, reversing the multiplications of Problem 44 one by one without changing their order. The reversal is thus taking place at the level of computational steps, where the order of divisions does not matter, and not on that of analytical reasoning. The composite expression "$^{2}/_{3}$ of $^{1}/_{10}$ of $^{1}/_{10}$" is not meant as another way to express the number $^{1}/_{150}$ but rather as a way to recapitulate the sequence of computational steps (in other words: To display the algorithm to be used).[45] Its single constituents ($^{2}/_{3}$, $^{1}/_{10}$ and $^{1}/_{10}$) are numbers but the composition is neither an authentic number nor a numerical expression to be trans-

out of a terminological distinction inside the table, which uses the construction α *of* β in cases where α is $^{2}/_{3}$ or can be obtained from $^{2}/_{3}$ by halving or successive halvings, but a construction β, its α (β α *.f*) in other cases. Since some of the formulations have been corrected by the scribe it seems indeed that the distinction is determined by a specific canon (which, as we observe, is broken by the $^{1}/_{5}$ *of* $^{2}/_{3}$ of MMP 20).

44 [Chace et al 1929, Plate 67].

45 What looks like "parts of parts" and ascending continued fractions in the Indian *śulva sútras*, e.g. in the passage customarily interpreted as an approximation $\sqrt{22} \approx 1+{}^{1}/_{3}+{}^{1}/_{34} - {}^{1}/_{3\cdot 4\cdot 34}$, has the same character, i.e., it is a prescription of a (geometric) procedure and no arithmetical number in itself (see *Baudháyana śulva sútra*, ed. [Thibaut 1875: II, 21]). Genuine "parts of parts" are absent from Indian mathematics (as confirmed to me by Guy Mazars in a private communication).

⟦Actually, one Sanskrit writer knows them, and even has a name for them, namely "*bhāgānubandha* fractions" [ed. trans. Raṅgācārya 1912: 63*f*]: the Jaina Mahāvīra. This exception, however, is not really one; precisely Mahāvīra is linked to the Near Eastern tradition in ways other Sanskrit mathematicians are not. See article I.4⟧

formed into a number (a "problem" in the sense which makes "$^2/_3$ of $^1/_5$" a problem and "$^1/_{10}$ + $^1/_{30}$" the answer in RMP 61B).[46]

Though exceptional, the few occurrences of composite fractional expressions *used as legitimate numbers* are sufficient proof that the schemes of "parts of parts" and ascending continued fractions are indeed connected to Egypt though not to be explained with reference to the preferred unit fraction notation of the Egyptian scribes. The Egyptians were able to understand "parts of parts" not only as problems or as sequential prescriptions but also as numbers in their own right. When would they do so?

It is difficult to deduce a rule from only three isolated instances. At least two of the present cases, however, are not isolated but embedded in a specific context, on which I shall make some observations in order to answer the question.

Firstly, the *ḥeḳat*-problems are formulated as riddles. When searching the Rhind Papyrus for other riddles I only found one – namely the cattle problem in N° 67 (this is actually how I first discovered my second instance). Stylistically, these five problems are intruders into a problem collection which is otherwise written in a didactically neutral style.

Secondly, we note that the "$^2/_3$ of $^1/_3$" of the cattle-problem is put into the mouth of the herdsman and not into that of the accountant-scribe (similarly, the $^1/_3$ of $^1/_3$ are the words of a jug).

Thirdly, the similarity was already noted between the *ḥeḳat*-problems and those problems of the *Propositiones* which make use of "parts of parts". The *ḥeḳat-problems* are thus connected to the whole fund of recreational mathematics.

All this matches a comprehension of recreational mathematics as a "pure" outgrowth of practitioners' mathematics.[47] "Parts of parts" appear to have belonged to non-technical, "folk" parlance, that is, to the very substrate from which the riddles of recreational problems were drawn. Scribal mathematics, on the other hand, made use of the highly sophisticated scheme of unit fractions; this was a technical language, and the tool which the scribe would use to solve the recreational riddles even when these were formulated in a different idiom.[48]

A parallel to the Old Babylonian situation is obvious. Even here, the ascending continued fractions appeared when the result of calculations in the "technical system"

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46 The non-numerical function of the composite expressions is confirmed by the non-observance of the canon deduced by Peet from RMP 61 in RMP 44, 45, 46 and 49, which all speak of $^1/_{10}$ *of* $^1/_{10}$ (44–46 also have $^2/_3$ *of* $^1/_{10}$ *of* $^1/_{10}$).

47 See [Høyrup 1990: 66-71].

48 The $^1/_5$ *of* $^2/_3$ of MMP 20, it is true, turns up inside the calculation. It looks like a slip, like the reformulation of a description of computational steps (which in the actual case would rather give $^2/_3$ *of* $^1/_5$) inspired by non-scholarly but familiar idiom.

of sexagesimals had to be transformed into "practical" units, while the "parts of parts" turned up in the statement of the riddles on stones of unknown weight, and when supplementary complication had to be added to purely mathematical problems.

"Parts of parts" *could* have arisen as a non-technical simplification and consecutive extension of the unit fraction system, inspired by the sequential prescriptions of reversed computational schemes. Alternatively, it could be the basis from which the unit fraction system had developed. It is as yet not possible to decide the question with full certainty. Strong chronological arguments can be given, however, for the priority of the folk parlance and the secondary character of the unit fraction system. In order to see that we will have to determine the epoch during which the latter system was developed – a question which has never been seriously approached before.

The unit fraction system is used in fully developed form in the RMP. The original from which this papyrus has been copied is dated to the Middle Kingdom, that is, to the early second millennium. Other papyri computing by means of the unit fraction system, some of them genuine accounts and not materials for teaching or tables for reference, belong to the same period. By this time, general unit fractions had thus become a standard tool for scribal calculators.[49]

Older sources, however, are almost devoid of unit fractions. Old Kingdom scribes made use of metrological sub-units and of those fractions which are *not* written in the standardized way (i.e., $^1/_n$ written as the numeral n below the sign *ro*), namely $^2/_3$, $^1/_2$, and $^1/_3$.[50] Only the Fifth Dynasty Abu Sir Papyri (24th century BCE) present us with the unit fractions $^1/_4$, $^1/_5$ and $^1/_6$.[51] At the same time, however, they present us with striking evidence that the later system was not developed. The sign for $^1/_5$, indeed, appears in the connection "$^1/_5$ $^1/_5$", meaning $^2/_5$. Later, $2{\cdot}(^1/_5)$ (or, as it is expressed in the RMP, "2 called out of 5") would be no number but a problem, the solution of which was $^1/_3+^1/_{15}$ – about one-third of the text of the Rhind Mathematical Papyrus is in fact occupied by the solution of $^2/_n$, n going from 3 to 101.[52] There are thus good reasons to believe that a notation for simple aliquot parts was gradually being extended toward the end of the Old Kingdom,

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49 The scribal corrections in RMP 61 would suggest, however, that the canon deduced by Peet may only have emerged after the writing of the original, but before the copy was made.

50 My main basis for this description of Old Kingdom sub-unity arithmetic is the material presented in [Sethe 1916].

51 I am indebted to Wolfgang Helck for referring me to the publications on the Abū Sir Papyri. The fractional signs in question are found in [Posener-Kriéger & de Cenival 1968: Plates 23-25], cf. translation in [Posener-Kriéger 1976] and the discussion in [Silberman 1975].

52 David Silberman [1975: 249] suggests that the writing be explained as a product of scribal ignorance. In view of the central position occupied in Egyptian arithmetic by doubling and ensuing conversion of fractions this is about as plausible as finding a modern accountant ignorant of the place value system.

but was not yet developed into its mature form. True, Walther Friedrich Reineke[53] thinks that it will have been needed in the complex administration of the Old Kingdom, and thus dates the development to the first three dynasties. As far as I can see, however, real practical tasks are better solved by means of metrological sub-units (which are standardized and can thus be marked out on measuring instruments). The advantage of the unit fraction system is theoretical; it will only become manifest in the context of a school system.

This conclusion is supported by analysis of the pyramid problems of the RMP (N^{os} 56, 57, 58, 59A, 59B, 60). Those of them which appear to deal with "real", traditional pyramids, that is, which have a slope close to that of Old Kingdom pyramids (N^{os} 56-59B), measure the slope in adequate metrological units – namely palms [of horizontal retreat per cubit's ascent].[54] The result of N^{o} 60, which deals with some other, unidentified structure, is given as a dimensionless, abstract number. At the same time, the dimensions of the first five, "real" pyramids are given without the unit, as it would be adequate for master-builders who knew what they were speaking about; N^{o} 60 states the data as numbers of cubits, as suitable for a teacher instructing students who do not yet know the concrete practices and entities spoken about. It is thus likely that the author of the papyrus took over the first 5 problems with their metrological units from an older source but created or edited the final, abstract problem himself.[55]

The time when teaching changed from apprenticeship to organized school teaching is fairly well-established.[56] Schools were unknown in the Old Kingdom (if we do not count the education of sons of high officials together with the royal princes), which instead relied upon an apprentice-system. Only after the collapse of the Old Kingdom do we find the first reference to a school (and the absence of a God for the school shows that schools only arose when the Pantheon had reached its definitive structure). By the time of the early Middle Kingdom, on the other hand, scribal education was school education. There is thus a perfect coordination between the changing educational patterns, the move from metrological toward pure number, and the development of the full unit fraction system as far as it is reflected in the sources.

It is therefore fairly certain that the systematic use of unit fractions was a quite recent development when the original of the Rhind Papyrus was written – and implausible, as a consequence, that a non-technical usage built on "parts of parts" should already have been derived from it. On the other hand, the traces of an incipient use of the unit fraction

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53 [Reineke 1978: 73*f*].

54 See the comparison of real and "Rhind" slopes in [Reineke 1978: 75 n. 28].

55 This is also plausible from "a serious [conceptual] confusion [which] has taken place" in the text of N^{o} 60, and which is pointed out and discussed by Peet [1923: 101*f*].

56 See [Brunner 1957: 11-15], and John A. Wilson in [Kraeling & Adams 1960: 103].

notation in the Abu Sir Papyri fits a development starting from a set of elementary aliquot parts in popular use but extending and systematizing this idiom in agreement with the requirements of school teaching.

A scenario

The single occurrences of "parts of parts" and ascending continued fractions are easily established. When it comes to questions of precedence and to possible connections, however, conclusions will have to be built on indirect evidence and on plausibility. Instead of proposing candidly a theory and claiming it to be necessary truth I shall therefore propose a *scenario* and, in cases where this is needed, try to evaluate the merits of alternative interpretations. Instead of treating the matter in chronological order I shall begin with the most obvious, leaving the more intricate matters to the end.

Most obvious of all are the connections within Western Asia. The Old Babylonian "parts of parts" and ascending continued fractions are so close to the usage later testified in Arabic sources that the existence of unbroken habits in the Babylonian-Aramaic-Arabic-speaking region is beyond reasonable doubt. The minor differences between canons and materializations of shared principles can easily be explained as effects of the peculiarities of the single languages and from the use of different computational tools or techniques.

In the early Islamic period, the composite fractions belonged to the "finger-reckoning" tradition and thus to the non-scholarly discourse of merchants and other practical reckoners.[57] One may assume this to have been the case already in earlier times – not least because most of the Old Babylonian occurrences suggest so. The intense interaction of merchants along the Silk Road, which was able to carry a shared culture of recreational problems, will also have been able to spread a Semitic merchants' usage to traders and calculators of neighbouring civilizations. The early role of the Phoenicians and the persistent participation of Syrian and other Near Eastern merchants in Mediterranean trade, in particular, will have been an excellent channel for the spread of the system to the West (as it was probably the channel through which a shared system of finger-reckoning spread from the Near East to the whole Mediterranean region and as far as Bede's Northumbria).[58] The striking coincidence that problems from the *Anthologia graeca* concerning parts of the day refer to the very usage which also turns up in Seleucid calculations dealing with that subject, as well as the references to astrology and to dial-makers in the *Anthologia*, suggests that not only traders but "Chaldean" astrologers and instrument-makers were involved in the spread of the usage from the Near Eastern to the Greek orbit.

[57] After the mid-11th century, the originally separate "finger-reckoning" and "Hindu reckoning" traditions merged – cf. [Høyrup 1987: 309-11]. Al-Qalaṣādī, like Fibonacci, would hence combine the two.

[58] References in [Høyrup 1987: 291].

To the Greek orbit, but not general spread *within* the orbit of Greek culture. The reason that we can speak of striking coincidences is, in fact, that no such spread took place. “Parts of parts” and derived expressions are restricted to those very domains where their original practitioners employed them, using probably an idiom borrowed together with other professional instruments from the Near East. Other domains were not affected.

The above argument presupposes that diffusion took place, and that a channel for that diffusion has to be found. Caution requires, however, that this presupposition be itself examined critically. After all, “parts of parts” seems to be an idea close at hand. Everybody who understands the fractions will also understand their composition, we should think. Ascending continued fractions, furthermore, is a generalization of the metrological principle of descending subunits; any culture possessing a linearly ordered and multi-layered metrology should be able to invent them.

So it seems. But the actual evidence contradicts the apparent truisms. Greek Antiquity, though having demonstrably the schemes before its eyes, did not grasp at a notation which was so near at hand. It accepted the notation in a few select places – precisely the ones to where it can be assumed to have been brought. But the Greeks did not like it. For everyday use, they stuck to the Egyptian system; for mathematical purposes, they developed something like general fractions; and in astronomy, they adopted the Babylonian sexagesimal fractions.

The same holds for Latin Europe. The *Propositiones* became quite popular and influenced European recreational mathematics for centuries. But a 14th century problem coming very close to those dealing with *medietas et medietas medietatis* transforms this number into “$^{1}/_{2}$ and $^{1}/_{4}$”.[59] The usage “at hand” did not spread – on the contrary, it was resorbed.

The ascending continued fractions had a similar fate. As told above, they were taken over from Arabic arithmetic as an obligatory subject in Italian practical arithmetic from Fibonacci onwards without acquiring ever any importance. ⟦[60]⟧ Outside Italy, only Jordanus of Nemore tried to naturalize them as part of theoretical mathematics. He did so in his treatises on “algorism”, computation with Hindu numerals. For this purpose he invented a special concept “dissimilar fractions”. To explain what the concept was about he connected it precisely to systems of metrological subunits.[61] Not even his closest followers, however, appear to have found anything attractive in the idea, and no echo whatsoever can be discovered. Ascending continued fractions, no more than “parts of parts”, came naturally to the minds of medieval European reckoners and mathematicians.

59 Ms. Columbia X 511 A13, ed. [Vogel 1977: 109].

60 ⟦Article I.12, *passim*, discusses their occasional appearance in the 13th and 14th centuries.⟧

61 See the preface to *Demonstratio de minutiis*, ed. [Eneström 1913]. Cf. [Høyrup 1988: 337*f*].

If a concept cannot spread inside a given culture but remains restricted to a very particular use (ultimately to be resorbed) it is not likely to have been invented by this culture – at least not if there is no specific need for it in the context where it establishes itself. On this premise the "parts of parts" occurring in the *Anthologia graeca* and the *Propositiones* can safely be assumed to be there because of borrowings.

In the case of the *Anthologia*, as we have seen, the only conceivable source is Western Asia; as far as the *Propositiones* are concerned, the question of the direct channel is less easily decided. As we have observed, composite fractions are absent even from the problems inspired by the Eastern trade. Only one specific type of riddle employs them – a type which ultimately points toward ancient Egypt and not to the trading network ⟦(but see note 64)⟧. During the Achaemenid and Hellenistic eras, however, Egyptian and Western Asiatic methods and traditions had largely been mixed up. Even if the composite fractions of the *Propositiones* can ultimately be traced to Egypt, the road from Egypt to Aachen may therefore have passed through anywhere between Kabul and Seville.

Tracing the composite fractions of the *Anthologia* to the Semitic-speaking world of Western Asia and those of the *Propositiones* to Egyptian sources brings us back to the most intricate question: How did these (or, more precisely: the Babylonian and Egyptian usages) relate to each other?

We have found the traces of an Old Kingdom Egyptian as well as an Old Babylonian "folk" usage of elementary aliquot parts (including $^2/_3$). We have seen, moreover, that these were combined in both cultures into "parts of parts"; that they were expanded at least in Babylonia into a system of ascending continued fractions, and that they presumably provided Middle Kingdom Egypt with the foundation on which the full unit fraction system was built.

In principle, the Babylonian and Egyptian composite fractions may have developed in complete independence; two arguments, however, contradict this assumption. For one thing, "parts of parts" seem not to come naturally to an "average" culture, if we trust the Greek, Latin and Italian evidence. The Ancient Mesopotamian compositions appear, moreover, to be strictly bound to the Babylonian language. Third millennium Sumerian texts employ elementary unit fractions freely; but they never combine them as "parts of parts"; these, and the ascending continued fractions, only appear when mathematical traditions carried by the Babylonian language took possession of the scribal school in the Old Babylonian epoch. Shared origins or at least shared roots are thus more credible than full independence.

Shared origins are by no means excluded. Both the Semitic (including the Babylonian) and the Ancient Egyptian languages belong to the Hamito-Semitic language family. Furthermore, a socio-cultural need for simple fractions can reasonably be ascribed to the (presumably pastoral) carriers of the language before the Semitic and the Egyptian branch

broke away from each other.[62] Already at this early epoch, the habit of combining them as "parts of parts" may also have existed, even though the (scarce) comparative evidence suggests no need for such arithmetical subtleties in a non-monetary economy. Alternatively, diffusion of the habit via trade routes from one culture to the other at a later moment can be imagined: during the fourth as well as the third millennium BCE, connections existed, in all probability via Syrian territory.[63]

Yet, whether such commercial links were able to influence the development of arithmetical idioms is an open question. They may have involved a whole chain of intermediaries. An argument in favour of diffusion through trading contacts one way or the other (or from an intermediary) could be the common "institution" of recreational mathematics, which is not likely to have existed when the Semitic and Egyptian branches of the family separated (probably no later than the fifth millennium); but since Babylonian and Egyptian scribes have only the institution but no members (that is, problem-types) in common,[64] independent development of the recreational genre as a response to the

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62 See the table of shared vocabulary in [Diakonoff 1965: 42-49], and other shared vocables mentioned elsewhere in the book. Common property is, *inter alia*, the term *hsb*, "to count, "to reckon", "to calculate".

63 See [Moorey 1987] on the 4th millennium, and [Klengel 1979: 61-72] on the third.

64 ⟦Since this article was written, I have discovered one indubitably shared problem, close kin moreover of RMP problem N° 37: IM 53957, from Eshnunna and from c. 1775 BCE, published in [Baqir 1951: 37], with corrections and interpretation in [von Soden 1952: 52]):

> If [somebody] asks (you) thus: To $^2/_3$ of my $^2/_3$ I have appended 100 *silà* and my $^2/_3$, 1 *gur* was completed. The *tallum*-vessel of my grain corresponding to what?

I quote the discussion in [Høyrup 2002: 321]:

> The coincidences are too numerous to be accidental: firstly, there is the shared use of an ascending continued fraction; these are not only extremely rare in the rich Egyptian record, the RMP example appears to be the only ascending continued fraction occurring at all. To this comes the details of the dress: an unknown measure which is to be found from the process, the reference to a standard unit of capacity, and the notion of filling.
>
> The Egyptian problem is solved in agreement with the normal procedures of Egyptian arithmetic, in a way that depends critically on the fine points of the system of aliquot parts. The Eshnunna solution, on the other hand, is no solution at all but a sequence of operations which only yields the correct result because the solution has been presupposed – what 16th-century cossists would call *Schimpfrechnung*, "mock reckoning", a challenge meant to impress and make fools of the non-initiate. Problems of this type turn up regularly in medieval and Renaissance treatises on applied mathematics that draw directly on oral or semi-oral practitioners' traditions – exactly the traditions where rules for practical computation go together with mathematical riddles that seem to refer to practice but rarely have any practical application. Without pursuing the argument for the moment we may infer that the problem has its origin in a similar practitioners' environment (merchants?) in touch with both Egypt and Mesopotamia in the early second millennium, and that it was adopted by both Egyptian and Eshnunna scribes – in Eshnunna preserving the eristic

similar social environments of professional reckoners – that is, *shared* (sociological) *roots* of the genre – is an alternative explanation at least as near at hand as *shared origins* through common descent or through diffusion.

Similarly, *shared roots* (though linguistic or computational and not sociological) may be the better explanation that composite fractions are found in both Egypt and Babylonia. As one will remember, the objection against fully independent development of systems of composite fractions was founded on the observation that the creation of a scheme of "parts of parts" is *not* near at hand, in spite of what might look like reasonable a priori expectancies. Strictly speaking, however, this observation was only made on a *Greek*, *Latin*, or *Italian* linguistic background and on the background of the computational techniques and tools in common use in classical Antiquity and medieval Europe. But developments in Egypt and Babylonia will not have been fully independent: they will have taken place on structurally similar linguistic backgrounds, and maybe on the background of shared techniques and tools. A common heritage of Babylonians and Egyptians could be a set of elementary fractions and a pattern of linguistic or computational habits being naturally open to specific developments – in particular the development of a scheme of "parts of parts".[65] This would be *parallel developments from shared roots*.

Summing up we may conclude with a high degree of certainty that later occurrences of "parts of parts" and ascending continued fractions outside the Egypto-Semitic area are due to borrowings from developed usages (in some cases distorting or rudimentary borrowings). We may also assume that the parallel Semitic and Egyptian idioms can be ascribed to a shared heritage. But we cannot know whether the shared heritage was an

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form and purpose, in Egypt transformed into "good mathematics".

This eliminates the need to discuss from where the Egyptian scribe got the idea of an ascending continued fraction. The discussion of the character of possible shared roots in this and the next two paragraphs thus only concerns the Egyptian use of "parts of parts".

It also changes the way we should look at the problems in the *Propositiones* that speak in terms of composite fractions. They are indeed as close to the Eshnunna- as to the RMP-problem, and thus to their common (Syrian?) recreational root.⟧

[65] In his book [1965] on the Hamito-Semitic language family, Igor M. Diakonoff mentions many instances where different languages of the family have developed similar features independently; thus as complex a phenomenon as the *pluralis fractus* (p. 68). We might speak of "structural causation", the effect of shared linguistic structures determining that specific developments are near at hand and compatible with general linguistic habits.

"Structural causation", however, need not be linguistic. Non-linguistic instruments for accounting and computation (be they mental or material) may in the same way open the way for specific inventions and block others which are not compatible with existing habits, tools or conceptualizations.

Knowledge of the way fractions are spoken about in other Hamito-Semitic languages might seem to offer a way to distinguish linguistic from non-linguistic causation. However, native and ethnically conscious Berber speakers studying mathematics whom I interviewed in Algeria confessed to speak about fractions in Arabic and to be ignorant of any Berber idiom for fractions.

actual way to speak about fractional entities or *only a potential scheme* inherent in language structures or computational practices. Personally, I confess to be inclined toward belief in the potential scheme.

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Chapter 3 (Article I.2) A Note on Old Babylonian Computational Techniques

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J. Høyrup, *Selected Essays on Pre- and Early Modern Mathematical Practice*,
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Abstract

Analysis of the errors in two Old Babylonian "algebraic" problems shows

(1) that the computations were performed on a device where additive contributions were no longer identifiable once they had entered the computation;

(2) that this device must have been some kind of counting board or abacus where numbers were represented as collections of calculi;

(3) that units and tens were represented in distinct ways, perhaps by means of different calculi.

Contents

Observations and conclusions

It has been known for more than a century that Babylonian calculators made use of tables of multiplication, reciprocals, squares, and cubes. It is also an old insight that such tables alone could not do the job – for instance, a multiplication like that of 2 24 and 2 36[1] (performed in the text VAT 7532, obv. 15, ed. [Neugebauer 1935: I, 294]) would by necessity require the addition of more partial products than could be kept track of mentally, even if simplified by means of clever factorizations. It has therefore been a recurrent guess that the Babylonians might have used for this purpose some kind of abacus – Kurt Vogel [1959: 24] also pointed to the possibility that the creation of the sexagesimal place value system might have been inspired by the use of a counting board.

Denise Schmandt-Besserat's discovery [1977] of the continuity between an age-old accounting system based on clay tokens and the earliest cuneiform writing could only give new life to such speculations, the fullest development of the argument being probably [Waschkies 1989: 84*ff*]. Unfortunately, neither material finds nor texts could transform the speculations into something more substantial – as pointed out explicitly by Waschkies [1989: 85], no document was known at the time which contained intermediate calculations or which told in clear terms how they were made. Moreover, the indirect support seemingly offered by lexical lists and cited by Waschkies has dissolved into nothing in the meantime – cf. below, note 10.

Old Babylonian documents containing "rough work" were only identified by Eleanor Robson ([1995], republished in final form as [Robson 1999]; further examples, for example, in [Robson 2000]).[2] What we learn from these is, however, that calculations whose result could not be found by mental calculation (after adequate training) were performed in a different medium; thus, the tablet UET VI/2 222 states directly (and correctly) that 1 03 45 times 1 03 45 (expressed by the writing of one number above the other) is 1 07 44 03 45. Since none of the round tablets discussed by Robson contains the details of such calculations, we must presume that they were not made in clay. How they were then made remains an open question.

Fortunately, not all calculators are equally precise, and calculational errors in the sources may often be as informative about the process in which they were produced as those made in the class-room may be about the way school children think about mathematical objects. Errors contained in two (equally Old Babylonian) texts belonging

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1 Since the present discussion regards calculation within the sexagesimal floating-point place value system, I render the numbers without any indication of a presumed absolute order of magnitude. 2 24 thus stands for $2 \cdot 60^n + 24 \cdot 60^{n-1}$, where n can be any integer.

2 The Old Babylonian period lasts from 2000 BCE to 1600 BCE in the currently used "middle chronology".

to the so-called "algebraic" genre turn out to shed some light on the nature of the devices of which their authors made use.

1 57 *21* 40
1 57 ***46*** 40
excess ***25***

Box 1

The first is problem no. 12 of the tablet BM 13901 (obv. II, lines 27–34, ed. [Neugebauer 1935: III, 3]). Line 29 asks for the multiplication of $10'50''$ by $10'50''$,[3] and line 30 states the result as $1'57''46''40''''$ – wrongly, indeed, the true answer being $1'57''21'''40''''$ – see the first box. Since the erroneous result is used further on, it must be due to the author of the text, not to a copyist. The computation can be made in many ways, but if a contribution 25 shall be produced by a single error, the one actually used must involve a multiplication $5{\cdot}5$. The only simple method which does this is a calculation which makes use of partial products and which transforms the partial product $50''{\cdot}50'' = (5{\cdot}10'')\cdot(5{\cdot}10'') = (5{\cdot}5)\cdot(10''{\cdot}10'') = 25{\cdot}1'''40'''' = 25'''+16'''40''''$ – see box 2.[4]

10 50 × 10 50	
01 40	(10×10)
08 20	(10×50)
08 20	(50×10)
25	(25×1)
25	(*ditto*)
16 40	(25×40)
01 57 46 40	

Box 2

This does not inform us about the tool on which the computation was performed, but is interesting in itself. The use of a factorization $50 = 5{\cdot}10$ agrees well with what is known from other sources – for instance, from the tablets for rough work published by Eleanor Robson and from the "algebraic" transformations performed in the same text, for instance, in problem 14. But factorization only intervenes when the answer cannot be given immediately. We thus discover that the author of the tablet knew by heart (or because he had just made use of it for the determination of the first partial product) that $10{\cdot}10 = 1\ 40$; but he seems not to have known by heart that $50{\cdot}50 = 41\,40$. (Since the details of the computation are not presented in the text, a pedagogically motivated detour can be safely

[3] Actually the text asks for the laying-out of a rectangle with these sides and the ensuing determination of the area. However, the present inquiry concerns only the numerical aspect of the question, for which reason it will be convenient to disregard the geometrical setting.

Since the analysis requires that the relative order of magnitude of members be kept clear, from this point onward I make use when adequate of Thureau-Dangin's extension of the degree-minute-second notation, in which, for instance, $10'50''$ stands for $\frac{10}{6}+\frac{50}{60{\cdot}60}$. It should be kept in mind that the tablet contains no similar indications.

[4] Additive splitting into a more complex sum may evidently also produce a contribution of 25. If we split $50''$ into $45''+5''$, $50''{\cdot}50''$ can be determined as $45''{\cdot}45'+2{\cdot}45''{\cdot}5''+25''''$, which unfortunately is in a wrong order of magnitude. In order to obtain a contribution of 25 in the right order of magnitude we have to make the transformation $10'50''{\cdot}10'50'' = (5'+5'+45''5'')\cdot(5'+5'+45''5'')$. Both transformations are laborious detours, and no reason can be imagined that the Babylonian calculator would take any of them into account.

excluded.) We may also suspect that he did not know immediately that $10{\cdot}50 = 8\ 20$, since this transformation might have been used to compute $50''{\cdot}50''$ as $(5{\cdot}10''){\cdot}50'' = 5{\cdot}(10''{\cdot}50'') = 5{\cdot}8'''20'''' = 40''' + 1'''40'''' = 41'''40''''$; however, predilection for symmetry (which is visible elsewhere in the text) might have induced him to use the symmetrical procedure for $50''{\cdot}50''$ instead of this shortcut.[5]

The fact that the contribution $25'''$ is added twice does tell us something about the calculational tool. Omission of a contribution can occur in almost any kind of device. Insertion twice instead of once, on the other hand, is next to excluded if the single contributions remain visible to the reckoner, as in our paper algorithms (anybody going from paper to a pocket calculator will have experienced the unpleasant change on this account). We must therefore conclude that our Old Babylonian calculator operated in a medium where at least additive contributions were no longer identifiable once they had entered the computation – as in the medieval dust abacus or on a counting board, but not in the paper algorithms that were developed by the late medieval *maestri d'abbaco* and which are still with us.

3 39 28 4*3* 2***7*** 24 26 40	true result
3 39 28 4***4*** $\underline{2\boldsymbol{6}\ 24\ 26}$ 40	first error
3 39 28 44 $\underline{26}$ 40	second error

Box 3

Further information is obtained from the second problem of the text TMS XIX (rev., lines 1–12, ed. [Bruins & Rutten 1961: 103, pl. 29]).[6] In line 4, the square on $14'48''\,53'''20'''$ is determined as $3'39''[28''']44'''\ 26^{(5)}40^{(6)}$,[7] and not as $3'39''28'''43''''\ 27^{(5)}24^{(6)}26^{(7)}40^{(8)}$. Two errors appear to have been committed (cf. Box 3): first, 43 27 has become 44 26; next, the

5 *If* he did not remember that $50{\cdot}10 = 8\ 20$, the scribe might have found it easily either by factorization, as $(5{\cdot}10){\cdot}10 = 5{\cdot}(10{\cdot}10) = 5{\cdot}(1\,40)$, or from an additive splitting of 50, as $(5{\cdot}10){\cdot}10 = (20{+}30){\cdot}10$. Both calculations, by the way, give the result as a sum 5+(3 20). He could also have taken advantage of the fact that $50 = (1\,00) - (00\,10)$, which would have given the result as $(10\,00){-}(1\,40)$. All three procedures correspond to tricks that are used elsewhere in the corpus. In any case, the absence of errors prevents us from knowing what he did.

6 In the problem, the area of a rectangle is given together with the area of another rectangle, whose length is the cube on the length of the first rectangle, and whose width is the diagonal of the first rectangle. This is a problem of the eighth degree, which is solved correctly (apart from the calculational errors which are discussed below) as a bi-biquadratic.

7 Bruins's transliteration has «3.39.2[8.43.27]⟨24⟩.26.40», but Ruttens's hand copy shows that there is space for nothing more than 28, and that the presumedly missing «24» is present but as «44»; moreover, the number is repeated in line 7 as «$3'39''28'''44''''26^{(5)}[40^{(6)}]$».

There is no photo of the tablet in the edition, which means that this is one of the tablets that were mislaid by the Louvre after having been hidden away in the late thirties because of fear of imminent war [Jim Ritter, personal communication]; Bruins will therefore have had to make his transliteration from the hand copy, and cannot have improved the readings of the latter after collation.

repetition of «26» (in both cases preceded by «4») has made the calculator change "«44 26 24 26 40» into «44 26 40». Since the number that is produced is used further on, even the latter error must have been committed by the original calculator when transferring it from a separate device on which it had been found. The first error – the misplacement of a single unit in a wrong order of magnitude – implies that numbers were represented as collections of units in the calculational device, in the manner of calculi placed on a counting board, and not as the written numbers on a dust abacus.

In lines 6–7, $11''6'''40''''$ is added to the number $3'39''[28''']44''''26^{(5)}40^{(6)}$,[8] and the result is stated to be $3'50''36'''43''''34^{(5)}26^{(6)}40^{(7)}$ instead of $3'50''35'''24''''26^{(5)}40^{(6)}$. This error is more complex, and since the number is not used further on[9] we cannot know whether a copyist's error (or an unsuccessful copyist's attempt to repair a recognized error) has been superimposed upon an original calculator's error. It seems, however, that a unit has been misplaced in the order of fourths instead of that of thirds; besides, two tens have been added wrongly to the fourths, and an extra place 34 inserted after the fourths – cf. Box 4.

Since misplaced units appear not to turn up as tens, it is likely that counters for units and tens were different (as are the corresponding cuneiform signs, and as would also be expected if the preliterate accounting system had provided the original inspiration); alternatively, a counting board may have been in use where cases (or carved grooves, or whatever was used to keep together counters that belonged together) for units and for tens were clearly distinguished but cases for units in neighbouring orders of magnitude were so spatially close that single counters could be mislaid or pushed accidentally from one to the other.

This is as far as the errors can bring us. Other textual evidence is at best ambiguous.[10] Archaeology only tells us that the implements in question either were made of

[8] Bruins gives the number as «3.39.28.44.26.24.[26.40]»; however, according to the hand copy only «3.39.28.44.26» is legible, and the final lacuna has space for only one sexagesimal place – i.e., exactly for the number found in line 4. Bruins has evidently reconstructed with an eye to the correct value.

[9] Its square root is taken in lines 8–9, but the stated value «$15'11''6'''40''''$» is obviously found from the known end result, which is the reason that the solution can be stated correctly. The same, by the way, happens in BM 13901 no. 12.

[10] Stephen Lieberman [1980, 346*f*], it is true, supposed to have found terms in lexical lists that designated wooden accounting and calculational devices. His key piece, however, was a first millennium version of one such list; the Old Babylonian version of the same list which has been found in the meantime does not corroborate his interpretations (Eleanor Robson, personal communication).

However, combination of terminological evidence with other types of errors than those analyzed here has allowed Christine Proust [2000] to infer both the number of columns in the device and

perishable materials or were of a type that has not allowed archaeologists to identify their function. It may be added that the use of a counting board would explain the rarity of mistaken place ascriptions in the mathematical texts, for instance in the addition of multiplace numbers. Mistakes are, indeed, much less common than could be expected if absolute orders of magnitude were to be kept track of mentally, without any material support.

It may also be added, and should be emphasized, that all conclusions drawn above were based upon Old Babylonian material. There is no certainty that similar techniques were used in the first millennium BCE. By then, the wax tablet had come into use. A dust abacus is also likely to have been employed, as revealed by the Greek name for the abacus (αβαξ): as first pointed out by Nesselmann [1842: 107 n.5], it is a West Semitic loan word, derived from a verb meaning “to fly away” and/or from a cognate noun meaning “light dust.”⟦[11]⟧

3 50 35 2***4***	26 40	true result
3 50 3***6*** <u>2</u>***3***	26 40	first error?
3 50 36 <u>4</u>3	26 40	second error?
3 50 36 43 34 26 40		third error?

Box 4

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its name. To some (but only some) extent Proust takes up an idea which was suggested by Jöran Friberg [1990: 536] in 27 words but to my knowledge never pursued by him.

[11] ⟦When Christine Proust saw the preprint of this article, it immediately spurred her to make the discovery mentioned in note 10 – namely that the device, called “a hand”, contained four or five sexagesimal places, and that calculations involving more places asked for the combination of several of these (leading to those errors which allowed her conclusion). Too scrupulous, she refused to include evidence I then suggested to her that the name was in use as early as the 26th century BCE and as late as the fifth century BCE “because it was mine”; instead I have to refer to [Høyrup 2002] and [Høyrup 2009].

The fifth-century evidence belongs with astronomer-scribes; it is therefore still quite possible that non-scholarly, Aramaic-speaking calculators used a dust abacus.⟧

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Chapter 4 (Article I.3) On a Collection of Geometrical Riddles and Their Role in the Shaping of Four to Six "Algebras"

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧
Translations, if not otherwise identified, are mine

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Abstract

For more than a century, there has been some discussion about whether medieval Arabic *al-jabr* (and hence also later European algebra) has its roots in Indian or Greek mathematics. Since the 1930s, the possibility of Babylonian ultimate roots has entered the debate. This article presents a new approach to the problem, pointing to a set of quasi-algebraic riddles that appear to have circulated among Near Eastern practical geometers since c. 2000 BCE, and which inspired first the so-called "algebra" of the Old Babylonian scribal school and later the geometry of *Elements* II (where the techniques are submitted to theoretical investigation). The riddles also turn up in ancient Greek practical geometry and Jaina mathematics. Eventually they reached European (Latin and *abbaco*) mathematics via the Islamic world. However, no evidence supports a derivation of medieval Indian algebra or the original core of *al-jabr* from the riddles.

Contents

Tilegnet CARL BERTIL som velkomst

Opening

Scholars are human beings; in particular, we are readers and writers. As human beings we partake in the general weaknesses of the human race, one of which is the tendency to believe that the kind of thing *we* are doing is of extraordinary importance, for good or for evil; our particular dependence on the written word therefore often induces us to ascribe more absolute importance to this variety of *logos* than warranted – not least to the written words of identifiable scholars.

When historians of mathematics observe that closely related or identical problems turn up in the Chinese *Nine Chapters on Arithmetic* and in Diophantos' slightly later *Arithmetica,* the question that spontaneously presents itself is whether Diophantos learned from the Chinese work, or whether the Chinese author had access to a lost work written by a Greek mathematician and later also used by Diophantos. Even the very different methods used to solve problems in the Chinese and the Greek works will not automatically cast doubt on the basic model according to which knowledge stored in book form derives from other knowledge stored in a similar form.

It is my claim that such conclusions are not only arbitrary, but often demonstrably wrong, and that in general the knowledge of pre-modern practical professions was not transmitted in books that we know about. The knowledge in question may have been regarded as craft secrets; but if so, we may be fairly sure that these secrets were not considered to be esoteric knowledge.

The notion of "practical professions" covers many diverse activities with appurtenant domains of knowledge at least since the great Bronze Age cultures. *Mutatis mutandis,* much of my general thesis will arguably hold broadly, but I shall concentrate on mathematical practice. For brevity, I shall permit myself to speak of the practitioners as members of "professions", "professional groups", and "crafts" as if these were well-defined entities. The reader should keep in mind, however, that we rarely possess much information about the precise social organization of carpenters, glass-makers, surveyors, etc. All we know for sure is that they were specialists and must have possessed *some* social organization that was able to carry their professional knowledge and activity.

Even "mathematical practice" is far from uniform, but it always tended to be the preserve of the few to a much larger extent than implied by the pervasive reference to "popular traditions". The mathematical techniques and knowledge of such professionals as surveyors, architects, tax officials, and accountants were certainly no less practical secrets than the knowledge of a modern engineer or computer technician, and had no need of being understood as esoteric in order to deserve that status.

A General Characterization

Before investigating a particular case, I shall summarize some of the general characteristics of the mathematical knowledge of pre-Modern practical professions; for fuller treatment and exemplification I shall refer to earlier publications of mine.[1]

Not least when mathematical practice is concerned will it be important to distinguish two (ideal) types: one carried by a school institution, and one taught "on the job" in a master-apprentice-system. Both types, of course, find their ultimate legitimacy in the world of *know-how,* not that of *know-why* (the world of "productive", not "theoretical knowledge", in Aristotle's terminology). But a knowledge system, if existing as a system, has to be taught and not only to be used; furthermore, inasmuch as it is carried by a particular social group it will also serve to identify this group. Both in their relation to teaching and with regard to professional identification, the "scholasticized" and the apprenticeship-based systems differ fundamentally. We shall start by looking at the latter type, for which I have suggested the label "sub-scientific" rather than "popular" or "folk"; afterwards, we shall confront the transformation that practitioners' knowledge undergoes when transmitted through a school.

In the pre-Modern world, proper schooling was mostly the privilege of the few. Some members of crafts who did not transmit their knowledge through a school system may still have been literate or semi-literate; as a rule, however, the *culture* of such crafts will have been of the oral type.[2] A first and obvious effect of this is that we have no direct sources for their knowledge, only indirect references and a small number of works written by authors actively engaged in transferring the knowledge in question from the orally based to the literate domain. Another effect of the underlying oral culture, of no less consequence for the present study, is the creation of a characteristic type of "supra-utilitarian" knowledge.

The larger part of the fund of knowledge of a practitioner's craft is evidently meant to serve its daily routines. At this level, the technical problems encountered every day are primary, and the methods ancillary, invented in order to solve these problems. Teaching already offers a slightly different case: here, the transmission of methods is primary, and some tasks may serve simply to teach the methods. But as long as teaching is effected as *training of apprentices* and thus embedded in the activity of a "shop", there is a natural tendency to focus training as much as possible on practically (that is, economically) useful tasks. No strong incentive for going beyond the utilitarian level is likely to present itself in the context of training.

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[1] In particular [Høyrup 1990b; 1997b].

[2] For analysis of the distinctive characteristics of this cultural type, one may consult various writings of Walter Ong (e.g., [1967; 1982] and Jack Goody (e.g., [1968; 1977; 1987]).

The supra-utilitarian level, instead, is important for the function of the knowledge system as an ingredient of craft identity. Knowledge that identifies the craft must certainly be, *or in any case appear to be,* relevant for the activity of the craft. But at least for members of the craft, familiarity with the all-too-familiar basic professional tools is not sufficient to support professional pride. For this purpose, *virtuosity* in the handling of the tools is needed; this is displayed as the ability to deal with problems that are more difficult than those encountered in everyday practice but which are still related in kind to utilitarian tasks, that is, which can be characterized as *supra-utilitarian.* In mathematics, these problems are known as *recreational problems.*

Since the function of the supra-utilitarian knowledge is to display proficiency in the handling of methods, the methods are primary and the problems whose solutions they serve become secondary on this level of knowledge. In this respect, recreational problems belong to the same breed as the Eiffel Tower, built with the sole purpose of showing what miracles late 19th-century engineering could perform in iron constructions. Further, since this function is accomplished within an oral culture, they take on the same form as other tests of the right to enter a specific place, environment, or group: They are *riddles,* but certainly meant not for entertainment but as challenges'[3] – less deadly than the riddle of the Sphinx, but belonging to the same kind as this "neck riddle".

Qua riddles, the "recreational problems" share several features with other riddles. First of all, the formulation of a recreational problem is always somehow striking, as it befits a riddle – at times it is directly absurd. This depends on two factors: on one hand, the question should kindle interest or curiosity in order to serve as a worthwhile challenge; on the other, a formulation which is not stunning will easily mutate during oral transmission, but only until the point when somebody finds a formulation which *is* stunning – for instance, that one hundred monetary units buy exactly one hundred animals; then the formulation freezes.

Next, it is only true to a certain point that the problems are determined from the stock of available methods. At times, a solution presupposes the application of a particular trick that has no practical use, but which of course, when handed down together with the riddle, becomes part of the stock of available techniques known by members of the profession. An example that we shall encounter below is the quadratic completion of mixed second-degree area problems.

At times, the trick is mathematically wrong, and the problem possesses no proper mathematical solution at all (a feature which is particularly puzzling if we try to understand the problem as *mathematics* in the proper sense). What is requested is in fact an answer in agreement with the established canon of the group, as proof of familiarity with this canon – and only because the group is demarcated by exercising a mathematical practice

[3] See, beyond the references in n. 2, [Koch 1992] and [Pucci 1997: 59–63 and *passim*].

will this canon *on the whole* but not exclusively be mathematical. This sometimes oblique character of the answer is no different from what we encounter in other riddles, the solutions to which are rarely straightaway answers – we may think again of the riddle of the Sphinx, the answer to which transposes "morning", "noon", and "evening" into "childhood", "adulthood", and "old age".

In an institutionalized school, teaching is taken care of by teachers who have this chore as their main activity, not genuine professional use of the tools that they transmit. The work of students, on its part, is of no direct use; it serves training purposes only. For both reasons, even the training of elementary abilities may make use of problems that are not directly relevant for professional practice, and are thus secondary with respect to the methods they should teach (for reasons of pedagogical efficiency, training will still be based on *problems,* not on the explanation of theory or abstractly formulated rules); in this respect they will be similar to the recreational problems. But whereas riddles tend to exist in one or a few authorized versions, the number of similar elementary training problems in a school will tend to grow by simple variation, in which process any striking character of problems is easily lost.

Genuine "recreational" problems still have their place in school teaching; they provide some diversion from the tedium of simple variation of elementary training problems, as argued by Pier Maria Calandri in his 15th-century *Tractato d'abbacho* [ed. Arrighi 1974: 105]:⟦[4]⟧

> I believe for certain that the human intellect, using always the same thing, may occasionally be disgusted with it however enjoyable it be; and in order not to end up in this trouble in the beginning of our working, in the present chapter some pleasant problems will be dealt with".

It is thus precisely in the school context (and in literate use) that the "recreational" problems come to deserve their name. Even the recreational problems, however, will be affected by the school context, and tend to appear in many variations. This may be illustrated by the way the Italian *scuola d'abbaco* presents us with the problem of the "hundred fowls": both the quantity of money and the number of animals may vary. (Below, the transformation of the "surveyors' riddles" into "algebra" will provide us with a historically more consequential example.)

If one school tradition borrowed directly from another, there would be no particular reason that precisely the striking version should be the one that was invariably shared. The fact that the various school traditions have, for instance, precisely "100 animals for 100 units" in common demonstrates that the diffusion has taken place at the level of oral

[4] ⟦Arrighi's ascription is mistaken, the author of the treatise is Benedetto da Firenze, see [Van Egmond 1980: 96].⟧

riddles, from which all the school traditions from "Ireland to India"[5] have taken their inspiration, and which has provided the starting point for the particular pattern of variation of the single school system (for instance, that the Indian version normally involves four instead of three species).

"The Four Fronts and the Area" and its Kin

The rest of the article will investigate a case of particular importance for the development of high-level mathematics in a variety of cultures and thereby – this is the intention – illuminate the applicability of the preceding general statements.[6] The starting point is problem N° 23 from the Old Babylonian text BM 13901 [ed. Neugebauer 1935: III, 4*f*]:[7]

> ⟨If somebody asks you thus⟩ about a surface: the four fronts and the surface I have accumulated, 41′40″.
> 4, the four fronts, you inscribe. The reciprocal of 4 is 15′.
> 15′ to 41′40″ you raise: 10′25″ you inscribe.
> 1, the projection, you append: along 1°10′25″ (laid out as a square area), 1°5′ is equal (as side).
> 1, the projection, which you have appended, you tear out: 5′ to two
> you repeat: 10′ NINDAN confronts itself.

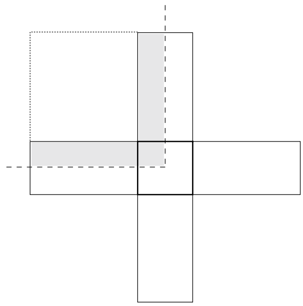

Figure 1. The method of BM 13901 No. 23.

The diagram illustrates the way the problem is solved:[8] The four "fronts" or sides (s) are represented by four rectangles ⊏⊐(1,s) which "project" or stick out from the

5 A chapter heading from Stith Thompson's renowned book [1946: 13] on another aspect of oral culture.

6 The basis for the case is presented in [Høyrup 1996a]. Unfortunately the publishers of the volume (the Mathematical Association of America) supposed proof reading to be superfluous, for which reason at least my contribution abounds with printing errors, mostly due to faulty computer conversion and ensuing equally poor repair. A somewhat different presentation in French is [Høyrup 1995], which however (for identical reasons) is even more defective (some 12 errors per page, including an omitted passage of 457 words); the latter version was republished in corrected form as [Høyrup 1996b].

7 My translation from the original language. I follow Thureau-Dangin's degree-minute-second notation for sexagesimal numbers; 1°10′25″ thus stands for $1+{}^{10}/_{60}+{}^{25}/_{60\cdot60}$.

8 The reasons that the Old Babylonian 'algebra' texts must be understood in agreement with their geometrical wording (and lengths and areas thus not as metaphors for abstract numbers and their products) are set forth in detail in [Høyrup 1990a]. ⟦A more mature treatment of the question is [Høyrup 2002].⟧

square surface $\square(s)$;[9] the total area of this configuration is hence $41'40''$. A quarter of this configuration (shaded in the diagram) is a gnomon with area $10'25''$; it is completed by adjunction of a square $\square(1)$ as another square $1°10'25''$, whose side is $1°5'$. Removing the adjunction we are left with $5'$ as the halved side of the original square, which is doubled and then told to be $10'$ *nindan* (1 *nindan* $\approx$ 6m).

All problems on the tablet deal with one or more squares – in the terminology introduced by Jöran Friberg, the text is a "theme text". In symbolic translation, they may be represented thus (Q stands for the quadratic area, s for the side; the unit is always understood to be the *nindan*):

1. $Q+s = 45'$
2. $Q-s = 14^{\backprime}30$
3. $Q-\frac{1}{3}Q+\frac{1}{3}s = 20'$
4. $Q-\frac{1}{3}Q+s = 4^{\backprime}46°40'$
5. $Q+s+\frac{1}{3}s = 55'$
6. $Q+\frac{2}{3}s = 35'$
7. $11Q+7s = 6°15'$
8. $Q_1+Q_2 = 21'40''$, $s_1+s_2 = 50'$ (reconstructed)
9. $Q_1+Q_2 = 21'40''$, $s_2 = s_1+10'$
10. $Q_1+Q_2 = 21°15'$, $s_2 = s_1-\frac{1}{7}s_1$
11. $Q_1+Q_2 = 28°15'$, $s_2 = s_1+\frac{1}{7}s_1$
12. $Q_1+Q_2 = 21'40''$, $\sqsubset\!\sqsupset(s_1,s_2) = 10'$
13. $Q_1+Q_2 = 28'20''$, $s_2 = \frac{1}{4}s_1$
14. $Q_1+Q_2 = 25'25''$, $s_2 = \frac{2}{3}s_1+5'$
15. $Q_1+Q_2+Q_3+Q_4 = 27'5''$, $(s_2,s_3,s_4) = (\frac{2}{3},\frac{1}{2},\frac{1}{3})s_1$
16. $Q-\frac{1}{3}s = 5'$
17. $Q_1+Q_2+Q_3 = 10^{\backprime}12°45'$, $s_2 = \frac{1}{7}s_1$, $s_3 = \frac{1}{7}s_2$
18. $Q_1+Q_2+Q_3 = 23'20''$, $s_2 = s_1+10'$, $s_3 = s_2+10'$
19. $Q_1+Q_2+\square(s_1-s_2) = 23'20''$, $s_1+s_2 = 50'$
20. [missing]
21. [missing]
22. [missing]
23. ${}_4s+Q = 41'40''$
24. $Q_1+Q_2+Q_3 = 29'10''$, $s_2 = \frac{2}{3}s_1+5'$, $s_3 = \frac{1}{2}s_2+2'30''$

N° 23 is the only conserved problem that indicates the unit, but this is merely one of many features that set it apart. It is found in a part of the text where all other problems treat of several squares; it is the only square problem in the whole Old Babylonian corpus to speak of the sides before the area (and to use a specific phrase that points to *the* four sides, rendered symbolically as ${}_4s$); with one exception to which we shall return it is alone in speaking of the side of the square as a "front" (moreover, it does so in syllabic Akkadian, whereas normal "algebra" texts using this term about the short side of a rectangle invariably use a Sumerian word sign[10]); and it uses a trick which depends

9 The "projection" is that breadth 1 which transforms a line of length s into a rectangular area with the same numerical value.

10 Sumerian had been the dominant language in southern Iraq in the third millennium BCE; Akkadian, a Semitic language (which is split into a Babylonian and an Assyrian dialect in the second millennium), was present in the area already by then and was written from c. 2500 BCE onward; in the second millennium, it was the main spoken and written language of Babylonia. In Sumerian, semantic cores were mostly written by means of word signs, whereas grammatical complements had to be expressed in syllabic writing. In principle, Akkadian is written syllabically, but Sumerian

critically on the “coefficient” 4, whereas all other solutions (except numbers 9 and 10, where the question does not present itself) are obtained by means of coefficient-independent techniques. The problem is indeed so aberrant that Neugebauer proposed it to be the outcome of a scribal confusion that happens to make mathematical sense.

When Neugebauer did so, nobody had observed that the problem is quite widespread and therefore not to be explained away; it is indeed as characteristic and hence as good an index fossil as are trilobites for the Cambrian and immediately following periods in palaeontology. The next occurrence which I know of is no. 3 in a treatise which Heiberg included as chapter 24 in the second-order conglomerate known as *Geometrica* and found in the Codex Constantinopolitanus Palatii Veteris N^{o} 1 [ed. Heiberg 1912: 418]:[11]

> A square surface having the area together with the perimeter of 896 feet. To get separated the area and the perimeter. I do like this: In general [καθολικῶς, i.e., independently of the parameter 896 – JH], place outside [εκτίθημι] the 4 units, whose half becomes 2 feet. Putting this on top of itself becomes 4. Putting together just this with the 896 becomes 900, whose squaring side becomes 30 feet. I have taken away underneath [ὑφαιρέω] the half, 2 feet are left. The remainder becomes 28 feet. So the area is 784 feet, and let the perimeter be 112 feet. Putting together just all this becomes 896 feet. Let the area with the perimeter be that much, 896 feet.[12]

The *Liber mensurationum* written by an otherwise unidentified Abū Bakr[13] and known

word signs may be used for the roots of words (and may or may not be provided with phonetic complements, at times instead with Sumerian grammatical complements). In mathematical texts, a few terms (the length, width, and area of “abstract” rectangles, square and cube roots, the terms for the reciprocal and for multiplication of number with number) are invariably written with Sumerograms – roots and reciprocal sometimes in syllabic writing *of the Sumerian term,* obviously used as a loan word; most terms, however, in particular the characteristic vocabulary of the “algebra” and the length and width (“front”) of real fields, are often written in syllabic Akkadian.

[11] This manuscript (S) also contains Heron’s *Metrica* and one of the two main constituents of Heiberg’s construction (the other being constituted by mss A+C); however, chapter 24 is an independent piece. Both S and A+C are already composite entities; they are analyzed in Høyrup 1997a ⟦= article I.9⟧ together with the second-order conglomerate which Heiberg [1912] produced from them. As already pointed out by Heiberg [1914: xxi], the collections which he combines into one pseudo-Heronian “bulk” (*moles*) are of disparate character, and are “not made by Heron, nor can a Heronian work be reconstructed by removing a larger or smaller number of interpolations”.

[12] My diagram follows the description of the procedure as given in the text, including the orientation; the diagram in the manuscript only shows the square.

[13] The name does not help us to identify the author; since Abū Bakr was the name of the first caliph, it is simply too common (so to speak the Giovanni/Jean/Hans/Juan/John of Sunni Islam). Since the treatise is characterized by a conscious effort to merge different traditions (cf. n. 14), it is not likely to be much earlier than the early ninth century CE; certain features of the terminology, on the other hand, will have been archaic (but not impossibly archaic) if the treatise postdates al-Khwārizmī’s *Algebra* considerably. Whatever the actual date of composition, it reflects the status

from Gerard of Cremona's Latin translation is also of interest. Its problem 4 – dealing with an "equilateral and right quadrangle" – looks as follows [ed. Busard 1968: 87]:[14]

> And if he [a "somebody" introduced in N° 1] has said to you: I have aggregated its 4 sides and its area, and what resulted was hundred and 40, then how much is each side?
>
> The working in this will be that you halve the sides which will be two, thus multiply this by itself and 4 results, which you add to hundred and ⟨forty, and what results will be hundred and⟩ 44, whose root you take which is 12, from which you subtract the half of four, what thus remains is the side which is ten.

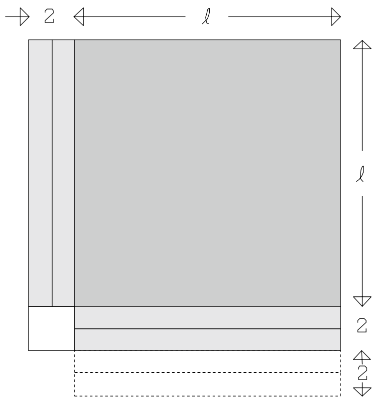

Figure 2. The method described in *Geometrica* **24**.3.

In Savasorda's early 12th-century *Liber embadorum,* the following is one of a group of problems on squares and rectangles that (as Savasorda tells) serve no practical purpose but will allow the reader to display his ability [ed. Curtze 1902: 36]:

> If, in some square, when to its surface is added the sum of its four sides, you find 77, how many cubits are contained in the surface? Taking the half of its sides, which is two, and multiplying it with itself, you find 4. If you add this to the given quantity, you will have 81; when you take its root, which is 9, and when you subtract from this the half of the addition that was mentioned already, 7 remains. This is the side of the square in question, whose surface contains 49.

Not only the numerical values but also comparison of the surrounding sequence of problems with the corresponding sequence in the *Liber mensurationum* shows beyond reasonable doubt that Savasorda does not draw directly on Abū Bakr.

Leonardo Fibonacci, in the *Pratica geometrie,* has the same numbers as Abū Bakr; but he "normalizes" the order of area and sides so as to make it coincide with the algebraic canon (the problem serves as his paradigm for the type "square plus sides equal number"), and like the Greek specimen he wants to "separate" the constituents of the sum [ed.

of c. 800 CE.

[14] Not in the present case but for many of the other problems, Abū Bakr offers alternative solutions by means of *al-jabr.* The synthesis of two approaches is typical of what happened in Islamic mathematics in the ninth century and a main reason not to date Abū Bakr much before that epoch; but apart from that the *al-jabr* solutions are not relevant in our present context. As Abū Bakr sees it, the staple method for problems of this type is evidently the one he uses in the present case. He only presents the solutions as algorithms without argument, but a couple of erroneous shortcuts (in N^{os} 38 and 46) show that the procedures presuppose a geometry of the kind shown in Figure 2.

Boncompagni 1862: 59]:[15]

> And if the surface and its four sides make 140, and you want to separate the sides from the surface. [. . .].

In Piero della Francesca's *Trattato d'abaco* we find still another version [ed. Arrighi 1970: 122]:[16]

> And there is a square whose surface, joined to its four sides, makes 140. I ask what is its side. [. . .].

Once again, not only the actual words but also the context show that Piero's reference is not Fibonacci (nor Abū Bakr/Gerard, nor Savasorda).

Finally, Luca Pacioli's *Summa de arithmetica* has the following formulation [1494: II, fol. 16^r]:

> And if the 4 sides of a square with the area of the said square are 140. And you want to know how much is the side of the said square. […].

The proof follows Fibonacci closely; the distinct statement (and in particular the return to the traditional order where the sides precede the area, certainly not what any Italian abbacist trained in algebra would invent on his own) thus shows that even Pacioli had access to the tradition through at least one more channel.[17]

Most of these texts contain other problems that refer explicitly to "all four sides" of squares or rectangles (or "both sides" of a rectangle, i.e., length and width): area plus or minus "all four" (or "both") sides equal to a given number, etc. Problems that involve the equality of all sides and the area are also present or referred to in certain texts: The Old Babylonian problems AO 8862 Nº 4 and AO 6770 Nº 1 (length and width equal to rectangular area) [ed. Neugebauer 1935: I, 111; II, 37; III, 62*f*];[18] Mahāvīra's *Gaṇita-*

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15 The proof may be of Fibonacci's own making. Elsewhere he follows Abū Bakr word for word when using him; the present deviations therefore indicate that his source for the actual problem was different (and his use of the traditional notion of separation – also found in certain Old Babylonian texts – shows that he did copy from a source).

16 1 am grateful to Luis Radford for having first directed my attention to this occurrence of the problem.

17 Most likely, this access is through the (so far unpublished) 15th-century Italian version of Fibonacci's *Pratica* on which Pacioli drew – cf. [Picutti 1989: 76].

⟦Ettore Picutti refers to the manuscript Palatino 577, Biblioteca Nazionale, Florence, which at inspection does contain the same problem on fol. 56^r. Inspection also shows, however, that this manuscript cannot be Pacioli's source, since it lacks many of the diagrams which are preserved by Pacioli. Instead, both treatises depend on the same source, Pacioli being more faithful than the anonymous compiler of Palatino 577. Picutti's mistake notwithstanding, the coincidence shows that somebody within the *abbaco* environment before Pacioli and the compiler in question had access to the original formulation.⟧

18 A number of other Old Babylonian texts deal with the sum of a rectangular area and length plus

sāra-saṅgraha, §§ 113½ and 115½ (square and rectangular perimeter equal to the area) [ed., trans. Raṅgācārya 1912: 221];[19] finally, the pseudo-Nichomachean *Theologumena arithmeticae* mentions that the square □(4) is the only square that has its area equal to the perimeter (II.11 and IV.29. trans. [Waterfield 1988: 44, 63], whereas Plutarch's *Isis et Osiris* 42 [ed., trans. Froidefond 1988: 214*f*] refers to Pythagorean knowledge of the equality of area and perimeter in the rectangle ⊏⊐(3,6).

Apart from the Old Babylonian tablet BM 13901 and a marginal exception in Fibonacci's *Pratica,* none of these texts explore problems that involve "non-natural" coefficients – as, for instance, "3 times the area plus $^{1}/_{3}$ of the side of a square equals 20′" (BM 13901 Nº 3). The constant reference to the striking (and the absence of systematic variation)[20] is characteristic of riddles and a contrast to the customs of school systems. Most if not all of the texts are of course connected to school traditions of some kind, and they contain other material which is not common property; but as in the case of the "100 fowls", the shared striking structure indicates that they draw on a common riddle tradition of oral cultural type rather than being directly connected (apart from the obvious but not all-explaining connections between some of the Italian treatises).

If we observe which other problems turn up regularly in the company of the "four sides", we may identify the main carrier tradition for the riddle (certainly a tradition with branchings): a heritage of practical geometers (surveyors rather than architects) diffusing from the Near East, probably with its original centre in the Syro-Iraqi-Iranian area. Here it provided the starting point for the creation of Old Babylonian school algebra. Perhaps already during the Assyrian or Achaemenid conquests, perhaps only in Hellenistic times, a characteristic variant reached Egypt; in a way which we cannot trace *the same variant* reached India – quite likely by way of the Jaina, since Mahāvīra belongs to this group. A type which may be closer to the original form was encountered by the Islamic mathematicians and borrowed from them by Savasorda, Fibonacci, and the Italian *abbaco* mathematicians. A form close to the original type also provided inspiration for the metric geometry (the so-called "geometric algebra") of *Elements* II. The "four sides and the area" of our Babylonian text, instead of being a scribal mistake, turns out to be perhaps the single six most significant lines in the record of documents for the history of pre-Modern mathematics.

width.

[19] Elsewhere (VII, §129½, p. 224), Mahāvīra treats the case where the rectangular area and perimeter are given separately.

[20] Or absence of variation at all: Piero gives the problem $_{4}s–Q = \alpha$ twice, once to illustrate an algebraic rule, and once under geometry; words and grammar are slightly different, but α remains 3 [ed. Arrighi 1970: 133, 177]; the same value is used by Fibonacci and Pacioli, in words which differ enough to exclude direct copying.

This accumulation of rash statements requires detailed arguments in order to be made credible. Space will only allow me to present some of them; others are discussed in the publications mentioned in note 6.

An Inventory – and a Noteworthy Discovery

First of all it will be convenient, however, to inventory the stock of riddles which seems to have been carried by the tradition at least since the early second millennium BCE. All recur in many of the texts mentioned above, or in others belonging to the same cultures and epochs. Some are so peculiar – either in their mathematics or in their formulation – that they are highly unlikely to have been invented independently in the various contexts where they turn up; others go so regularly together with the weird specimens that this can serve as an argument that even they have been transmitted and not reinvented by accident.

A crucial member of the group is of course the riddle of "the four sides and the square area". As we shall see below, we may even assume with fair certainty that the solution was 10 originally, as still in the last occurrence (and that the sides were mentioned first). All in all, the following problems on a single square seem to have been present from an early moment ("???" indicates doubt as to the date from which the problem is present; d is the diagonal of the square; here and everywhere in the following, Greek letters stand for given numbers):

$$
\begin{array}{ll}
s+Q=\alpha\ (=110) & s\text{-}Q=\varepsilon \\
{}_4s+Q=\beta\ (=140) & {}_4s\text{-}Q=\zeta\ (???) \\
Q\text{-}s=\gamma & {}_4s=Q \\
Q\text{-}{}_4s=\delta\ (???) & d-s=4\ (???)
\end{array}
$$

Most of these may be described as "quasi-algebraic" because they were solved in analytical steps which may easily be translated into an algebraic procedure, even though the actual argument was based on the same kind of "naïve" cut-and-paste geometry as illustrated in Figure 1 and Figure 2;[21] the last problem (solved exactly and repeatedly by Abū Bakr), if indeed present as a 'riddle' in early times, is likely to have been answered by then from the approximation $d = 14$ for $s = 10$.

The following problems dealing with two squares will have circulated already in the early second millennium:

$$Q_1+Q_2=\alpha,\ s_1\pm s_2=\beta \quad Q_1\text{-}Q_2=\alpha,\ s_1\pm s_2=\beta$$

At least when the difference between the areas is given, the two squares were thought

21 I use the term "naïve" for an approach where the correctness of the steps is "seen" immediately, as opposed to a "critical" approach (the term taken in a quasi-Kantian sense) which scrutinizes the *reasons why* and the *conditions under which* the procedure is valid.

of as concentric, and the difference thus as the area of a quadratic border; the cuneiform text where they are contained gives no solution, but in later (late classical and medieval) times the areas of such quadratic and circular borders were determined as the product of the "average length" (here $2s_1+2s_2$) and the width (here $(s_1-s_2)/2$).[22]

These problems dealing with a rectangle (length l, width w, diagonal d area A) can be traced back to the same early epoch:

$$A = \alpha,\ l \pm w = \beta \qquad\qquad A = \alpha,\ d = \beta$$
$$A + (l \pm w) = \alpha,\ l \mp w = \beta$$

Everything we may say about the beginnings of the tradition we are tracing depends on its interaction with the Mesopotamian scribe school (we shall return to the reasons that prevent us from identifying it directly with this school tradition). ~~A hint which possibly suggests its existence already before 2500 BCE is a school tablet from twenty-sixth century BCE Shuruppak showing a configuration of four equal circles touching each other [ed. Jestin 1937, N° 77).~~⟦[23]⟧ The same configuration (not wholly idiosyncratic, nor however one that would automatically arouse attention in every geometrically interested environment) recurs not only in the Old Babylonian tablet BM 15285 but also in Mahāvīra's *Gaṇita-sāra-saṅgraha* [ed., trans. Raṅgācārya 1912: 206]; in Mahāvīra's work, and probably in the somewhat damaged Old Babylonian specimen, the area of the enclosed space is asked for.

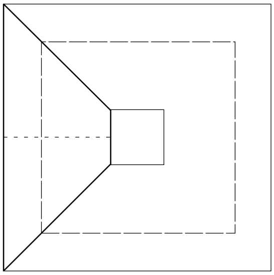

Figure 3. The bisection of a trapezium argued naïvely from concentric squares.

More firm evidence comes from the twenty-second century BCE. As pointed out by Robert Whiting [1984: 65*f*], the area problems in a school tablet from this outgoing "Old Akkadian" period[24] are so much facilitated by familiarity with the geometric-"algebraic" rule

$$\square(R\text{-}r) = \square(R)\text{-}2 \sqsubset\!\sqsupset(R,r) + \square(r)$$

that this rule is highly likely to have been presupposed; a roughly contemporary tablet exhibits familiarity with the principle that a trapezium is bisected by a parallel transversal

22 The quadratic case is in ibn Thabaṯ's *Reckoners' Wealth* (c. 1200 CE), ed., trans. [Rebstock 1993: 119]; the circular case is widespread.

23 ⟦The tablet in question has now been re-dated to the Old Babylonian period [Krebernik 2006: 14]; it is thus irrelevant to the dating.⟧

24 In the Old Akkadian period (mid-24th to mid-22nd centuries BCE), the Akkadian Sargonide dynasty united the Sumerian city states of southern Iraq into a territorial state. The language of school and administration remained Sumerian, but the impact of specifically Akkadian culture in various domains is not subject to doubt.

whose square is the average between the squares on the parallel sides.[25] This bisection problem follows the tradition until Abū Bakr and Fibonacci.

Other Old Akkadian problem texts ask, for instance, for one side of a rectangle if the area and the other side are known[26] (a problem which in Medieval sources is included in the group of "rarities" or supra-utilitarian problems when these are treated as a specific group).[27] Noteworthy, however, is the complete absence from the Old Akkadian record of problems to be solved by means of a quadratic completion ("mixed second-degree problems").

When the Old Akkadian state collapsed and was superseded by the "Neosumerian" empire (roughly coinciding with the 21st century BCE), all supra-utilitarian mathematics (as well as utilitarian mathematics described in abstraction from the actual use) disappear from the horizon. After a chaotic period following upon the disintegration of even this empire, the new states that emerged were Akkadian-speaking; much in the literate culture of the Sumerian states was taken over, but some new genres turn up in the record – or rather *almost new,* since they can be connected one way or the other to the Old Akkadian period.

One of these genres (which shall not occupy us any further, but which is important for showing that Akkadian cultural elements were adopted systematically into the new literate tradition) is the omen literature; the other is the reintroduction of supra-utilitarian mathematics into the scribe school (on a scale never seen before), with "second-degree algebra" as the *pièce de résistance.* The starting point was provided by the "sides-and-area riddles" of the surveyor tradition. In the following pages, we shall see how the scribe school transformed this material; for the present purpose we shall only notice that the fundamental technique – the quadratic completion – is spoken of in one text as "the Akkadian (method)", see [Høyrup 1990a: 326].

In the Old Akkadian record, as observed, there is no trace of this method (nor of problems where it might have served), even though other area problems are well presented (problems which in later mensuration texts would go together with "sides-and-area-problems"); around 1800 BCE, on the other hand, it was the central element of a borrowing of Akkadian material into the scribe school – a piece which within a few decades would give rise to the development of a genuine mathematical discipline. The obvious conclusion seems to be that the artifice was invented at some moment between 2200 BCE and 1800 BCE (most likely between 2100 and 1900) in a lay (that is, non-school) surveyors' environment, at first certainly as a mere trick, but that it was soon (and still in the lay

25 The rule can be grasped "naïvely" in a special case by means of the configuration of concentric squares (see Figure 3), and then generalized by proportional distortion.

26 See examples with analysis in [Powell 1976: 424–427].

27 Thus by ibn Thabāt, ed., trans. [Rebstock 1993: 124].

environment) discovered that the device permitted the construction and solution of a whole class of problems on squares and rectangles (most of those listed in the beginning of the present section).

Some of these problems do not require the trick: those on two squares, and the rectangle with given area and diagonal (for which, however, the calculator chooses a method which makes use of it). It is noteworthy, however, that all three as encountered in the school texts go via the average of the two unknowns and the common deviation from this average ($a = (x+y)/2$, $d = (x-y)/2$), and then find x and y as $a \pm d$. The same method is used for the other rectangle problems (in single square problems the same procedure is used, but in these cases a and d by necessity have a different interpretation, since only one magnitude – either $a+d$ or $a-d$ – is asked for).

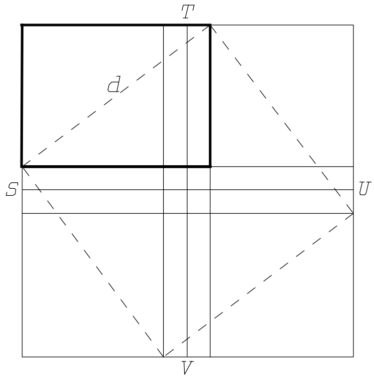

Figure 4. The diagram showing that □ $(d) \pm 2A$ =□$(l \pm w)$ in a rectangle.

That the use of average and deviation is no compulsory choice can be illustrated by the solution to the diagonal-area problem: In the mainstream of the tradition (as reflected in the very early Old Babylonian tablet Db_2-146, in the *Liber mensurationum,* in the *Liber embadorum* and in Fibonacci's *Pratica*) this was always reduced to one of the other rectangle problems (see Figure 4): either the case where the area and the difference between the sides is given – □$(l-w)$ = □$(d)-2A$ – or the one where the area and the sum is known – □$(l+w)$ = □$(d)+2A$. This problem was then solved by the usual method involving quadratic completion and average and deviation, even though it seems much easier, at least to us, to find both the sum and the difference between the sides, and to go on from there.[28]

The two-square problems $Q_1+Q_2 = \alpha$, $s_1 \pm s_2 = \beta$ are also solved in a way that suggests use of the diagram in Figure 4, including average and deviation, that is, the division of

[28] The number of arithmetical operations is the same in both computations, and with a single exception even the operations themselves are the same – the alternative method has a halving instead of a squaring, which is the only real simplification. Mathematical simplicity is more a question of habit than we may tend to believe.

the diagram by means of the lines *SU* and *TV*.[29] Here no reduction to a different problem takes place, and the predilection for average and deviation is therefore by necessity an independent choice.

No Old Babylonian school text determines the area of a triangle from a computed height, even though a few late Old Babylonian tablets from Susa contain numerical parameters for regular polygons that presuppose the determination of a height in an isosceles triangle (TMS II and III, ed. [Bruins and Rutten 1961: 23–27]).

Elements II.12 and 13 (the "extended Pythagorean theorem") allow the determination of external and internal heights in scalene triangles from the sides, and it has been a natural assumption that this was a Greek discovery; in Heron's *Metrica* I.5–6, the Euclidean theorems are indeed used for this purpose; so they are in *Geometrica,* mss. A+C, 12.1–29. A large number of medieval treatises, however, give a different (though of course algebraically equivalent) formula for the inner height,[30] at times mentioning the Euclidean formula as a possible alternative; for outer heights, however, the Euclidean model is invariably the only possibility. The sole coherent explanation is that the practitioners' environment had discovered (somewhere between 1500 and 500 BCE, probably late in this timespan since the formula is absent from Demotic sources) how to determine the height already before the Greek theoretical geometers took over – but only in the case of "genuine", that is, inner heights;[31] the Greek theoreticians generalized the concept,

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29 This is at least the method used in the Old Babylonian text BM 13901 and (without geometry) in Diophantos's *Arithmetica* I.28. The treatment of the corresponding problem in *Elements* II.9–10 (to whose relevance we shall return) makes use of considerations that are incongruous with anything we know from the second millennium BCE, and is therefore almost certainly a late development (but possibly invented in the practitioners' environment, as suggested by the use of the characteristic configuration from *Elements* II.10 for a wholly different purpose in *Geometrica* A and C, ed. [Heiberg 1912: 331]. Diophantos's routine use of average and deviation in the second-degree problems is in significant contrast both to what he does in I.1–13 (simple first-degree problems) and I.15–25 (undressed recreational first-degree problems – "give and take", "purchase of a horse", etc.) In the former group, one of the numbers is routinely identified with the *arithmós;* in the latter, a particular choice adapted to each case is made.

30 The underlying reasoning (occasionally made more or less explicit) goes as follows (see Figure 5): The difference $\square(q)–\square(p)$ between the squared projections of the sides equals the known difference $\square(b)–\square(a)$ between the squared sides themselves, since $\square(a) = \square(p)+\square(h)$, $\square(b) = \square(q)+\square(h)$; but $\square(q)–\square(p)$ can also be understood as the quadratic border between two concentric squares, and thus be determined as the "average length" $2{\cdot}(p+q) = 2c$ times the width ${}^{q-p}/_2$ of the border; together with ${}^{q+p}/_2 = {}^{c}/_2$ as average, this deviation yields p and q. Afterwards, h is determined from a and p by means of the Pythagorean theorem.

⟦The appearance of this way to determine heights in medieval treatises is dealt with in article I.9.⟧

31 As al-Khwārizmī explains in the geometrical chapter of his *Algebra* [ed., trans. Gandz 1932: 81], obtuse-angled triangles possess but a single height.

and devised a formula that allowed the determination of external heights (*viz Elements* II.12). This was adopted by the practitioners; for the inner heights, however, they stuck to the traditional formula.[32]

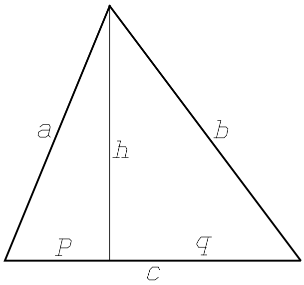

Figure 5. The quasi-algebraic determination of the height in a scalene triangle.

Adoptions I: The Scribe School

As briefly hinted at above, the early Old Babylonian scribe school borrowed the traditional stock of surveyors' riddles and made it the starting point for the construction of a genuine discipline.

One may legitimately ask how it is possible to distinguish a presumed original stock from a further development in the school, given that the texts left by the school constitute our only source for both. The key is the totally different character of the bundle of problems that turn up in all the later sources and the Old Babylonian corpus as a whole. For the present purpose, the characteristics of this corpus are adequately represented by the totality of the theme text BM 13901 (see p. 74): it begins by two simple cases – square area plus and minus the side. Then it goes on with systematic variation of coefficients, both for the side and for the area; next follow various problems on two, later three or four squares.

In contrast, the problems that recur in classical and medieval (and even later Babylonian) sources are all restricted to "natural" coefficients. A square possesses one area, and either four sides or *the* (characteristic) side; a rectangle possesses a single area, a single diagonal (possibly two, but that is never used), four sides or two characteristic sides (which determine a single sum or a single difference).

The systematic variation (tedious for those who have already understood no less than for those who never understand) is characteristic of the school situation, and only possible here (cf. p. 72); the reference to the naturally occurring, on the other hand, is characteristic of the riddle. This harmony between, on one hand, the distribution of problems in the sources, on the other, institutionally determined distinctive modes of thought, is what allows us to discriminate an original stock of riddle-type problems from the transformation of the material effected by the school.

Our original starting point – "the four fronts and the area", N^{o} 23 of our theme text – highlights the difference by pointing to another subtle difference between the riddle and the school problem. A riddle-problem will start by mentioning what is obviously or most actively *there,* and next introduce dependent entities – in the riddle of the three brothers (protectors and potential rapists) and their sisters (virtual victims) the brothers come first,

[32] The topic is discussed in detail and with added shades in Høyrup 1997a, 81–85. ⟦Included here as article I.9.⟧

in the case of somebody encountering a group of people, these first, next their double, etc.[33] In systematic school teaching, the order will tend to be determined by internal criteria, for instance derived from the method to be applied. The typical school problems will therefore mention the area before the side – in the solution, the area is drawn first, only afterwards will a rectangle $\sqsubset\!\sqsupset(1,s)$ be joined to the area in order to represent the numerical value of the side. The surveyors' riddle, on the other hand, will start with what is immediately given to surveying experience, that is, by the side; the area is found by calculation and hence derivative, and therefore mentioned last.

The ordering of the members is thus one way in which "the four sides and the area" refers to roots outside the school environment. Other ways are the terminology, as already pointed out on p. 74, and the identification of the unit. Finally, the value of the solution is remarkable: With one partial exception (on which presently), no other extant Old Babylonian problem on a single square chooses the side to be 10 (in any order of sexagesimal magnitude) – the standard is 30 (mostly 30′, i.e., ½), at times 20 (actually 20′ = $^1/_3$) is used instead. 10′ (= $^1/_6$) is no obvious choice at all – but read as an originally integer 10 moved into the habitual order of magnitude of the school it makes sense if coming from an environment where 10 (and not, for instance, 60) is a round number. Even the value of the solution thus points away from the school and toward lay Akkadian speakers (in Akkadian, as in all Semitic languages, counting is decadic).

The occurrence of "the four sides and the area" within BM 13901 thus illustrates the role and place of "recreational" problems within a school-based teaching system. After the relative monotony of systematic variation and gradual increase of the intricacy of problems comes an unexpected variation: simple in its mathematical contents but solved by means of a surprising trick; similar to the usual method (which corresponds to Figure 2) but nonetheless strikingly different; and obviously school-external in its formulation. The problem, once a challenge and test of competence and professional identity, has really become a piece of mathematical recreation.

The surveyors' riddles was only one of several sources for the Old Babylonian mathematical curriculum (though certainly the most important source for its supra-utilitarian level – well beyond half of all problems are mixed second-degree problems), which merges several strains of recreational problems with problems and methods clearly derived from Neosumerian mathematical practice[34] and with others which seem to derive from an older stratum of Sumerian mathematics.[35] This situation obviously does not facilitate

33 Both examples are picked from the *Propositiones ad acuendos iuvenes* (N° 17 and 2, respectively) [ed. Folkerts 1978: 54, 45].

34 Not least problems making use of technical coefficients that had gone out of practical use in Old Babylonian times; see [Robson 1995: 170, 232].

35 The distinction between two strata of Sumerian mathematics was first suggested by Robson [1995:

the attempt to disentangle the original lay tradition from the contributions of the school.

Nor shall we try to trace the adoption process in geographical detail ⟦but see article II.8⟧; it will suffice that the adoption occurred more or less independently in at least two different places, Eshnunna in the northern periphery of the Neosumerian empire, and somewhere in the Sumerian heartland (possibly Larsa); and that school texts from some other place toward the north (almost certainly Sippar) and from Susa suggest continuous interaction with the lay tradition.[36]

One text from the group that may come from Sippar should be mentioned: BM 80209 – discussed in [Friberg 1981]. It is a catalogue of problem statements on squares and circles. Squares are introduced with a phrase meaning "*s,* each, confronts itself" (*s* being a number designating the side), that is, with the very phrase used in the last line of BM 13901 Nº 23, merely with an added "each" (which recurs, for instance, in *Liber mensurationum* when the single and not all four sides are spoken of). Already this phrase (found in one more text from the "Sippar" group; in the text referred to in note 37; and nowhere else) is sufficient to show the close affinity between the lay tradition and the text in question.

One of the square problems of BM 80209 is the above-mentioned partial exception to the rule that no Old Babylonian single-square problem except BM 13901 Nº 23 chooses the side to be 10. In the present case, however, the square in question is not real and not alone but the mid-square of a quadratic border contained between two concentric squares.[37]

The circle problems have no direct counterpart elsewhere in the Old Babylonian corpus, but may be regarded as analogues of familiar square problems. If A designates the area, d the diameter and p the perimeter, then a first sequence contains problems $A\pm\alpha p = \beta$, where α is varied systematically; then comes an analogue of BM 13901 Nº 9, viz the two-circle problem $A_1+A_2 = \alpha$, $p_1 = p_2+10'$ (with three different values for α). Finally come three problems $A+d+p = \alpha$. Everywhere we must presume p to be the basic parameter, and A and D to be determined as $A = 5'\cdot\square(p)$, $d = 20'\cdot p$ ("$\pi = 3$").

204–209]; the idea is vindicated by a conspicuous contrast between two different ways to refer to a "square root", that is, the "squaring" side of an area (through a verbal construction, or as a noun); see [Høyrup 2000a: 65*f*].

[36] The arguments for the whole geographical analysis builds on close analysis of terminological differences and orthography; see [Høyrup 2000a].

[37] This interpretation follows from another text (UET 5,864) discussed in [Kilmer 1964], which asks for the construction of a quadratic border around the corresponding mid-square $\square(s)$ and with the same area, and finds the width of the border through multiplication of s by the reciprocal "of 4, of the four fronts" ("front" however with the Sumerogram, as almost the whole text). The text is from the South (Ur), but certain features of its highly untypical terminology are shared with "Sippar", Eshnunna and other peripheral texts.

The types $A \pm \alpha p = \beta$ and $A_1 + A_2 = \alpha$, $p_1 = p_2 + \beta$ are not found in later sources, and it seems a natural assumption that they are school generalizations (not least the first type, with its systematic variation of α). The type $A+d+p = \alpha$, on the other hand, is also found in two of the *Geometrica* treatises (mss A+C and chapter 24); in ibn Thābat's handbook [ed., trans. Rebstock 1993: 113*f*]; and in Mahāvīra [ed., trans. Raṅgācārya 1912: 192]. The Greek and Arabic treatises presuppose π to be $3^1/_7$, whereas Mahāvīra takes it to be 3 in the present problem (in a later section he uses $\sqrt{10}$ as the "precise" value). The Greek and Arabic order is $d+p+A$, Mahāvīra's is $p+d+A$.

Mahāvīra, we observe, has the members in "riddle order" ⟦*Bronze Age* riddle order, however; his own basic circle parameter parameter is not the perimeter but the diameter⟧ (the Babylonian text, as we notice, is in "school order"). It thus seems probable that his ultimate source is not the school version but the lay riddle tradition, from which even the Old Babylonian school will have taken over the problem.

This has an interesting consequence. When expressed in terms of p, the equation $A+d+p = \alpha$ becomes $5' \cdot \square(p) + 1°20' \cdot p = \alpha$. In contrast to all the riddle problems listed above (p. 79), this equation is non-normalized. The trick used routinely in the school texts to solve such problems is to transform it into a problem $\square(5'p) + 1°20' \cdot (5'p) = 5' \cdot \alpha$ (geometrically, by changing the scale in one direction). If the present problem actually belonged to the stock of surveyors' riddles, as seems most likely, then this trick is no invention of the school but another borrowing – but a borrowing which, like the quadratic completion, was transformed in the process of borrowing, from trick into fundamental mathematical technique.

The Old Babylonian social system was brought to a violent end by a Hittite raid in 1595 BCE. This was also the end of the old school institution – from now on ⟦scholar⟧ scribes were trained as apprentices in scribal "families", about whose actual constitution we know little. The advanced parts of the algebraic discipline seem to have been wiped out in the process. When "algebraic" problems turn up in the texts again in the later first millennium BCE, problems with "non-natural" coefficients are totally absent, and discontinuities in the technical use of Sumerograms point to a re-Sumerianization of mathematics, and thus to transmission through communities with scant knowledge of Sumerian (that is, non-scribal though hardly fully illiterate communities). ⟦Much more about this is said in article II.9.⟧

The Seleucid text AO 6484 [ed. Neugebauer 1935: I, 96–99][38] shows that not all second-degree algebra had been forgotten in environments that made use of sexagesimal computation and of the appurtenant tables of reciprocals: it contains several problems where the sum of a number and its reciprocal are given. Since the product is automatically known to be 1, this is a translation into numbers of the rectangular problem where the

38 The scribe of the tablet introduces himself as an astrologer-priest.

area and the sum of length and width are given. The same unmistakable problem type is well represented in the Old Babylonian corpus.

We possess no information about the social organization of practical mathematics in the region in the millennium following upon the collapse of the Old Babylonian social system; in any case, however, the ongoing use of the sexagesimal place value system appears to presuppose a minimal level of cuneiform literacy. The transmission pattern of mathematics from early second to late first millennium Mesopotamia has probably been complex, involving some kind of scribal as well as definitely nonscribal milieus.

Interaction with environments that were literate in Aramaic (which was written alphabetically and hence allowed lay restricted literacy) is also likely to have existed,[39] as are farther-ranging links. Whereas the extant Late Babylonian but pre-Seleucid texts (fifth to fourth century BCE?) still move within the range of themes that are known from the Old Babylonian period (even though they are likely to have adopted material from local lay practitioners of some kind), Seleucid texts (third or second century BCE) go far beyond these confines, at times using procedures that in later times are only found outside the Mesopotamian region.

Some novelties are contained in the above-mentioned Seleucid text AO 6484, together with the evidence for continuity presented by the "number and reciprocal" problems. Most important, however, is the text BM 34568 [ed. Neugebauer 1935: III, 14–17]. One problem treats of alligation, all the others deal with rectangular sides, diagonals and areas;[40] apart from determinations of d from l and w or of w from d and l, everything is new in some way. Two problems are traditional as such, giving A and either $l+w$ or $l–w$; but the procedures differ from traditional ways, finding for instance in the former case $l–w$ as $\sqrt{(l+w)^2-4A}$, and next $w=\frac{(l+w)-(l-w)}{2}$, $l = (l+w)–w$; no single problem in the text makes use of average and deviation.

The remaining problems belong to totally new types, among which the following are the most important:[41]

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[39] See [Friberg, Hunger and al-Rawi 1990: 510, 546]. Continuation of the lay surveyors' tradition in a marginally literate community is the most likely explanation of the striking presence in the *Liber mensurationum* of whole phrases and of complex grammatical structures that have a clear Old Babylonian ring (and are totally absent from the Late Babylonian texts!) (cf. [Høyrup 1986: 459*f*]), whereas the problems are of the riddle type (including the order of members) and thus point to a carrying tradition with oral cultural characteristics.

[40] One of them is dressed as a problem on a reed leaned toward a wall (a situation which is also known from an Old Babylonian text, but used there for a quite elementary purpose). In the context of rectangular problems it is obvious, however, that the underlying problem is $d–l = 3$, $w = 9$ (symbols as on p. 80).

[41] N° 5 is the problem to which "given diagonal and area" is reduced in the *Liber mensurationum;*

(1) $l+w+d = \alpha$, $A = \beta$; solved from ⊏⊐($l+w+d$)–2A = 2⊏⊐(d,$l+w+d$)
(2) $d–l = \alpha$, $w = \beta$ (the reed problem)
(3) $d+l = \alpha$, $w = \beta$
(4) $d+l = \alpha$, $d+w = \beta$
(5) $l+w = \alpha$, $d = \beta$

All turn up again (at times in slightly altered form) in the *Liber mensurationum,* and again in Fibonacci's *Pratica.* The latter sometimes presents the geometrical argument behind the solutions (diagrams which generalize the principle of Figure 4, for instance the square on $l+w+d$, in which the partial squares of course fulfil □(l)+□(w) = □(d). Given the character of Fibonacci's treatise and his faithful rendering of proofs we know he has seen, it is likely that these proofs go back to the invention of the problems.

Types (1)–(4) cannot make use of average and deviation, and it is thus no wonder that even the *Liber mensurationum* (otherwise quite faithful to this old procedure) follows the same principle. Type (5), on the contrary, is reduced (twice) in Abū Bakr's manual to the type $l+w = \alpha$, $A = \beta$, and then solved as in the Old Babylonian texts. If the new problems had been invented within an orbit that was wholly accustomed to the average-deviation technique, it seems more plausible, either that it would have shaped even type (5) in this traditional pattern, or that further transmission within the Mesopotamian region (where Abū Bakr is likely to have composed his treatise and to have found his sources) would have retained the new form. The argument is not conclusive, but all in all BM 34568 seems to have adopted its problems not only from a non-school (that is fairly certain) but even from a non-local tradition. The historical context certainly does not exclude such a process: for half a millennium, Assyrian, Persian and Macedonian armies with train, accountants and surveyors had moved back and forth between the shores of the Indus and those of the Nile; merchants were no less mobile.

Given the similarities of problem types and techniques, the environment where the new problems emerged is likely to have drawn on that surveyors' tradition which we have discussed so far. Even this is of course historically quite possible – all references to (supposedly identifiable and clear-cut) "traditions" are shorthand for situations that will have been much more turbid than the term tends to make us believe. This is illustrated by the way the diagonal-area rectangle problem is dealt with in various locations. In sources that keep close to the Near-Eastern beginnings (as shown, e.g., by the use of the traditional linguistic format in the *Liber mensurationum*), it was always solved in the traditional way (cf. p. 82). In the Demotic Papyrus Cairo J.E. 89127–30, 89137–43 [ed., trans. Parker 1972: 41-43] from the third century BCE, the problem is solved by what was presented above (p. 82) as the seemingly "much easier way", without recourse to average

but in contrast to this work, the Seleucid solution does not go via average and deviation, and thus remains untraditional.

and deviation.[42] The same procedure is followed in the Latin *Liber podismi* [ed. Bubnov 1899: 511*f*] and by Mahāvīra [ed., trans. Raṅgācārya 1912: 224].

Adoptions II: Greek Theory

In 1936 Neugebauer launched the theory that the area geometry of *Elements* II (routinely characterized as "geometric algebra" since Zeuthen [1886: 5*ff*] formalized a much older notion) would be a translation of the results of a supposedly numerical Babylonian algebra into the idiom of geometry, called forth by the discovery of irrationality; ⟦[43]⟧ in [Neugebauer 1963: 530] he further argued that the Babylonian heritage had become "common mathematical knowledge all over the ancient Near East", and that a (historically rather implausible) direct translation of cuneiform tablets hence needed not be involved. Neugebauer's thesis was generally endorsed and only subjected to criticism by Árpád Szabó [1969: 445*ff*] and Sabetai Unguru ([1975], [Unguru and Rowe 1981]), both of whom emphasized the wholly different thought styles and cognitive aims of the Babylonian and the Euclidean texts. Szabó proposed as an alternative a development from "naïve" considerations similar to those described in the passage of Plato's *Meno* (82B–85E) where a slave boy is led to find out how to double a square.

The discovery that Babylonian "algebra" is not numerical but indeed a "naïve" cut-and-paste technique dealing with measurable segments and areas evidently changes the situation, and makes it seem much more plausible that the Greek geometers borrowed and transformed a Babylonian discipline. It has to be asked, however, exactly what kind of mathematics was "common […] knowledge all over the ancient Near East", and exactly which knowledge is reflected in the Greek texts.

For this purpose a look at the contents of *Elements* II.1–10 will be useful. In symbolic notation the propositions can be summed up as follows:

1. $\sqsubset\!\sqsupset(e,p+q+\ldots+t) = \sqsubset\!\sqsupset(e,p) + \sqsubset\!\sqsupset(e,q) + \ldots + \sqsubset\!\sqsupset(e,t)$
2. $\square(e) = \sqsubset\!\sqsupset(e,p) + \sqsubset\!\sqsupset(e,e-p)$
3. $\sqsubset\!\sqsupset(e,e+p) = \square(e) + \sqsubset\!\sqsupset(e,p)$

[42] The same papyrus contains material with indubitable Babylonian affinities: the "reed against a wall" is found both in the elementary Old Babylonian variant (to determine w from d and l) and in the sophisticated variant of BM 34568 (together, no less than 8 out of 40 problems). The (heavily damaged) Demotic Papyrus Carlsberg 30 [ed. Parker 1972: 74] contains a diagram showing the 10x10 square with diagonal $14^1/_7$, and another with side $14^1/_7$ and indicated area 200. As we remember from p. 76, Savasorda's version of the quadratic "surface added to its four sides" presupposes $s = 7$; a problem in AO 6484 [ed. Neugebauer 1935: I, 97] asks for the square side when the diagonal is 10. Even here the Demotic material is thus witness to an interest in "cascades of squares" which also turns up both in Seleucid material and in relation to the index fossil which constituted our starting point.

[43] ⟦Much more on the topic of "geometric algebra" can be found in article II.5.⟧

4. $\square(e+f) = \square(e) + \square(f) + 2\sqsubset\!\sqsupset(e,f)$
5. $\sqsubset\!\sqsupset(a+d,a-d) + \square(d) = \square(a)$
6. $\sqsubset\!\sqsupset(e,e+2d) + \square(d) = \square(e+d)$
7. $\square(e+p) + \square(e) = 2\sqsubset\!\sqsupset(e+p,e) + \square(p)$
8. $4\sqsubset\!\sqsupset(a,d) + \square(a-d) = \square(a+d)$
9. $\square(a+d) + \square(a-d) = 2[\square(a) + \square(d)]$
10. $\square(e) + \square(e+2d) = 2[\square(d) + \square(e+d)]$

At closer inspection, all of this turns out to be familiar from the above – all propositions are indeed justifications ("critiques") of the ways the surveyor tradition handled its problems. In detail:

Prop. 1 shows that rectangles can be cut (or, the other way round, pasted if possessing a common side); prop. 2 and 3, actually nothing but corollaries of prop. 1, show that sides (transformed into rectangles by being provided with a projecting width *1*, cf. note 9) may be removed from or joined to a square.

Prop. 7 is nothing but the rule $\square(R\text{-}r) = \square(R)\text{-}2\sqsubset\!\sqsupset(R,r)\text{+}\ \square(r)$ which appears to be presupposed in the Old Akkadian area text referred to on p. 80 ($R = e{+}p$, $r = e$), and prop. 4 is its additive counterpart (which is not likely not to have been discovered together with the other).

Prop. 5 and 6 were always used in later times when the algebraic solution to mixed second-degree equations were argued on Euclidean foundations (only a few Renaissance writers would use II.4); if a is the average between e and $e{+}2d$, they are easily seen to be algebraically equivalent. Geometrically, however, they correspond to different situations: In prop. 5, the lines $a{+}d$ and $a{-}d$ are imagined as parts of a total $2a$, in prop. 6 the line e is part of the line $e{+}2d$. The proofs are made correspondingly, and agree with the Old Babylonian respective ways of resolving the two rectangle problems $A = \alpha$, $l{+}w = \beta$ and $A = \alpha$, $l{-}w = \beta$. The former also conforms to the solution of problems of the type $\alpha s - Q = \beta$, the latter to those of problems $Q \pm \alpha s = \beta$.

Prop. 8 corresponds to the rule that the border between concentric squares $\square(a+d)$ and $\square(a-d)$ is equal to four rectangles with length equal to the average side a and width equal to the width d of the border, that is, to a rectangle whose length is the average length of the border and whose width is the distance between the squares; this serves, as we have seen, for two-square problems $Q_1 - Q_2 = \alpha$, $s_1 \pm s_2 = \beta$. Prop. 9 and 10 correspond to the two-square problems $Q_1 + Q_2 = \alpha$, $s_1 \pm s_2 = \beta$.

Prop. 4–7 serve later in the *Elements* (in particular in Book X), but the others are never referred to again: their substance seems so familiar that it need not be mentioned explicitly once its reliability has been validated – cf. [Mueller 1981: 301]. Clearly, *Elements* II.1–10 establish nothing new, they are meant to consolidate the well-known – to be a "critique of mensurational reason", showing why and under which conditions (e.g., genuine right angles) the traditional ways could be accepted, and formulating the outcome in a general form.

How this critique is made can be exemplified on the proof of prop. 6 [trans. Heath 1926: I, 385],

> If a straight line be bisected and a straight line be added to it in a straight line, the rectangle contained by the whole with the added straight line and the added straight line together with the square on the half is equal to the square on the straight line made up of the half and the added straight line.

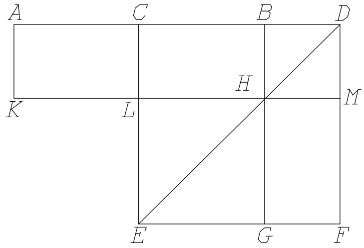

Figure 6. The diagram of *Elements* II.6.

The first half of the demonstration constructs the square on *CD,* where *C* is the mid-point of *AB;* draws the diagonal *DE;* draws *BG* parallel to *DF* and intersecting the diagonal in *H;* etc. Then Euclid is able to prove (in agreement with the "critical" standards of theory) in the second half of the demonstration the equality of the rectangles *AL* and *HF,* and to go on precisely as the *Geometrica* area-and-perimeter problem in Figure 2 – which on its part is nothing but the traditional cut-and-paste solution known from the Old Babylonian texts.[44] Euclid shows that what was traditionally "seen" to be correct can in fact be proved according to the best standards of theoretical geometry. But the *proof idea* remains the same. This holds for the first seven propositions. That of prop. 8 is likely to have been modified so as to facilitate agreement with the general formulation of the theorem, since it locates the smaller square in the corner of the larger one and not concentrically; those of prop. 9–10 were discussed in note 29.

It is noteworthy that the proofs of the single propositions are independent, though some of them could easily be proved from others. That each proposition gets its own proof shows that not only the knowledge contained in the theorems but also the traditional heuristic proofs were meant (if not by Euclid then by a source which he follows faithfully) to be consolidated by theoretical critique.

Euclid is certainly not responsible for the adoption. What little we know about the work of Hippocrates of Chios and Theodoros of Cyrene shows them to have used some

44 Except for one detail: Euclid's orientation of the diagram (which is similar in all manuscript traditions and therefore likely to be original). The only Old Babylonian text which suggests an orientation for an area-and-sides configuration (TMS IX, see [Høyrup 1990a: 325] puts a side to be added under the rectangle, as does *Geometrica* 24.3. Informal experiments conducted with historically innocent students reveal a tendency to order area and adjoined sides in the direction of writing (as does TMS IX). If this holds true even for Euclid or his source, the diagram constitutes a piece of weak evidence in support of an adoption from an Aramaic environment.

⟦In 2012, Gregg de Yong kindly looked through his collection of digitized medieval manuscripts of the *Elements* together with me. Not *all* but only most exhibit the "Aramaic" orientation. The evidence is certainly weak.⟧

kind of metric geometry. The step from *use* to *critique* could but need not be slightly later: coins from Aegina, which in the fifth century had carried a "naïve" geometric diagram, exhibit the diagram of II.4 (including the diagonal that makes the proof "critical") from 404 BCE onward [Artmann 1990: 47], which could mean that the topic was hot at that date. All in all it seems plausible that the theory presented in *Elements* II was created in the mid- to late fifth century BCE. ⟦[45]⟧

One of the interesting features that connects the Hippocratic fragment on the lunules and Plato's oblique account of Theodoros's work on irrationals with each other and with "algebra" and later metric geometry is the use of the term *dýnamis.*

In Diophantos's *Arithmetica,* to which we shall return, we are told that "it has been approved" to designate the second power of the unknown number as *dýnamis,* thus making it an "element of arithmetical theory", that is, of *algebra* as treated by Diophantos [ed. Tannery 1893: I, 4]. This, and various other agreements, show us that divers passages in the *Republic* and other Platonic works refer to a second- and third- degree calculators' algebra, and that even this early algebra used the term *dýnamis* for the second power – cf. [Høyrup 1990c: 368*f*].

But as it is evident from other passages in Plato and Aristotle; from the Hippocratic fragment; and from the *Elements* and certain Archimedean writings, the term was also part of the geometers' idiom. In this function, its interpretation has provoked much discussion – at times it seems to mean a square, at times it seems to stand for the side of a square or a square root.

Closer analysis of all occurrences shows that the term is no more ambiguous than mathematical terms in general, only unfamiliar [Høyrup 1990c]. It stands for the quadratic figure parametrized by its side, that is, for a square *identified by* – and hence potentially *with* – its side. In other words, a *dýnamis* is a square that *is* its side and *possesses* an area, whereas our square (and the Greek *tetrágonon) has* a side of, for example, 2 m, and *is* 4 m^2.

The *dýnamis* did not correspond to the Greek conceptualization of a figure as "that which is contained by any boundary or boundaries" (*Elements* I, def. 14, trans. [Heath 1926: I, 153]), and already in Euclid's and Archimedes's times the term tended to vanish from geometry. This incongruity is striking, given the central importance of the term in the corpus of early references to geometry. It is therefore interesting that a concept with exactly the same unfamiliar structure is present in Old Babylonian mathematics: the *mitḫartum,* literally a "[situation characterized by the] confrontation of equals", the square conceived primarily as the square frame ⟦and parametrized by the side⟧.

[45] ⟦Two fanciful alternative explanations, proposed by David Aboav and Gerhard Michael Ambrosi, can be safely disregarded, see [Høyrup 2013].⟧

Szabó [1969: 46*ff*] points out that both *dýnamis* and the verb *dýnasthai*[46] have connotations of equivalence and commercial value, together with the basic denotation of physical strength; exactly the same range of de- and connotations belongs with the verb *maḫārum,* from which *mitḫartum* is derived.

All this does not prove that the Greek dynamis is a calque of the Babylonian *mitḫartum* (or, rather, of some Aramaic term with a corresponding meaning and semantic range and used by Near Eastern mathematical practitioners around 500 BCE). But taken together it must at least be counted as circumstantial evidence pointing in that direction. It certainly fits other types of evidence for a fifth-century adoption of that quasi-algebraic technique in which the Babylonian word had served.

In the interest of (relative) brevity I shall abstain from closer discussion of two other Euclidean works which exhibit influence from the surveyors' tradition and riddles: the *Data,* and the treatise *On the Division of Figures.* Instead we shall look at Diophantos's *Arithmetica,* book I.

This book contains a variety of "recreational" riddles translated into pure-number problems – N° 24 [ed., trans. Tannery 1893:1, 56–59], for instance, asks for three numbers (say, p, q, and r) that fulfil the condition $p+(q+r)/3 = q+(p+r)/4 = r+(p+q)/5$ – an unmistakeable expurgated version of the "purchase of a horse". Translated into symbols, propositions 27–30 look as follows:

27. $p+q = \beta,\ \ p{\cdot}q = \alpha$	29. $p+q = \beta,\ \ p^2-q^2 = \alpha$
28. $p+q = \beta,\ \ p^2+q^2 = \alpha$	30. $p-q = \beta,\ \ p{\cdot}q = \alpha$

or, when interpreted as rectangle and two-square problems:

27. $l+w = \beta,\ \ \mathrm{A} = \alpha$	29. $s_1+s_2 = \beta,\ \ Q_1-Q_2 = \alpha$
28. $s_1+s_2 = \beta,\ \ Q_1+Q_2 = \alpha$	30. $l-w = \beta,\ \ \mathrm{A} = \alpha$

Diophantos's immediate aim coincides with that of the practitioners: to (teach how to) find the solution – neither to construct a critique nor to formulate solvability theory. In contrast to what we find in sources that reflect the culture or teaching of practitioners, however, theoretical reflection is made explicit in two different ways: firstly, each problem is formulated in general terms, even though the solution is demonstrated on a paradigmatic example; secondly, by the formulation of diorisms telling the conditions for solvability when such conditions exist. These conditions are told to be *plasmatikós,* which may (but need not) mean that they can be seen in a diagram, a *plásma* (which indeed they can, namely the traditional "naïve" diagrams) – cf. the discussion in [Høyrup 1990a: 349*f*].

[46] To "master" or to "be worth", used in geometry to tell that a line "masters" that square of which it is the side; in Aristotle's formulation of the Pythagorean theorem (*De incessu animalium* 708[b]33-709[a]2), the hypotenuse "is worth" the sides containing the right angle)

All of this – *Elements* II.1–10 (and VI.28–29), *Data* 84–85, the bisected trapezium from the treatise *On the Division of Figures, Arithmetica* I.27–30 – refers to the stock of problems that seems to have belonged to the lay surveyors' tradition already before the Old Babylonian scribe school adopted its riddles. With explainable exceptions it also exhausts this stock: rectangle problems $A+(l\pm w) = \alpha$, $l\mp w = \beta$ were always solved by an elegant "change of variable" which allowed reduction to the types $A = \alpha$, $l\pm w = \beta$, and they were thus uninteresting on their own from a theoretical point of view – the only place where they *might* at a pinch have fitted in is in the *Arithmetica.* The circle problem $A+d+p = \alpha$ was of course inaccessible to treatment inasmuch as the ratio between the circular diameter and perimeter was inexpressible. The rectangle with given area and diagonal is not present in itself, but by means of the Pythagorean theorem it is solved via either *Elements* II.4 or II.7 (as explained by Fibonacci and Savasorda). The equality of perimeter and area for squares and rectangles has no place within a geometry not based on a unit length.

In all cases where the alternative presents itself, these texts make use of average and deviation; the new ways of BM 34568 and the Demotic papyri leave no trace in the works of the Greek theoreticians.

Something more than mere traces is found, however, if we go to the rare surviving representatives of the practical tradition of the Graeco-Roman world proper. As reported on p. 90, the Latin *Liber podismi* (whose title shows it to be derived from a Greek model) finds the sides of a rectangle (actually of a right triangle) from diagonal and area in the same way as in the Demotic Cairo Papyrus, that is, from sum and difference. The Graeco-Egyptian Papyrus Geneve 259[47] contains three problems on right triangles with the following data:

1. $w=3$, $d=5$
2. $w+d=8$, $l=4$
3. $l+w=17$, $d=13$

The first, of course, tells us nothing. The second and third, on their part, belong to types that only turn up in the Seleucid and Demotic material. Nº 3, moreover, is one of the types which, when adopted into the tradition reported by Abū Bakr, was solved by means of average and deviation. The Geneva Papyrus does nothing similar; even though its exact method for numbers 2 and 3 are idiosyncratic, their general tenor is that of the Seleucid text.

Other echoes of the surveyors' tradition in Greek practitioners' mathematics (and sources acquainted with this kind of mathematics ⟦cf. article I.10⟧) were mentioned above: The presence of the square problem $_4s+Q$ and the circle problem $d+p+A = \alpha$ in the

47 Ed., trans. [Rudhardt 1978], further discussion in [Sesiano 1986], and (with an ingenious reconstruction of No. 3) in [Sesiano 1998: 278–280, 298]; probably second century CE.

Geometrica compilations, the references to the equality of area and perimeter of squares and rectangles in the *Theologumena arithmeticae.* They constitute important supplementary evidence for the link between the Greek world and the Near Eastern tradition, and confirm the observation that no trace of the specific inventions of the Old Babylonian scribe school can be found. But they are neutral with respect to the distinction between links to the core tradition and the "Seleucid-Demotic" innovations.

Adoptions III: The Proofs and Associates of *al-jabr*

In the early ninth century, al-Khwārīzmi wrote his treatise on the topic *al-jabr wa-'l-muqābalah* on the exhortation of the caliph al-Ma'mūn. It contains a chapter on practical geometry and one on inheritance computations, but none of them concern us here.[48]

As I have argued elsewhere [Høyrup 1998], the best extant witness of al-Khwārizmī's original text is Gerard of Cremona's 12th-century Latin translation [ed. Hughes 1986]; the best translation of the published Arabic text [ed. Mušarrafa and Mursi 1939] – whose branch of the stemma has undergone at least three successive revisions after its separation from the version used by Gerard – is [Rozenfeld 1983]. ⟦After this was written, Roshdi Rashed has published a critical edition with French translation[2007].⟧

The *al-jabr* technique, we are told, is based on three kinds of numbers: [square] roots, possessions,[49] and simple numbers. Fundamentally, it thus deals with [unknown] amounts of money, square roots of these amounts, and numbers; al-Khwārizmī explains, however, that the possession is produced as the product of the root with itself, in agreement with the identification of the "root" with the "thing" and of the "possession" with the product of the "thing" with itself when the technique is applied (see presently).

The three kinds of numbers are combined in 6 equation types:

48 The chapter on inheritance computation is indeed algebraic in the current sense, but all is of the first degree and based on the *šay'*, "thing" (*res* in the Latin translations, *cosa* in the Italian tradition). This kind of computation is called *regula recta* by Fibonacci [ed. Boncompagni 1857: 191 and *passim*] and introduced long before his presentation of *al-jabr wa'l-muqābalah,* namely when he solves the dressed version of Diophantos' *Arithmetica* I.15 exactly as Diophantos solves it by means of an unknown *arithmós,* "number". Unless we are deceived by a highly improbable coincidence, Arabic *šay'*- and Greek *arithmós*-algebra (which is no exclusive property of Diophantos – see [Robbins 1929] and [Vogel 1930]) belong to the same kin, and only coalesced with *al-jabr* at a late moment.

49 The Arabic word is *māl*, meaning a [monetary] property, adequately translated by Gerard as *census.* Rosen's widely used translation from [1831] has a misleading "square", but at least takes care to translate the Arabic term for a square (*murabba'*) as "quadrate" – a point too fine to be observed by most users of the text. Rozenfeld uses *kvadrat* for both *māl* and *murabba'*. ⟦Rashed uses *carré* respectively *surface carré.*⟧

> Possession is made equal to roots[50]
> Possession is made equal to number
> Roots are made equal to number
> Possession and roots are made equal to number
> Possession and number is made equal to roots
> Roots and number are made equal to possession

For each of these, a numerical example is given, and a rule for solving it (followed by non-normalized examples, whose normalization is explained). In the fourth case, for instance, the example is “a possession and 10 roots are made equal to 39 dirhams”, and the rule [ed. Hughes 1986: 234] that

> you halve the roots, which in this question are 5. You then multiply them with themselves, from which arises 25; add them to 39, and they will be 64. You should take the root of this, which is 8. Next remove from it the half of the roots, which is 5. Then 3 remains, which is the root of the possession. And the possession is 9.

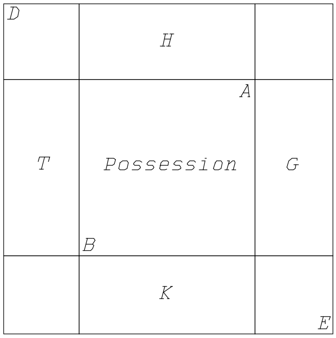

Figure 7. Al-Khwārizmī’s first proof of the case “Possession and roots made equal to number”.

Thus, if the equation is $y+a\sqrt{y}=b$, then $\sqrt{y} = \sqrt{b+(\frac{a}{4})^2} - \frac{a}{2}$ and $y=(\sqrt{y})^2$.

Working in the ambience of the House of Wisdom, al-Khwārizmī was not satisfied with a list of unexplained rules (though this may have been all the caliph had asked for), and he therefore added a set of geometrical proofs.[51] Later on he makes it clear that the geometrical proofs he constructs in order to illustrate the calculation with binomials (and which he tries, though without being satisfied with the outcome, to construct for trinomials) are of his own making; he says nothing similar about the present proofs, and already for this reason we may therefore assume that he borrowed them from somewhere.

“Somewhere” turns out to be familiar. The case whose rule was quoted above gets two proofs. The diagram of the first is shown in Figure 7. At first is drawn the square *AB* with unknown side, which represents the possession, and each of whose sides is thus

[50] The revised Arabic text speaks in the plural of “possessions”, in agreement with the habit of later Arabic algebra; since all the basic examples are normalized, we may be confident that Gerard’s singular form corresponds to the original text.

The rules as quoted by Thābit (see n. 51) also refer to a single possession.

[51] That only the rules and not the proofs belonged with the current *al-jabr* technique (that which al-Ma’mūn had asked al-Khwārizmī to expound in handy form) follows from Thābit ibn Qurrah’s slightly later Euclidean justification of the rules of *al-jabr* [ed., trans. Luckey 1941].

As we shall see (p. 100), later representatives of the “low” variety of *al-jabr* are also devoid of proofs.

equal to the root. The 10 roots are distributed equally, as the four rectangles G, *H, T* and *K*, each of which has the width $2^1/_2$. In each corner a square $2^1/_2 \times 2^1/_2$ is missing; adding these we get for the larger square a total area $39+4\cdot6^1/_4 = 39+25 = 64$; etc. All in all we get a proof of the rule

$$y = \sqrt{b+4\cdot(\tfrac{a}{4})^2} - 2\cdot\frac{a}{4},$$

– not of the one that was to be proved.

Afterwards comes a second proof, the diagram of which is shown in Figure 8 (analogous to the procedure shown in Figure 2 and to Figure 6). This time the proof fits the procedure to be proved perfectly. Both for grammatical reasons and because the proof style is more concise and formal (also compared with the proofs of the following cases), there are reasons to believe that this second proof was added by al-Khwārizmī in a later revision of the text. Even if this is not so it comes after the first, and since it is obviously more adequate, al-Khwārizmī must have had some particular, conscious or sub-conscious reason to put it in this position – and to include the first proof at all. This reason can be one of two: it may have been the one which came first to al-Khwārizmī's mind, and which he found simpler; or it may be the one with which he expected the reader to be more familiar (or both). Since the first proof is evidently derived from the solution of the problem of "four sides and quadratic area" from BM 13901 (without the quadripartition, which is likely to be a scribe school innovation), al-Khwārizmī's source for the proof in question (and then by association also the others, which are all of the habitual types) is clearly our familiar surveyors' tradition and its riddle collection. These must hence have been around.

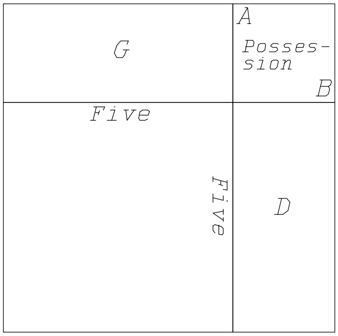

Figure 8. Al-Khwārizmī's second proof of the case "Possession and roots made equal to number".

The origin of the *al-jabr* technique is not known. Al-Ma'mūn's request suggests that it was not stock knowledge in Iraq in the early ninth century. It has been suggested at times that it came from Central Asia, since not only al-Khwārizmī but also ibn Turk, another early writer on the topic, had family roots there. A Medieval story reported by David King [1988] tells that the technique was adopted from the Iranian Fars province already under the caliph ʻUmar (634–644 CE), transmitted orally for a while and then lost, and only restored by al-Khwārizmī. Though hardly reliable in its details, the story supports a descent from a location somewhere to the east or north-east of Iraq. Supplementary arguments will be presented on p. 108 that point in the same direction.

Nor is it known whether *al-jabr* is somehow a descendant of earlier "algebras" – Babylonian, Greek "geometric" or Diophantine, Indian, or the surveyors' riddles. Once the geometric proofs and the *šay'*-technique have been seen to be secondary grafts, both

Greek sources become unlikely; since *al-jabr* is a numerical technique and not geometrical, having the possession and not the root as its basic unknown, any assumption of a link to the naïve-geometrical techniques requires stronger arguments than the similarity between the *al-jabr* "halving of the roots" and Abū Bakr's and Savasorda's "halving of the sides" (which was probably the expression nearest at hand in the absence of a formal term for the coefficient). As to a possible descent from Indian algebra as known, e.g., from Brahmagupta, Léon Rodet already argued in [1878] that the sophisticated algebraic schemes of the Indians and their free use of negative quantities make al-Khwārizmī's work look much too primitive for this hypothesis to seem plausible. Even this, however, is a question to which we shall return.

Thābit does not mention al-Khwārizmī's proofs when presenting his own, based on *Elements* II.5–6 (cf. note 51). Further on in the "high" tradition of Islamic algebra – from Abū Kāmil onward – al-Khwārizmī was the recognized founder of the discipline as it had come to look, and the geometric proofs were accepted as an integrated part of the subject (though from Thābit and Abū Kāmil onward mostly formulated with reference to *Elements* II.5–6).[52] They were evidently taken over in the Latin translations of al-Khwārizmī and Abū Kāmil, and also in Fibonacci's *Liber abbaci* and *Pratica geometrie.* With Fibonacci, however, the development is taken one step further: when paraphrasing Gerard's translation of al-Khwārizmī in the *Pratica* [ed. Boncompagni 1862: 56] he corrects the statement that numbers are "roots, possessions, and simple numbers". Now they are *aut radices quadratorum, aut quadrati, aut numeri semplices,* "either roots of squares, squares, or simple numbers" – and the squares are real geometrical squares, whereas the roots are strips as long as the side of the square, and with width 1. The formulations in the *Liber abbaci* are similar [ed. Boncompagni 1857: 406]. Here, the *census* only appears when the first problem type is introduced (*ibid.,* p. 407): "The first mode is, when the square, which is called *census,* is made equal to roots".

The *Liber abbaci* gives two versions of the geometric proof of the first mixed case; one is similar to al-Khwārizmī's second proof (see Figure 8), the other to Thābit's and Abū Kāmil's (that is, a reference to *Elements* II.6). In the *Pratica,* the paradigmatic example for the same case, here defined as "number is made equal to quadrate and roots", is nothing but the problem "a [quadratic] area and its four sides make 140" (solved however with a reference to *Elements* II.6). Regarding geometric proofs as the gist of the discipline, Fibonacci in fact reconstructs it, deriving it in part indirectly, in part directly from the old sides-and-area riddles (which he knew from Gerard's translation of Abū Bakr, from Savasorda, and from unidentified sources[53]).

52 [Dold-Samplonius 1987] is a convenient survey.

53 One trace of the latter is Fibonacci's replacement of a corrupt problem from the *Liber mensurationum* with a problem which is certainly not of his own making – cf. [Høyrup 1996a:56].

Not all Islamic *al-jabr* after al-Khwārizmī and Thābit belongs to the "high" division, even though the "low" register has attracted the attention of modern scholars and medieval translators into Latin much less. The "low" register seems to be characterized by an ordering of cases that differs from al-Khwārizmī's; by defining the cases in non-normalized form, as in the revised al-Khwārizmī text; and by *having no geometric proofs.* This is exemplified by al-Karajī's *Kāfī* [ed., trans. Hochheim 1878];[54] ibn al-Bannā''s *Talkhīs* [ed., trans. Souissi 1969: 92]; and ibn al-Yāsamīn's *Urjuza fī'l-jabr wa'l-muqābalah* (paraphrase in symbols in [Souissi 1983: 220–223]).

Common prejudice notwithstanding, this type – and not Fibonacci's *Liber abbaci* – was the kind of algebra that inspired the beginnings of Italian vernacular algebra in the earliest 14th century. This can be seen from the earliest specimens: the Vatican manuscript of Jacopo da Firenze's *Tractatus algorismi* (dated 1307);[55] Paolo Gherardi's *Libro di ragioni* from 1328 [ed. Arrighi 1987]; and a composite *abbaco* book from Lucca from c. 1330 [ed. Arrighi 1973]. But even vernacular European algebra succumbed to the spell of geometrical reasoning, and *not* in the first instance because of the influence of Fibonacci or al-Khwārizmī. Like the former in the *Pratica,* but in wholly independent and fully "naïve" and non-Euclidean terms, Piero della Francesca uses the problem of "a square whose surface, joined to its four sides, makes 140" as the paradigmatic example explaining the rule for the case "*censo* and things are equal to number" (and like Fibonacci he takes *censo* to be another term for the square figure) – see Figure 9: *TE* is told to be the square or *censo, AI* to be 4, and *G* the midpoint of *AI; GF* is then drawn so as to exceed *BE* with as much as IG; etc.[56]

Among other things, the problem in question has the sides before the area, whereas Fibonacci's own preference (strong enough to make him correct Abū Bakr) is to have the area first.

54 It may be astonishing to see al-Karajī listed in the "low" category, but the surprise may illustrate that the categorization has nothing to do with mathematical competence and incompetence; as shown by Saliba [1972], al-Karajī's terminology in the *Kāfī* demonstrates its algebra to be derived from a pre-al-Khwārizmīan model; similarly, much in its geometry turns out to be close to the traditional practitioners' model – cf. [Høyrup 1997a]. The *Fakhrī,* it should be pointed out, is different; it shows al-Karajī to be fully conversant with the geometric proofs and ready to present them when he thinks they fit the context.

55 Described in [Karpinski 1929] on the basis of the manuscript Vat. Lat. 4826. Two other copies of the manuscript exist: Ricc. 2236 (Florence) and Trivulziana 90 (Milan). An edition of the former was made by Annalisa Simi [1995]; the latter is described in [van Egmond 1980: 166*f*]. The chapter on algebra, however, is only present in the Vatican manuscript; my observations are derived from my preparation of an edition of this chapter in [Høyrup 2000b]. Jacopo's algebra is distinguished from the Latin tradition by complete absence of geometric proofs. Moreover, no single example or problem is shared with the *Liber abbaci* or with the Latin translations of al-Khwārizmī and Abū Kāmil, and no example is stated directly in the abstract form involving *censo, cosa* and number.
⟦An edition of all three manuscripts was published in [Høyrup 2007]. See also article II.11.⟧

56 Much later in the treatise comes a whole collection of problems derived from the riddle tradition:

As a rule, Piero's algebra problems are derived from the preceding vernacular tradition, not from the *Liber abbaci,* as his geometrical riddles are generally derived from some unidentified (but certainly indirect) link to the Islamic world; but his use of geometry shows that times were ripe for Pacioli's reintroduction of Fibonacci's version in *Summa de arithmetica.* In part in fairly original "naïve" shape, in part in versions more or less touched by the Euclidean "critical" form, the riddle tradition had reconquered all levels of European algebra, as Cardano was to encounter it.[57]

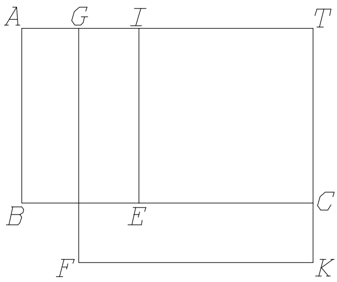

Figure 9. Piero's diagram for his "surface joined to its four sides".

So far this article has concentrated on the development of *al-jabr* and its continuation in the Christian world. Time and again, however, the argument has presupposed the survival of the geometrical riddle tradition well beyond its adoption into al-Khwārizmī's *Algebra.* Abū Bakr is likely to be a contemporary of al-Khwārizmī – perhaps slightly earlier (cf. note 13), but his manual was still at hand in Toledo when Gerard went there in the mid-12th century. Savasorda wrote some three centuries after al-Khwārizmī, and clearly did not depend on Abū Bakr. Ibn Thabāt is another century younger, and totally independent of both Abū Bakr and Savasorda.

Even the Christian world turns out to have been in repeated contact with the geometrical tradition. Fibonacci, as mentioned, draws on another representative of the tradition when replacing a corrupt passage in the Gerard-translation of Abū Bakr; Piero seems to have access to yet another representative – and even Pacioli, and probably his

$Q = 2\cdot{}_4s$; $Q = {}_4s+60$; $d–s = 6$; $A = \alpha$, $l = w+2$; etc., in versions which (when numerical parameters were not fixed by tradition) are shared neither with Abū Bakr nor (with a single exception) with Fibonacci. Piero has obviously borrowed the area-with-sides problem from this group and put it in the place where he needed it for pedagogical reasons. The two other mixed cases [ed. Arrighi 1970: 133, 136] are illustrated by the problems ${}_4s–Q = 3$ and $Q–{}_4s = 77$, similarly borrowed from the geometric collection.

⟦As shown by Enrico Giusti [1991: 64], Piero's algebra is an uncritical and messy compilation; whether Piero himself or a source of his made the borrowings is thus an open question.⟧

[57] The introductory passage of Cardano's *Ars magna* [trans. Witmer 1968: 7*f*] runs as follows:

> This art originated with Mahomet the son of Moses the Arab. Leonardo of Pisa is a trustworthy source for this statement. There remain, moreover, four propositions of his with their demonstrations, which we will ascribe to him in their proper places. After a long time, three derivative propositions were added to these. They are of uncertain authorship, though they were placed with the principal ones by Luca Paccioli. …

Cardano obviously knows some of the particular types of the *abbaco* tradition – but the names of the discipline are the geometrizers: al-Khwārizmī, Fibonacci, and Pacioli. Quite appropriate as a background for his own decomposition of the cube.

vernacular source, seems to possess some kind of information about the tradition which allows him to correct in part another corrupt passage in Fibonacci without understanding it so well that he is able to correct it fully.[58]

But the geometrical tradition has left yet another possible trace which was not mentioned so far, namely – curious as it seems at first – in a particular type of number problems that went together with the *al-jabr* tradition.

It seems never to have attracted much attention that by far the larger part of the problems to which al-Khwārizmī applies the *al-jabr* technique proper are of the type "I have divided 10 into two parts [which fulfil some arithmetical relation]", and that similar but mostly different problems also constitute a dominant group in the *Liber abbaci* (31 problems in total, to which comes a division of 10 into three and two divisions of 12 into two parts). From one point of view, the type reminds us of the staple recreational problems; the "purchase of a horse" is also present in the *Liber abbaci* in an impressive number of different versions. From another point of view, however, the two situations are fundamentally different. The horse problems differ by the number of potential buyers that are involved, and by the fractions which they ask from each other; but in as far as mathematical structure is at stake, the only important difference is that some versions are indeterminate and others determinate. The "divided 10", however, is as varied in structure as can be within the framework of second-degree algebra (with some *abbaco* writers, it also serves the formulation of higher-degree problems) – so much so indeed that one is almost tempted to speak of a particular discipline. For structural reasons, its origins must be presumed to be independent of *al-jabr* – even though it may be the availability of the *al-jabr* technique that allowed it to unfold as creatively as attested in the *Liber abbaci,* whereas, *vice versa,* the existence of this problem type may be exactly what allowed the possession-root riddles – the *al-jabr* archetype – to unfold as a genuine algebra, that is, a functionally abstract representation serving to find unknown quantities (this would explain that the problem type is so prominent in al-Khwārizmī, Fibonacci and others).

Arguments from the choice of a particular numerical parameter for a particular use – in particular if this parameter seems as "natural" a choice as 10 – are wholly gratuitous unless it can be shown, firstly, that this choice is not constrained;[59] secondly, that the same choice is not made with comparable frequency in other unconstrained cases (that is, not made just because the number itself is "remarkable" independently of its use or function).[60]

58 Cf. [Høyrup 1996a: n. 21 with preceding text, and n. 49].

59 The choices of 6 and 8 as sides of a rectangle and of 13, 14, and 15 as the sides of a scalene triangle are constrained in the sense that they constitute, respectively, the second-simplest and the simplest example with integer diagonal and height.

60 1 refer to the category of "remarkable numbers" introduced in [Høyrup 1993].

In the Old Babylonian case, it was possible to argue in this way for the significance of the value 10 for the square side; further on in the surveyors' tradition, 10 turns out to be the privileged value of the side of regular polygons, but to be rare in other functions – see [Høyrup 1997a: 90*f*].

Returning to the divided 10, one may observe that quite a few of the problems in question also make use of other unconstrained parameters; none of these are ever 10.[61] The *Liber abbaci* also contains a problem where the excess of one number over the other is given – but this excess is 6. There is thus no doubt that the choice of 10 is significant; the question is, what it signifies beyond the delimitation of the type.

The majority of the problems in both al-Khwārizmī and (in particular) the *Liber abbaci* do not look meaningful in a geometrical perspective; but some of them have the same structure as familiar geometrical riddles – if a and b designate the parts into which 10 is split, the following:

$a^2+b^2 = 58$ (al-Khwārizmī)

$ab = 21$ (al-Khwārizmī)

$a^2–b^2 = 40$ (al-Khwārizmī)

$a^2+b^2 = 62^1/_2$ (*Liber abbaci*)

Moreover, Fibonacci's splitting of 10 into $a+b+c$ requires that $a{\cdot}c = b^2$ (thus that a, b and c be in continued proportion) and that $a^2+b^2 = c^2$ (thus that the squares be in extreme and mean ratio). The same problem, only with c given instead of $a+b+c$ and formulated about the sides and the diagonal of a rectangle, is No. 51 in the *Liber mensurationum.*[62] Finally, al-Khwārizmī has the problem $a^2+b^2+(a-b) = 54$, which can be reduced to the type $c^2+d^2 = \alpha$, $c+d = \beta$ by the same trick as the reduction of the old rectangle problem $A+(l–w) = \alpha$, $l+w = \beta$ to the type $A = \alpha$, $l+w = \beta$.

If we go to a less sophisticated treatise – namely Jacopo da Firenze's *Tractatus algorismi* – we shall find three representatives of the group: $^a/_b = 100$ (fol. 36^v); $a{\cdot}b = 20$ (fol. 398^v); and $a{\cdot}b+(a–b) = 22$ (fol. 40^v); the latter two of course correspond to the riddles $A = \alpha$, $l+w = \beta$ and $A+(l–w) = \alpha$, $l+w = \beta$. A fourth problem about two abstract numbers a and b (other problems of his concern composite interest, partnership', etc.) demands that they be in the proportion 4:9, and that $a{\cdot}b = a+b$ (fol. 37^v), and is thus a determinate version of the type $A = l+w$. The last three of the four problems are thus not only geometrically meaningful but even members of the earliest group of riddles.[63]

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61 10 does occur in the *Liber abbaci,* but in a constrained situation which makes critical use of the fact that $\frac{10}{a}+\frac{10}{b}=\frac{10}{a}\cdot\frac{10}{b}$ precisely if $a+b = 10$.

62 In principle, Fibonacci might have borrowed the problem from Abū Bakr and translated it into the familiar structure of the split number 10; but this would not agree with his normally faithful use of sources.

63 The first problem, too, possesses a geometrically meaningful interpretation: to find a rectangle

The less sophisticated the source – this is the obvious conclusion – the more manifest the similarity with the geometrical riddles. A second, almost as obvious conclusion is that these riddles constitute the starting point even for the arithmetico-algebraic quasi-discipline of "splitting the 10". Since the problems are also well represented in Jacopo's treatise, clearly related to the "low" type and not otherwise influenced by al-Khwārizmī,[64] there is no reason to believe the latter to be responsible for integrating even this problem type into *al-jabr,* as he integrated the technique of naïve geometry; indeed, his explanation of the three types of numbers (where the possession is told to be produced as the product of the root with itself, see p. 96) refers to what he "found", that is, found in common use among the practitioners of the art.

When al-Khwārizmī solves these problems, his initial name for the unknown is the *šay'*, the "thing"; its square is then identified with the *māl,* after which "thing" and "root" are used indiscriminately. This suggests that the adoption of the divided 10 into *al-jabr* may be part of the general process in which the *regula recta* or *arithmós/šay'* algebra was integrated into *al-jabr*. If *regula recta, arithmós* and *šay'* -techniques are really historically linked, as suggested in note 48, it seems near at hand to connect even the numbers divided into two in Diophantos's *Arithmetica* I to the "divided ten", although his numbers are different.

Adoptions IV: India

Indian algebra is not *prima facie* a likely consanguine of any of the "Western algebras" so far discussed. However certain the connections when we discuss planetary theory and the mathematics of astronomy and the sharing of a number of "recreational problems", the algebraic schemes of Brahmagupta or the Bakhshali manuscript (see [Datta 1929: 28*f*], and [Datta and Singh 1962: II, 28–32]) are so different from anything Babylonian, Greek, or Arabic that Rodet's rejection [1878] of an Indian inspiration for al-Khwārizmī would seem to hold no less certainly the other way.

The source which makes this certainty less than certain is Mahāvīra's *Gaṇita-sāra-saṅgraha* – as it may be guessed from the repeated references to this work in the preceding pages: to the problems of square and rectangular area equal to the perimeter and to rectangle problems of the type $A = \alpha$, $2l+2w = \beta$; to the determination of the area enclosed

of given shape with given (semi)-perimeter; but it is too simple to constitute a noteworthy riddle, and hardly specific enough to be an argument in favour of borrowing. Moreover, the corresponding problem in applied arithmetic (to divide a profit etc. according to a preestablished proportion) was always more common.

64 It may be noted that Jacopo uses the concept of "restoration" (i.e., *al-jabr)* not only for additive completion (as done by al-Khwārizmī) but more broadly, in agreement with the pre-al-Khwārizmīan usage of Abū Bakr.

between four mutually touching circles; to the circle problem $p+d+A = \alpha$ (with the ratio $p:d$ assumed to be 3); and to the "Demotic" determination of the sides of a rectangle from A and d.

But there is more to it. Mahāvīra also bisects the trapezium; he makes use of and has a particular name (*bhāgānubandha* fractions, ed., trans. [Raṅgācārya 1912: 63*f*]) for the "ascending continued fractions" (expressions of the form ${}^{a}/_{p} + {}^{b}/_{q}\cdot{}^{1}/_{p} + {}^{c}/_{r}\cdot{}^{1}/_{q}\cdot{}^{1}/_{p} + \ldots$) which are found in Semitic-speaking mathematical cultures but never spread much in those which adopted it – see [Høyrup 1990d] ⟦here article I.1⟧. He also has the same formula for the determination of the height of a scalene triangle as the practical treatises of the Islamic Middle Ages, and explains the way it is derived (p. 197) – but only for inner heights (cf. note 31). The area of a circular border is determined as the average perimeter times the breadth (not very significant, but alternatives do exist).

There are also puzzling links to the indeterminate rectangle problems of *Geometrica,* chapter 24, which were not considered above, and which I shall therefore not discuss further.[65] Nor shall I pursue other features of Mahāvīra's geometry which confirm the existence of connections to the Near Eastern tradition. What was already listed should suffice – and what really needs to be asked for is rather the date of these connections, and the direction of borrowing. ⟦Cf. also the more extensive treatment of the topic in article I.4.⟧

Date first. The question is whether Mahāvīra borrowed from the Islamic world of his own – and al-Khwārizmī's – century, or repeats traditional teachings of the Jaina school going back to its bloom in the later first millennium BCE (or at the latest to the early first millennium CE).

For many reasons, a borrowing in Mahāvīra's own times seems excluded. The technical names for various methods and the copious references to masters of old who knew or devised them show that these methods had been fully naturalized and assimilated into the Jaina tradition, in a way that this conservative tradition would certainly not have done within a century or two (Mahāvīra and other Jainas still stuck to $\pi = \sqrt{10}$ as the "precise" alternative to 3, centuries after the adoption of more precise approximations in non-Jaina astronomy – cf. [Sen 1971: 161].

The circle problem $p+d+A = \alpha$, if borrowed from the medieval Near East, would have gone together with $\pi = 3^{1}/_{7}$; Mahāvīra might of course have corrected it, and assumed the handy value 3 ($\sqrt{10}$ would be too inconvenient as a coefficient). But he would definitely have had no reason to order the members in a way that points back to an epoch where

[65] Strictly speaking, the *Geometrica* problems deal with right triangles, and only those of Mahāvīra with rectangles. In general, Greek geometry – even when practical – often translated traditional rectangle problems into problems dealing with right triangles (thus also *Liber podismi* and the Geneva papyrus).

the perimeter and not the diameter was the primary circle parameter (Mahāvīra himself determines the area as $A = 3\cdot({}^{d}/_{2})^{2}$, not as ${}^{1}/_{12}\cdot p^{2}$).

As a group, finally, Mahāvīra's borrowings from the surveyors' tradition remind us much more of the limited stock that is attested in the various classical sources (from *Geometrica* to *Theologumena arithmeticae*) than of, for instance, the *Liber mensurationum.*

As to the direction of borrowing, most of the material we are speaking of was present in the Near East well before the arrival of the Aryans into India. The only inventions which may as well have been made in India as anywhere else in the region where our tradition was diffused are the determination of the height in a scalene triangle; and the indeterminate rectangle problems.[66]

Socially, the Jaina community of the first millennium BCE – with its strong representation of artisans, merchants and officials (see, e.g., [Thapar 1966: 65] – is of course the best possible candidate for a channel through which foreign practical mathematics might be adopted.

Familiarity with the naïve quasi-algebra of the surveyors' tradition does not necessarily entail that Jaina – nor, a fortiori, Indian – *algebra* in general was strongly influenced by this technique. Such a claim remains a hypothesis; all that can be said is that the hypothesis is no longer implausible, the characteristics of mature Indian algebra notwithstanding. If we look at other features of Mahāvīra's work, we may notice a strong tendency toward technical formalization (and expansion) – precisely the processes that might lead from algorithms based on a naïve area technique to the algebraic schemes of the mid-first millennium. A paradigmatic example is the treatment of *mūla* or "root" problems. These are problems arising as variations of a familiar older type, called *Bhāga* by Mahāvīra – in symbolic notation $x-\frac{1}{p}x-\frac{1}{q}x-\frac{1}{r}x=\alpha$. This structure may be varied in different ways by the inclusion of square roots – and Mahāvīra lists five specific categories, each with its own fixed rule for solution, of which I quote two:[67]

Mūla: $x - (bx+c\sqrt{x}+a)=0$

Dviragraśēṣamūla: $x-a_1-b_1(x-a_1)-b_2(x-a_1-b_1(x-a_1))-\ldots-c\sqrt{(x-a_2)}=0$

Other variations are obtained by squaring, for instance:

66 My intuition (for what it is worth!) finds the idea behind the height determination so congenial to the ancient Near Eastern tradition that an import seems implausible; the indeterminate problems, on the other hand, are unlike the main body of what we know from the Near East and the Mediterranean region (only the rules for creating Pythagorean triples and certain techniques of Diophantos are comparable), whereas indeterminate analysis is known to have been an Indian favourite in the first millennium CE.

67 I borrow Raṅgācārya's translations into symbols [1912: 75–81]. In the text, all problems are evidently concretely dressed, dealing with elephants, peacocks, swans, pearls, etc.

Bhāgasaṁvarga: $x - \frac{m}{n}x \cdot \frac{p}{q}x - a = 0$

An elaborate system of this kind is obviously not the product of an oral or semioral practitioners' culture but of a school. It presupposes the fundamental techniques for transforming an equation, together with insights permitting the solution of quadratic equations. The transformations *could* be performed within the kind of algebraic schemes which we know from other sources from the first millennium CE, but the techniques have disappeared from view in the rules given by Mahāvīra; similarly, the kind of insights by which quadratic problems were solved may well have been those coming from the surveyors' riddles, since they were actually present – but we have no means to decide.

To sum up: The Near Eastern geometric and quasi-algebraic tradition, with its riddles and characteristic techniques, did reach India, probably already in Antiquity, and the riddles and procedures made enough impression to become part of venerated traditions; but whether they remained isolated, well preserved fossils or entered decisively in the process leading to the formation of Indian algebra – this remains an open question.

Al-jabr Revisited

What can be said with some certainty about the Indian development is that the complex system of *mūla* problems will have unfolded from a simpler base of problems involving an unknown quantity, its root and a number, in the likeness of the *māl*-root problems of *al-jabr*. Such problems may have circulated as riddles among those practical professional people who were part of the early Jaina community.

Al-jabr, as it was stated, is conceptualized as a numerical technique – the basic unknown quantity is an amount of money, and the simple numbers are also numbers of *dirhams.* On this background, the metaphorical use of a term for the square root which literally means "root [of a tree]" (*jidhr*) is therefore anomalous, and only explainable if it is a calque (and thus evidence of a borrowing from a terminology where the metaphor makes sense).

Now, Sanskrit *mūla* is also a metaphor; in non-technical language it refers to the root of a tree, and to the basis or fundament of something. In mathematics it refers (at least from Āryabhata onward, but probably well before that, if we can ascribe the core of Mahāvīra's mathematics to the early Jainas) to the square root understood as "that side on which a square rests" – see [Datta and Singh 1962: I, 169*f*].

The two parallels in combination – each one taken alone already quite characteristic – makes it almost certain that *al-jabr* is somehow linked to India. Rodet's objections remain valid, which means that it is certainly not derived from the high-level algebra of Āryabhata and Brahmagupta, not even from what we find in the Bakhshālī manuscript. But it may descend from a lay riddle tradition that had also inspired the beginnings of Jaina *mūla*

algebra, or from riddles still circulating in India outside the environments of schools and advanced astronomy in the Middle Ages. Alternatively, both *al-jabr* and the simple beginnings of Indian *mūla* algebra (and the "root" metaphor) may be derived from a geographically intermediate source, centered perhaps in Iran, perhaps in Khwārezm. Since the radical reduction of a rich variation of unknown quantities to one single standardized "possession" is less probable than poetical proliferation starting from simple beginnings, an origin outside the dominion of elephants and peacocks seems somewhat more likely than an original Indian inspiration.

If this is true, the possible influence of the geometrical riddles on the development of Indian algebra tells nothing about the ultimate inspiration of the *al-jabr* riddles. *Al-jabr,* that technique which gave algebra its name, remains the only kind of second-degree algebra whose connection to the Near Eastern surveyors' riddles remains a fully unsubstantiated assumption;[68] all we may say is that if it originated together with the root metaphor, it was at first represented geometrically. As we have seen, however, already the transformation of the riddle archetype into a proper algebraic technique may be due to interaction with a descendant of the surveyors' tradition (the divided 10) – and its transformation from a technique into a reasoned mathematical discipline is almost certainly based on a borrowing of their cut-and-paste technique, first in original "naïve" form, next as transformed by Euclid.

Closing

The Romanticist folklorists invented the idea of *Gesunkenes Kulturgut* – if not derived from literate culture, folktales and other popular genres were in their view the remains of shipwrecked mythologies, of disintegrated epics which had once formed the spirit of the "people" in the sense of "nation". True culture was the creation of bards and prophets; the weary peasant telling jokes in the tavern and the uneducated wet nurse with her fairy tales were nothing but filth from which it was necessary to free the expressions of the genuine people, the "nation in itself ".

The above pages have tried to tell a very different story, about a kind of culture that was "popular" in the sense of standing outside literate high culture and thus "low" in the moral topography of the *literati,* but was certainly neither national (a-national or cosmopolitan, however, rather than international) nor the undifferentiated possession of everybody not belonging to the learned class.

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68 I have not discussed the Chinese material, but the presence of the reed-against-a-wall in almost-"Seleucid" version in the *Nine Chapters on Arithmetic* strongly suggests the existence of a connection; whether accidental or essential remains an open question.

⟦I would now argue emphatically for "accidental" – see [Høyrup 2016: 474].⟧

In an epoch where "identity" – a concept which some thirty years ago gained wide currency thanks to the feminist movement – is rampantly reduced to "national identity", I find it worthwhile to trace how the transnational culture of a group of professionals provided nourishment for a variety of exclusive high cultures – one of which, while its science is developing de facto into "world science", takes pride in that development and uses it as an argument in favor of its own perennial superiority. ⟦[69]⟧

Such was my motivation for telling the story – but not for finding it out. This happened through two decades of intellectual Brownian movement guided by curiosity and by esteem for practitioners' knowledge gained during my teaching experience in an engineering school. But its merits should certainly be evaluated with reference to its verisimilitude and independently of any disgust with our favorite pretext of the moment for sending the others to a better world. In other words: Is the story a mere fable with a moral imposed from without – is it plausible – or is it inevitable?

"Inevitable" is a pompous word, and much of what was proposed was only stated to be plausible. It may be useful to sum up (what I see as) the main relative certainties and the main doubts.

The heart of the whole argument was of course the existence and transmission of the cluster of riddle-like problems inventoried on p. 98, and the mainly lay and oral character of the carrying environment. The existence of the cluster seems subject to no reasonable doubt: so many of the characteristic problems turn up together so often that accidental independent formulation of just these problems in each other's company is practically excluded – not to mention the conserved word order and solution of "the four sides and the area" over 3300 years.

As to the carrying environment, so much seems quite certain that Babylonian "algebra", Greek "geometric algebra", and the geometric technique reflected in Abū Bakr's and al-Khwārizmī's works are not in direct communication; they are much too different, and know too little about each other's finer creations for this to be the case. The connections of which the shared relation to the cluster of riddles bears witness must be due to a social environment that was "lay" at least in the sense that it has left no direct traces in the records of scholarly traditions. To speak of this environment as forming "a tradition", in preposterous singular form, is a rash simplification, permissible only because we have no knowledge of the precise character of the environments in different areas and epochs (which was certainly not without variation), of the relation between genuine master-apprentice networks and systems based on elementary schooling, nor of the extent to which basic literacy was involved in conservation and transmission. All that follows from the analysis of mathematical and expository style is that the environment was often much closer to the style of oral culture than the various scholarly cultures which it links – but

[69] ⟦This was written in 1998. Little did I anticipate how much worse things would be two decades later.⟧

that it was never exclusively oral since the end of the Old Babylonian period, nor uniformly literate.

That Old Babylonian school algebra starts on a foundation that had been laid outside the school environment seems very certain, and also that it was only the school that transformed a basic riddle technique into a genuine mathematical discipline. Precisely how much was invented before the school took over is less easy to decide, as are third- as well as second-millennium interactions between the school and the lay environment.

The Near Eastern inspiration for the geometry of *Elements* II (if not necessarily for Greek metric geometry in general) seems well established; also well established (though not argued in detail above) is the lack of inspiration from the developed algebraic discipline of the Old Babylonian school. As to the contact points, the material suggests (but does not tell with certainty) that the traces of the surveyors' "tradition" which we find in late Ancient practical mathematics are somehow linked to the shaping it received in the Achaemenid and Hellenistic melting pot (which also seems to have influenced Mahāvīra) – whereas the inspiration of the theoretical geometers (and of that "logistician's algebra" from which Diophantos took his term *dýnamis*) seems unaffected by this new shape, and may go back to Syrian (Aramaic-speaking) contacts.

The composite nature of *al-jabr* as found in al-Khwārizmī's treatise seems subject to little doubt; that he was responsible himself for the introduction of naïve-geometric proofs is equally well established, and almost as certain is that he borrowed the proofs from the surveyors' "tradition", which can be seen not least from Abū Bakr's manual to have been at hand in his world. The distinct origin of *šay'*/"thing"-algebra and its identification with the basic Greek *arithmós*-technique was concluded from a combination of various pieces of evidence, none of which is conclusive in itself, but which only fit well together under this condition; the singling-out of the "divided ten" and related problems is similarly built on combined evidence. That both (even if originally distinct) had been adopted into *al-jabr* before al-Khwārizmī encountered it seems to follow directly from his text, but is also suggested by other data. That the assimilation of the number problems in question was the occasion for the transformation of riddles into a general algebraic art is a conjecture which fits the evidence but is no necessary conclusion. More substantiated is the linking of the *al-jabr* riddles to India, either by descent or common descent.

That Mahāvīra draws on material going back to the surveyors' tradition is hardly to be doubted – and that the connection goes back to Antiquity is a reasonable inference. However, whether this material was of general importance for the creation of Indian algebra is an open question.

Only one of the texts used above was unpublished until recently (Jacopo's algebra chapter), and only one other was published less than 20 years ago (the Old Babylonian tablet BM 80209). For the rest, the argument was built on attention to details in familiar texts and on analysis of the distribution of characteristic features. There is no reason to

believe that the possibilities of this approach are exhausted. Further work along similar lines is likely to make some of the “reasonable conclusions” drawn above more certain – to transform them into open questions – or to invalidate them altogether. The grand canvas offered in these pages is certainly in need of that.

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Chapter 5 (Article I.4) Mahāvīra's Geometrical Problems: Traces of Unknown Links between Jaina and Mediterranean Mathematics in the Classical Ages

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Abstract

The first part of the article presents, as necessary background, the development of the Near Eastern surveyors' riddle tradition, from its pre-Old-Babylonian beginnings, over innovations that may go back to the fifth or fourth century BCE, until those that can be dated to the Seleucid-Demotic periods. It also gives a brief description of its impact on Greek theoretical as well as Pythagoreanizing and so-called practical mathematics.

The second part looks at the geometry chapter of Mahāvīra's *Gaṇita-sāra-saṅgraha.* It finds clear borrowings, respecting the phases of the Near Eastern development, each of the three sections of the chapter corresponding to one of the Near Eastern phases; surprisingly, the borrowings often point more clearly to the Mediterranean offset of Near Eastern practices than to these practices themselves. The borrowings can be seen to have been regarded by Mahāvīra as part of age-old and venerated Jaina tradition – which implies that the borrowings did not come from the Islamic culture of Mahāvīra's own century.

Contents

Near Eastern and Mediterranean geometries

In various publications [Høyrup 1995; 1996; 2001] I have argued for the existence in (what Western Europe sees as) the Near East of a long-lived community of practical geometers – first of all surveyors – which was not or only marginally linked to the scribe school traditions, and which (with branchings) carried a stock of methods and problems from the late third millennium BCE at least into the early second millennium CE. The arguments for this conclusion constitute an intricate web, and I shall only repeat those of them which are of immediate importance for my present concern: the links between the geometrical section of Mahāvīra's *Gaṇita-sāra-saṅgraha* and the practical mathematics of the Mediterranean region in the classical ages.

Many of the mathematical methods of pre-Modern practical geometry are too generic to allow us to discriminate diffusion from independent creation – once area measures are based on length measures, for instance, there is only one reasonable way to determine the areas of rectangles, right triangles and right trapezia. Some formulae, it is true, are so complex and/or allow so many variations that agreement in detail appears to make accidental coincidence implausible, in particular if identical patterns turn up repeatedly in the same textual setting. The best evidence for transmission, however, is constituted by those mathematical riddles (often known as "recreational problems") that pre-Modern communities of mathematical practitioners used to define themselves cognitively and to demonstrate the professional valour of the members.

Communities which in pre-Modern times were linked only marginally or not at all to school institutions have evidently left no written evidence of their knowledge; the information we can gather about the tradition in question thus comes from comparative analysis of the written sources produced by the various literate traditions that borrowed from or were otherwise inspired by it.

The first of these is the Old Babylonian scribe school, whose mathematical texts were created between 1800 and 1600 BCE. Among the hundreds of quasi-algebraic problems of the second degree dealing with fields and their sides,[1] a small core can be identified

[1] When speaking of these as "quasi-algebraic" I refer to two characteristics. Firstly, their technique is *analytic,* as analysis is defined by Viète, "the assumption of what is searched for as if it were given, and then from the consequences of this to arrive at the truly given" – *In artem analyticen isagoge,* Ch. I [ed. Hofmann 1970: 1]. Secondly, their steps may be mapped in symbolic algebra, even though the actual technique consisted of geometrical cut-and-paste procedures. Both attributes characterize the original surveyors' riddles no less than the school descendants. The school technique, however, was also algebraic in a third sense: its lines and areas were used to *represent* entities belonging to other categories – men, workdays, and the bricks produced by the men during the workdays in question; numbers and their products; prices and profits; etc. The surveyors' riddles,

as borrowings from a pre-existing non-school tradition.[2] Four of these treat of a single square with side s and area $\square(s)$ (here and in the following, $\square(s)$ stands for the square with side s, $\sqsubset\!\sqsupset(l,w)$ for the rectangle contained by the sides l and w; ${}_4s$ stands for "the four sides", Greek letters for given numbers):

$$s+\square(s) = \alpha \qquad \square(s)-s = \gamma$$
$$_4s+\square(s) = \beta \qquad s = \square(s)+\delta$$

Four others treat of two concentric squares (sides s_1 and s_2):

$$\square(s_1)+\square(s_2) = \alpha\,, \quad s_1 \pm s_2 = \beta$$
$$\square(s_1)-\square(s_2) = \alpha\,, \quad s_1 \pm s_2 = \beta$$

Further problems deal with a rectangle with sides l and w, area A and diagonal d:

$$A = \alpha\,, \quad l \pm w = \beta$$
$$A = l+w \text{ (alone or with } (l+w)+A = \alpha)$$
$$A+(l \pm w) = \alpha\,, \quad l \mp w = \beta$$
$$A = \alpha\,, \quad d = \beta$$

One problem, finally, deals with a circle with circumference c, diameter d and area A:

$$c+d+A = \alpha$$

Later evidence suggests but does not prove definitively that a few more single-square problems circulated in the pre-school environment without appearing in the extant Old Babylonian corpus:

$$_4s = \square(s)$$
$$d-s = 4$$

Combination of Old Babylonian and later evidence suggests that the following four problems on a rectangle with given area belonged together as a fixed sequence already before 1800 BCE:

$$l = \alpha \qquad l+w = \gamma$$
$$w = \beta \qquad l-w = \delta$$

The shape in which we find the problems in the clay tablet is often slightly changed with regard to the original format (as the latter is revealed by traces in some of the Old Babylonian specimens that agree with formats that turn up later).

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in contrast, were riddles about the entities known from surveying everyday and nothing else; they did not serve representation.

[2] I restrict myself to problems of clear riddle character; this eliminates, for instance, the finding of the area and the diagonal of a square from its side – problems which are anyhow too simple to serve as argument for any borrowing.

In the original format (the "riddle format", as I shall call it) sides are referred to before the area – all riddles, indeed, tend to mention first the familiar and the active before the derived or the passive, and the lengths of sides are certainly what is immediately given to the surveyor, whereas areas are calculated and thus derived. The only coefficients of which the riddles make use are "natural" and thus not really to be understood as coefficients: *the* side or *all four* sides of a square, *the* length, *the* width or *the* sides (length and width, perhaps both lengths and both widths) of a rectangle, etc. Within the tradition of which we are speaking, the preferred value of the sides of squares and other regular polygons, moreover, is 10.

The school format, in contrast, will preferentially speak of the area before the side, anticipating the method of solution (in which areas are drawn first, and sides drawn or imagined as "broad lines" with breadth 1, to be joined to or cut out from the areas). The circle riddle $c+d+A = \alpha$ thus is changed by the school into $A+d+c = \alpha$. The infatuation of schools with drilling also calls for systematic variation of coefficients – "$^1/_3$ of the side", "$^2/_3$ of the area", "the width plus $^1/_{17}$ of the sum of 3 lengths and 4 widths", etc. – whereas the reference to "all four sides" of the square is eliminated. Finally, the compliance with Sumerian numeration and metrological tradition in the school makes 30 (meant as $30' = {}^{30}/_{60}$) the standard side of the square (and of regular polygons in general).

For the solution of rectangle problems involving the area and the side, the school as well as the surveyors made use of the semi-sum and semi-difference of the sides, more precisely of the fact that $\square(\frac{l+w}{2}) = A+\square(\frac{l-w}{2})$. Problems about squares and their sides were solved in analogous ways. The rectangle problem with given area and diagonal was reduced by means of the identity $\square(d)-2A = \square(l-w)$ to the problem $A = \alpha, \quad l-w = \beta$.

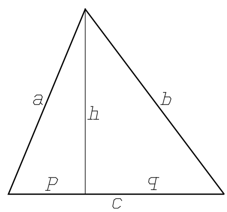

Figure 1.

In ca 1600 BCE, the Hittites made a raid against Babylon which turned out to be the final blow to the Old Babylonian social system. A consequence of the ensuing breakdown was the disappearance of the scribe school and of its sophisticated mathematics. We do not know the precise channels through which basic mathematical techniques survived, but they have plausibly been several: scribes trained within "scribal families" may have been taught something, scribal schools in the Syrian and Hittite periphery may have been involved too, but a further transmission within a non-scribal surveyors' environment of oral cultural type (though hardly not quite illiterate) is next to indubitable. A restricted number of quasi-algebraic problems turn up again in a tablet from Late Babylonian but pre-Seleucid times (perhaps c. 500 BCE) – but only the basic riddle types, without coefficients beyond the natural ones[3]. This and kindred tablets

[3] The rectangle with given area and given *w, l+w,* or *l−w;* two concentric squares, for which $\square(s_1)-\square(s_2) = \alpha, \quad s_1-s_2 = \beta$. The tablet is W 23291, ed., trans. [Friberg 1997].

are written by scholar-scribes, but discontinuities in the Sumerian translation of Akkadian words show that the riddles have survived in an environment where Sumerian was not learned.

We have no sources from Babylonia for the discovery of how the area of a scalene triangle may be calculated from the sides, but combination of Greek and medieval (mostly Arabic, but also Hebrew and Latin) practical geometries shows that the computation of the (inner) height in scalene triangles is pre-Greek and almost certainly pre-fourth century BCE.[4] The formula makes use of semi-sum and semi-difference (see Figure 1):

$$\frac{q-p}{2} = \frac{b^2-a^2}{2} \div C\ , \quad \frac{q+p}{2} = \frac{c}{2}$$

whence

$$q = \frac{c}{2}+\frac{b^2-a^2}{2} \div C\ ,\ p = \frac{c}{2}-\frac{b^2-a^2}{2} \div C$$

The probable argument behind this formula runs as follows:

$$\square(b)-\square(a) = \{\square(q)+\square(h)\}-\{\square(p)+\square(h)\} = \square(q)-\square(p)$$

$\square(q)-\square(p)$, however, is the difference between two squares, most likely to be understood as the band between concentric squares (see Figure 2):

$$\square(q)-\square(p) = \sqsubset\!\sqsupset\left(\frac{q-p}{2}, 2(q+p)\right) = \sqsubset\!\sqsupset\left(\frac{q-p}{2}, 2c\right).$$

(This argument, a "naive" version of *Elements* II.8, is found in ibn Thabāt's *Reckoners' Wealth* [ed., trans. Rebstock 1993] and in Hero's *Metrica* I.xxvi [ed., trans. Schöne 1903], and suggested in the two-square problem of W 23291 just mentioned). Therefore,

$$\frac{q-p}{2} = \frac{b^2-a^2}{2} \div C$$

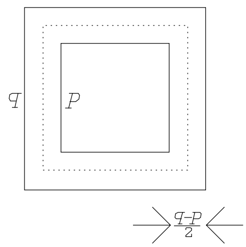

Figure 2.

Other innovations turn up more or less simultaneously in a couple of Seleucid texts and in a papyrus from Demotic Egypt;[5] the new problems and methods are not identical in the three texts, but the overlap is sufficient to show that they represent a single development – see [Høyrup 2000a; 2000b].

One innovation (only attested in the Demotic papyrus) is a new version of the rectangle with known area and diagonal: $\square(l+w)$ and $\square(l-w)$ are both found, as $\square(d)+2A$ and $\square(d)-2A$, respectively, and l and w from these without use of average and deviation ($2l =$

[4] See [Høyrup 1997: 81–85] ⟦= article I.9⟧.

[5] AO 6484, ed. [Neugebauer 1935: I, 96–99]; BM 34568, ed. [Neugebauer 1935: III, 14–17]; Pap. Cairo J.E. 89127–30, 89137–42, ed. [Parker 1972: 41–43]. The papyrus will be from the third century BCE, the two tablets may be slightly later.

$[l+w]+[l-w]$). In BM 34568, the rectangle problem with known area and $l+w$ is similarly solved from $\square(l-w) = \square(l+w) - 4A$, whence $2l = (l+w)+(l-w)$, etc. (similarly with known $l-w$ and A).

Also in the Seleucid material, we find rectangle problems where the data are $d+l$ and w; $l+w$ and d; $d-l$ and w (dressed as a "reed against a wall"-problem); $d+l$ and $d+w$; $l+w+d$ and A. Several of the geometric solutions are given by Fibonacci in his *Pratica geometrie.*

The traces we find of the tradition in Greek theoretical mathematics all point to the pre-Seleucid-Demotic phase:

Elements II.1–10 can be read as "critiques" of the pre-Old-Babylonian ways to solve for instance rectangle problems (given A and $l\pm w$, II.5 and II.6) and two-square problems with given sum of the areas (II.9–10) and given difference (II.8) – that is, as investigations of *why* and under which conditions the traditional solutions work; mostly the proofs copy the traditional procedures. II.13 is a reformulation of the fundament for the determination of the inner height in the scalene triangle which connects it to the Pythagorean theorem (I.47), while II.12 is a parallel result for the external height (almost certainly a contribution of the Greek geometers – the practical tradition seems to have considered inner heights only). In all cases where the distinction is relevant, the method is based on average and deviation.

Elements VI.28–29 and *Data* 84–85 also point to the rectangle problems where A and $l\pm w$ are given (in similar treatment).

Euclid's *Division of Figures* contains as one of the simple cases a problem that was already solved in the 23d century BCE: the bisection of a trapezium by a parallel transversal.

Diophantos *Arithmetic* I is a collection of pure-number versions of a wide range of "recreational" problems – "finding a purse", "purchase of a horse", etc. This context leaves little doubt that prop. 27–30 are arithmetical versions of the rectangle problems $A = \alpha$, $l\pm w = \beta$, and the two-square problems $\square(s_1)\pm\square(s_2) = \alpha$, $s_1+s_2 = \beta$. The solution is based on average and deviation, in contrast to all other problems of the book.

Totally absent is, not only influence from the Seleucid-Demotic innovations (the extant sources for which are contemporary with or later than Euclid) but also everything that might point to the particular contributions of the Old Babylonian school.

The situation is different if we look at the "low" tradition of Greek practical mathematics. The sources for this tradition – carried by culturally unenfranchised strata – are meagre, but not totally absent.

Firstly, there is the Neopythagorean and similar evidence, produced by philosophers whose understanding of mathematics may not have allowed them to grasp the works of the theoreticians, or whose appreciation of mathematics may simply have been derived from what will have been closer at hand than the utterly few mathematicians of renown.

⟦Cf. article I.10.⟧ The pseudo-Nichomachean *Theologumena arithmeticae* mentions[6] that the square □(4) is the only square that has its area equal to the perimeter, and Plutarch[7] tells that the Pythagoreans knew 16 and 18 to be the only numbers that might be both perimeter and area of a rectangle – namely □(4) and ⊏⊐(3,6), respectively. The first is obviously the old "all four sides equal area" square problem, and the second an "all four sides" variant of the $l+w = A$ rectangle problem. Finally, both Theon of Byzantium[8] and Proclos[9] refer to the side-and-diagonal-number algorithm, which may also be an inheritance from the Near Eastern tradition (and may even be reflected in Old Babylonian texts and have to do with the square problem $d–s = \alpha$).

Secondly, a few texts belonging to the practical tradition itself have survived which contain identifiable borrowings.

One such text is Heiberg's conglomerate *Geometrica* [ed. Heiberg 1912], one component of which (ch. **24)** contains the problem "square area plus perimeter equals 896", and two of which (ch. **24** and mss A+C) contain the circle problem $d+c+A$ (in "riddle order", but now with the diameter as the basic parameter instead of the circumference).

It is worth noticing that the *Geometrica* manuscripts share certain standard phrases with the Near Eastern tradition, two of which (the idea of "separating" for instance circular diameter, circumference and area, and the directive "always" to make a step which is independent of the actual parameters) are also found in a few Old Babylonian texts.

The Greek Papyrus Genève 259 [ed. Sesiano 1999], probably from the second century CE, has the rectangle problem (formulated about a triangle) $l+w$ and d given, and solves it in a way that is related to (though not identical with) that of the Demotic papyrus;[10] it also has the "Seleucid" problem where $w+d$ and l are given.

A Latin *Liber podismi* [ed. Bubnov 1899: 511*f*], whose very title shows it to be of Greek origin, contains a short collection of problems about right triangles. Most of the problems are too simple to tells us much. One of them, however, repeats the old rectangle problem where d and A are given. The solution follows the same pattern as the Cairo papyrus (without referring to average and deviation), and is thus in the new Demotic-Seleucid style.

6 In II.11, and again in IV.29, ed. [de Falco 1922: $11^{11–13}$, $29^{6–10}$].

7 *Isis et Osiris* 42, ed. [Froidefond 1988: 214*f*].

8 *Expositio* I.XXXI, ed. [Dupuis 1892: 70–74].

9 *In Platonis Rem publicam*, ed. [Kroll 1899: II, 24*f*]; and *In primum Euclidis Elementorum librum*, ed. [Friedlein 1873: $427^{21–23}$].

10 $\square(l-w)$ is found as $2\square(d)-\square(l+w)$.

Mahāvīra

This finally brings us to the point where we may approach Mahāvīra's 9th-century *Gaṇita-sāra-saṅgraha* [ed., trans. Raṅgācārya 1912].

At first we may simply list the features which the geometrical chapter VII of this work (but no other Indian work I have looked at) shares with the Near Eastern tradition.[11] Taken singly, some of the sharings might be accidental, others cannot be explained away in this manner; taken as a whole, the cluster is convincing evidence of a connection:

- the rectangle problem with given area and $l+w$ is solved in Demotic-Seleucid manner (VII.129½);
- the problem "area = sum of the sides" is found in square as well as rectangle version (VII.113½ and 115½);
- the rectangle in which the area and the diagonal are given is solved in the Demotic way (VII.127½);
- the rectangle problem $2l+2w = \alpha$, $d = \beta$ is solved as in the Geneva papyrus (VII.125½);
- the circle problem turns up in the shape $c+d+A = \alpha$, and the three entities are to be "separated" (VII.30);
- the inner height of a scalene triangle is determined as described above; the argument that was suggested above is also outlined (VII.49). The two-tower problem[12] is

[11] Many of the arithmetical problems are certainly also shared with the Islamic tradition (and its European descendants), but these are mostly so widespread (also within India) that they tell us nothing specifically about borrowings or their direction.

It may be noted, however, that Mahāvīra describes the system of ascending continued fractions, which to my knowledge is not found in other Indian sources, and that this type of composite fractions even has a particular name (*Bhāgānubandha* or "associated" fractions, III.113–125). This is likely to be a borrowing from a Semitic-speaking area; given the full integration into the treatment the borrowing will have taken place long before Mahāvīra's times. All in all, the ascending continued fractions may well have been taken over in the same process as that in which Seleucid-Demotic quasi-algebra was imported (see below).

Chapter VI contains a number of formulae for the summation of series, which as they stand may or may not be related to analogous formulae found Demotic-Seleucid sources. Comparison with similar formulae given by Brahmagupta and Bhaskara II (trans. [Colebrooke 1817: 290–294] and [Colebrooke 1817: 51–57], respectively) makes a link more plausible, and suggests that the greater sophistication of Mahāvīra's treatment of the topic is due to further development of an original inspiration to which Brahmagupta was closer (and of whose geographical location we can say nothing).

[12] To find the point on the ground between two towers of unequal height which is equidistant from the two peaks.

solved by reference to this procedure (VII.201½–203½), which also presupposes the same argument.

Mahāvīra is a contemporary of al-Khwārizmī, or slightly younger. One may therefore ask whether the borrowings should be located in the 9th century CE or in an earlier epoch. All the evidence speaks in favour of the latter possibility. This is illustrated by Mahāvīra's treatment of the circle problem $c+d+A = \alpha$.

Firstly, his solution presupposes that $\pi = 3$. A borrowing from Arabic mathematics without simultaneous borrowing of the approximation $3^1/_7$ is not very likely. Moreover, the problem is normalized as a second-degree problem about c. Even if Mahāvīra would have introduced a venerated π-value instead of the unhandy $3^1/_7$, he would not have made this choice, given that his own basic circle parameter (VII.19) is the diameter. Finally, Mahāvīra gives the members in riddle order with c as the basic parameter. The *Geometrica* version and the Arabic version in ibn Thabāt's *Reckoners' Wealth* are in riddle order with the diameter as the basic parameter, $d+c+A$; the Old Babylonian specimen is in school order, $A+d+c$. Mahāvīra would have had no motive to introduce the order he uses if he had depended on Arabic or late Greek sources.

The *Gaṇita-sāra-saṅgraha* as a whole contains numerous references to the tradition. For instance, VI.1 refers to "the Jinas who have gone over to the [other] shore of the ocean of Jaina doctrines, and are the guides and teachers of [all] born beings". VI.2 goes on with "Those who have gone to the end of the ocean of calculation".

The meaning of the ocean metaphor (which turns up time and again) as well as the appurtenance of Mahāvīra's mathematical masters to the group of Jaina guides and teachers becomes clear in I.17–19, where the author tells that with "the help of the accomplished holy sages, who are worthy to be worshipped by the lords of the world, and of their disciples and disciples' disciples, who constitute the well-known jointed series of preceptors, I glean from the great ocean of the knowledge of numbers a little of its essence, in the manner in which gems are [picked up] from the sea". The end of chapter I (I.70) also ascribes the whole mathematical terminology to "great sages".

Similarly, the explanation of the calculation of the height of the scalene triangle is ascribed to "learned teachers". This is hardly how Mahāvīra would refer to recent foreign inspiration.

It should be also remembered that the Jaina mathematical tradition was often very conservative by deliberate choice – Mahāvīra and other Jainas still stuck to $\pi = \sqrt{10}$ as the "precise" alternative to 3, well after the adoption of more precise approximations in ⟦Brāhmana⟧ astronomy ⟦[Sen 1971: 161]⟧.

Socially, the Jaina community of the first millennium BCE – with its strong representation of artisans, merchants and officials [Thapar 1966: 65] – is of course the best possible candidate for a channel through which foreign practical mathematics might be adopted.

Mahāvīra's chapter VII on plane geometry is divided into three sections. "Approximate measurement (of areas)" is VII.7–48; "minutely accurate calculation of the measure of areas" is VII.49–111½; "devilishly difficult problems", are treated in VII.112–232½.

This division turns out to correspond to the periodization that can be derived from the Near Eastern material – a fact which suggests imports to have been made also at different moments and in different contexts. The circle problem $c+d+A = \alpha$, clearly pre-Old-Babylonian, thus is in the first section. The determination of the height in the scalene triangle is in the second. All the rest is in the section of "devilishly difficult problems", which means that the main trunk of the import is not likely to antedate 300 BCE – a limit which might rather be 200 BCE.

We notice that the import as a whole corresponds to what is found in the Greek "low" tradition, including what is reported in Neopythagorean and related writings. In contrast, Arabic writings that draw on the ancient Near Eastern tradition do not include problems of the type "area = circumference"; they make preferential use of semi-sum and semi-difference; and they tend to think of "the two", not "the four sides" of a rectangle. If the Jaina borrowing is not directly from the Mediterranean civilization, it is at least from somewhere we do not know about but which has also affected the level of practical geometry in the Mediterranean.

One might ask whether the "Seleucid-Demotic" innovations might have arisen *in* India, for instance within the Jaina community. If not totally excluded, this seems very improbable. In Mahāvīra's work, material that is familiar in the Near East and the Mediterranean region is mixed with much more conspicuous interests that are not reflected outside India. Moreover, a text like the Seleucid "rectangle" text BM 34568 exhibits an inner coherence which makes it unlikely that this should be an elaboration of a quite restricted range of problems taken over from India; eastward diffusion of part of the Demotic-Seleucid material is much more plausible.

So far, no positive evidence has suggested that the development of Indian *algebra* was inspired by the Near Eastern ("Babylonian") geometrical tradition. Is this changed by the evidence that (pre-) Old Babylonian geometry did reach India and was remembered among the Jainas?

It cannot be excluded, but no piece of positive evidence seems to support the hypothesis. Mahāvīra's work does contain an appreciable amount of second-degree algebra, but *if* a Near-Eastern geometric inspiration had once been of importance, then everything was already reshaped beyond recognition when Mahāvīra found the material. Moreover, Mahāvīra's second-degree problems are of the type that involves an unknown quantity and its [square] root (as are the fundamental Arabic *al-jabr* problems), not a quantity and its square ⟦cf. article I.3⟧. The kind of insights by which quadratic problems are solved in the *Gaṇita-sāra-saṅgraha* may well have been gained at an earlier moment from the

solution of the surveyors' riddles, since these were actually present – but we have no means to decide.

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Chapter 6 (Article I.5) Sanskrit-Prakrit Interaction in Elementary Mathematics as Reflected in Arabic and Italian Formulations of the Rule of Three – and Something More on the Rule Elsewhere

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

Sanskrit sources from Āryabhata to Bhāskara II have a standard formulation of the rule of three. However, it is clear that mathematics must also have been spoken of and performed during this period (and before) in vernacular environments, and that the two levels must have interacted - not least because the erudite astronomer-mathematicians use commercial arithmetic as the introduction to mathematics. But we have no surviving vernacular texts.

From Brahmagupta onward, however, the standard Sanskrit formulation is supplemented by the observation that two of the known magnitudes are similar in kind, and the third dissimilar. This could be an innovation made within the Sanskrit tradition, but comparison with Arabic and Italian medieval sources seems to rule this out. Instead, it must have been current in the commercial community spanning the Indian Ocean and the Mediterranean - but since the Sanskrit scholars are not likely to have borrowed from Arabic traders, also in vernacular commercial arithmetic as practised within India. So far, the story seems simple and coherent. However, if Latin 12th–13th-century writings and sources from the late medieval Ibero-Provençal area are taken into account, loose ends turn up that show the simple story not to be the whole story.

Contents

To the friends
MAHDI ABEDLJAOUAD and ULRICH REBSTOCK

An introductory observation

Let me start with a necessary observation on terminology: *the* "*rule of three*" is a rule, not a problem type. It is a rule for solving linear problems of the type

if A corresponds to X, to what will B correspond?

The rule states in one way or the other (but with *this order* of the arithmetical operations) that the answer is $Y = (B \times X)/A$. Analysis of this "one way or the other" will be my main tool in what follows.

There has been a tendency among historians of mathematics to conflate *the rule* and the *problem type,* which has allowed them to find "the rule of three" in ancient Mesopotamia, ancient Egypt, and in the arithmetical epigrams of the *Greek Anthology.* The consequences of this can at best be understood through the folk tale motif of painting white crosses on all the doors of the town once the door of a suspect has been marked in that way: it ensures that the investigator will find nothing. So, I shall stick to etymology and reserve the name "rule" for the rule.

In consequence of this choice we do not find the rule of three in ancient Mesopotamian, Egyptian or Greek mathematics (nor in any of those traditions that were directly derived from them before the Middle Ages). It belongs to ancient and medieval India and China, and (derived from India, as we shall see) to the medieval Arabic and Mediterranean world; from the Renaissance onward it also rises to fame in central and western Europe.

India

From Āryabhata in the late fifth century CE onward, Sanskrit mathematicians use a standard terminology for the four magnitudes A, B, X, and Y – see, for instance, [Elfering 1975: 140] (Āryabhata), [Raṅgācārya 1912: 86] (Mahāvīra) and [Colebrooke 1817: 33, 283] (Bhāskara II, Brahmagupta):[1]

A: pramāṇa ("measure")	*X: phala* ("fruit")
B: icchā ("wish")	*Y: icchaphāla* ("fruit of wish")

[1] The Bakhshālī manuscript makes copious use of the rule (in particular for verifications) and refers to it by the usual name; but the only time a partial terminology turns up (X 25, ed. [Hayashi 1995: 358, cf. 439]) it is clearly different.

There is, however, a much earlier Sanskrit appearance of the rule, albeit not making use of this terminology and therefore regarded by Sreeramula Rajeswara Sarma [2002: 135] as only "a rudimentary form of the Rule of Three". It is found in both recensions of the *Vedāṅgajyotisa* and may thus go back to c. 400 BCE [Pingree 1978: 536]. It states that the "known result is to be multiplied by the quantity for which the result is wanted, and divided by the quantity for which the known result is given", which as far as its arithmetic is concerned is not rudimentary at all. As pointed out by Sarma, the descriptive terms used – *jñāna⟨ta⟩rāśi,* "the quantity that is known", and *jñeya-rāśi,* "the quantity that is to be known" – also turn up in certain later texts.

All of these sources are written in Sanskrit – with the partial exception of the Bakhshālī manuscript, whose language "though intended to be Sanskrit, has been affected to a considerable degree by a dialect or dialects not only on the phonetic level but also on the morphologic level" [Hayashi 1995: 53], and which also (as mentioned) has a deviating terminology for the rule (and uses a standardized linear organization of the terms, which the Sanskrit sources hint at but do not always draw). Āryabhata as well as Brahmagupta present the rule within the context of astronomical treatises, and Bhāskara I and II were mainly astronomers. The very fact that the mathematics they introduce while having an astronomical purpose in mind is largely commercial or otherwise economic shows clearly, however, that it is borrowed from social groups that were distinct from that of the learned Brahmins and thus speakers of some Prakrit or other vernacular.[2] Mahāvīra, as a Jaina, was already part of an environment engaged in economic life [Thapar 1966: 65], and that exactly he would write a mathematical treatise not asked for by astronomy (though in the solemn language) fits the picture.[3] So does, finally, Bhāskara I's reference to "worldly practise (*lokavyavahāra*)" in connection with his discussion of the rule of three and elsewhere [Keller 2006: I, 107, cf. 12].[4] Here, as generally, to quote [Sarma 2010: 202], "Sanskrit has […] absorbed much from the local traditions. Anthropologists recognize today that the so-called 'Little Traditions;' played a significant role in shaping the 'Great Tradition'".

One thing is to deduct that vernacular mathematics must have existed. Another thing is to conclude anything about how it looked. In India as in other places where survival of (mathematical and other) texts relied on repeated copying, non-prestigious written culture

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[2] Since distinctions and precision in this domain is already difficult for the specialist – cf. [Pollock 2006] – I, as a definite non-specialist, shall abstain from proposing any.

[3] That Mahāvīra was part of a distinct tradition is highlighted by the presence of several layers of Near Eastern/Mediterranean influence in his geometrical chapter – cf. [Høyrup 2004] ⟦= article I. 4⟧.

[4] Cf. also the reference to "worldly computations (*laukikagaṇita*)" when a problem about walking men is used to illustrate astronomical conjunction computation [Keller 2006: I, 127].

had no better survival possibility than oral culture – that is, we depend almost exclusively on indirect evidence in the shape of references and quotations in the prestigious texts.[5]

Returning to the standard Sanskrit presentations of the rule of three, one feature may be a possible reference to vernacular ways (just *barely* possible when seen in the Indian context in isolation – but as we shall see, opening of the geographical horizon changes things). According to Brahmagupta [trans. Colebrooke 1817: 283],

> In the rule of three, argument, fruit and acquisition: the first and last terms must be similar.

Bhāskara I gives a somewhat related explanation in his commentary to the *Āryabhatīya:* not, however, when commenting upon Āryabhata's text but only in connection with the first example, with reference to the linear arrangement of the three known terms [ed., trans. Keller 2006: I, 109*f*],

> this has been stated,
>
> "In order to bring about a Rule of Three the wise should know that in the dispositions
>
> the two similar (*sadṛśa*) (quantities) are at the beginning and the end. The dissimilar quantity (*asadṛśa*) is in the middle".

The initial "has been stated" suggests that Bhāskara quotes an earlier commentary, cf. [Sarma 2002: 137], while the phrase "the wise should know" seems to imply that this will be *new* knowledge for the "the wise" (the scholars?): elsewhere in the text, when "the wise" appear, they *know* (pp. 71, 109).[6]

Mahāvīra [trans. Raṅgācārya 1912: 86] explains that

[5] One might hope that the strong reliance on memorization in Indian culture would improve the situation for the permanence of oral culture, but even memorization will probably have been reserved for prestigious cultural items – or at least have been selective, as illustrated by John Warren's observation in c. 1825 of a Tamil calendar maker who computed "a lunar eclipse by means of shells, placed on the ground, and from tables memorized 'by means of certain artificial words and syllables'" [Neugebauer 1952: 253]. It is next to certain that ethnomathematical field work will still be able to find surviving sub-scientific mathematical traditions (including their riddles), but the extent to which these are faithful in details to their first-millennium ancestors will be impossible to decide unless they can be connected to parallel sources, such as the "fragments of tables of multiplication, of squares and square roots, and of cubes and cube roots [which] are in Prakrit and must have been in use in the Andhra region at some time" in a Telugu commentary [Sarma 2010: 209]; Sarma relates in parallel that "in Uttar Pradesh, elderly people tell me that they had memorized several multiplication tables of whole numbers and fractions in Vrajbhasa or in Avadhi" (still languages which belong to the second millennium – and perhaps in a form that belongs to the latest century). Everything, we observe, concerns memorized *tables.*

[6] Thus at least according to the translation – always a slippery argument, even when a literal translation has been intended.

> in the rule-of-three, *Phala* multiplied by *Icchā* and divided by *Pramāna,* becomes the [required] answer, when the *Icchā* and the *Pramāna* are similar.

Bhāskara II [trans. Colebrooke 1817: 33, Sanskrit terms added], finally, states that

> The first and last terms, which are the argument [*Pramāna*] and requisition [*Icchā*], must be of like denomination; the fruit [*Phala*], which is of a different species, stands between them.

Āryabhata had given no corresponding explanation in terms of the similar and the non-similar (nor does the Bakhshālī manuscript, but it is anyhow outside the main Sanskrit stream in terminology, as we have seen). It thus seems as if the concepts have been adopted into the Sanskrit tradition around the onset of the seventh century. The very different ways in which the Sanskrit authors insert the observation shows that they do not copy one from the other.

That similarity is mentioned by Mahāvīra is a first argument that the concern with similarity originated in a vernacular environment (in economic transactions its relevance is obvious, in astronomical pure-number calculations it does not apply directly); that Bhāskara I introduces the observation in connection with a (commercial) example points in the same direction.[7] Neither argument is more than a non-compulsory hint, however; no wonder that those who have worked exclusively on Indian material have never been taken aback by the seventh-century introduction of what might be nothing but a reasonable mathematical observation.

Late medieval Italy

Things look different, however, if we take the Italian "abbacus" school and its mathematics into account. The abbacus school was a school mainly for merchants' and artisans' sons, who frequented it for two years or less around the age of 12, learning about calculation with Hindu-Arabic numerals and in general about basic commercial arithmetic – not least about the rule of three. The earliest references to the institution are from the 1260s, and the earliest textual witnesses of its mathematics from the outgoing 13th century.

One of the earliest formulations – perhaps the earliest one – presents the "rules of the three things"[8] as follows in literal translation [Arrighi 1989: 9, trans. JH]:

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7 True, as a referee insists, the observation is likely to be quoted from a scholar and not directly from vernacular parlance; but since this scholar refers to it as something "the wise" *should* but apparently do not know, it appears either to be his own invention or a borrowing from non-scholars, and in any case to be related to a practice where the distinction similar/non-similar applies.

8 *Le regole delle tre cose* – plural because separate rules are given according to the absence or presence of fractions.

The treatise in question is a *Livero de l'abbecho,* known from a 14th-century copy in the manuscript Florence, Ricc. 2404 [ed. Arrighi 1989]. Because of misinterpretation of copied internal

> If some computation was said to us in which three things are proposed, then we shall multiply the thing that we want to know with the one which is not of the same (kind), and divide in the other.[9]

Exactly the same formulation of the rule (except that multiplication is "against", not "with") is found in an anonymous *Liber habaci* [ed. Arrighi 1987b: 111] which can be dated to c. 1310.[10] Already because this treatise uses no Hindu-Arabic but only Roman numerals (and fractions written with words), we may be sure that it is not derived from the *Livero*.

Two versions exist of Jacopo da Firenze's *Tractatus algorismi,* originally written in 1307 but known from three 15th-century copies. In one of these (Vatican, Vat. lat 4826) the rule of three is slightly more elaborate [ed., trans. Høyrup 2007: 236*f*, error corrected]:

> If some computation should be given to us in which three things were proposed then we should always multiply the thing that we want to know against the one which is not similar, and divide in the third thing, that is, in the other that remains.[11]

The first example given runs as follows (*tornesi* are minted in Tours, *parigini* in Paris):

> I want to give you the example to the said rule, and I want to say thus, vii *tornesi* are worth viiii *parigini.* Say me, how much will 20 *tornesi* be worth. Do thus, the thing that you want to know is that which 20 *tornesi* will be worth. And the not similar is that which vii *tornesi* are worth, that is, they are worth 9 *parigini.* And therefore we should multiply 9 *parigini* times 20, they make 180 *parigini,* and divide in 7, which is the third thing. Divide 180, from which results 25 and $^5/_7$. And 25 *parigini* and $^5/_7$ will 20 *tornesi* be worth.[12]

The other two manuscripts (Milan, Trivulziana MS 90, and Florence, Riccardiana MS 2236), probably representing a revised version [ed. Høyrup 2007: 419, trans. JH], introduce the rule "If some computation should be *said* [*deta*] to us" (not "given"), while the rule

evidence, the treatise has been wrongly dated to 1288–90. It is likely to be somewhat but not very much later [Høyrup 2005: 27–28, 47].

9 Se ce fosse dicta alchuna ragione ella quale se proponesse tre chose, sì devemo moltiplicare quilla chosa che noie volemo sapere con quella che non è de quilla medessma, a partire nell'altra.

10 Gino Arrighi's ascription to Paolo Gherardi (*fl.* 1328) is safely disregarded.

11 Se ci fosse data alcuna ragione nela quale se proponesse tre cose, sì debiamo multiplicare sempre la cosa che noi vogliamo sapere contra a quella che non è simegliante, et parti nela terza cosa, cioè, nell'altra che remane.

12 Vogliote dare l'exemplo ala dicta regola, et vo' dire chosì, vij tornisi vagliono viiij parigini. Dimmi quanto varranno 20 tornisi. Fa così, la cosa che tu voli sapere si è quello che varranno 20 tornisi. Et la non simegliante si è quello che vale vij tornisi, cioè, vagliono 9 parigini. Et però dobiamo multiplicare 9 parigini via 20, fanno 180 parigini, et parti in 7, che è la terza chosa. Parti 180, che ne viene 25 et $^5/_7$. Et 25 parigini et $^5/_7$ varrano 20 tornesi.

itself turns around the final phrase, which becomes "… divide in the other, that is, in the third thing";[13] their formulation of the example only differs from that of the Vatican manuscript by using the phrase "the one which is not of the same"[14] (that of the *Livero* etc.) instead of "the one which is not similar".

Jacopo, an emigrated Florentine, wrote his treatise in Montpellier. Paolo Gherardi wrote his *Libro di ragioni* in the same place in 1328. *His* formulation is that of the *Livero* and of the *Liber habaci*. So is that of Giovanni de' Danti's *Tractato d l'algorisimo* from 1370 [ed. Arrighi 1987a: 29], even though it copies much of its introduction (a high-flown general praise of knowledge) from Jacopo.[15]

The examples of all these treatises differ; their shared formulation of the rule is thus not the consequence of one author copying from the other; it must represent a formulaic expression which was in general circulation. It remained so for long – it is still found in the first printed commercial arithmetic (*Larte de labbaco,* also known as the "Treviso arithmetic" from 1478[16]), while Luca Pacioli presents us with a slight pedagogical expansion in the *Summa de arithmetica* [1494: fol. 57ʳ, trans. JH], already present except for the words in {…} in his Perugia manuscript from 1478 [ed. Calzoni & Gavazzoni 1996: 19*f*]:

> The rule of 3 says that the thing which one wants to know is multiplied by that which is not similar, and divided by the other {which is similar}, and that which results will be of the nature of that which is not similar, and the divisor will always be of the similitude of the thing which one wants to know.[17]

In both cases an alternative follows:

> The same in other words. The rule of 3 says that the thing which is mentioned twice [*A* and *C* in the above letter formalism] should be looked for, of which the first is the divisor, and the second is multiplied by the thing mentioned once [*B*], and this multiplication is divided by the said divisor, and that which results from the said division will be of the

[13] … partire nel'altra, cioè nella terza cossa.

[14] quella che nonn'è di quella medesima.

[15] It is of course possible that Jacopo copies from an unknown earlier source, which might then just be shared by de' Danti (and the many others who have the same introduction). Given Jacopo's early date this is not very likely (but anyhow unimportant for our argument).

[16] Unpaginated, at least in my digital facsimile; but pp. 61*f* if the title page is page 1. In the end this treatise adds to the rule that the result will be of the nature of the non-similar thing, while the divisor will be similar.

[17] La regola del 3 vol che se multiplichi la cosa che l'homo vol saper per quella che non e simigliante e partire per l'altra {che e simigliante} e quel che ne vene si ene de la natura de quella che non è simigliante {e sira la valuta de la cosa che volemo inquirere}. E sempre el partitor convien che sia de la similitudine de la chosa che l'homo vol sapere.

> nature of the thing mentioned once, and so much will the thing be worth {precisely} which we try to know.[18]

Jacopo and Pacioli were not the only ones to insert pedagogical expansions. Another example is found in Pietro Paolo Muscharello's *Algorismus,* written in Nola[19] in 1478 [ed. Chiarini et al 1972: 59, trans. JH]:

> This is the rule of 3, which is the fundament for all commercial computations. And in order to find the divisor, always look for the similar thing, which is mentioned twice, and one of these will be the divisor, and I say that it will be the one which is not your request, and this your request you will get by multiplying with the other not similar thing, and this multiplication [i.e., product] you will have to divide by your divisor, and from it will come that which you will require.[20]

As we see, Pacioli's reference to "the thing which is mentioned twice" is inserted here in the standard formula.

A last formulation to look at is found in Paolo dell'Abbaco's mid-14th-century *Regoluzze* [ed. Arrighi 1966: 31, trans. JH], which does not formulate the rule as a merely arithmetical algorithm but prescribes a 2×2 organization on paper:

> If you want to calculate, that is, to make computations of sale and purchase, write the thing [*materia*] in front of its price, and the similar below the similar; and then multiply these two numbers that are askew, and always divide by the number which is beside.[21]

As we see, not only the standard formula and its variations but also this practical prescription all circle around the concepts of the dissimilar and the similar.

Before we leave the Italian corpus, one weird aspect of the standard formula might be taken note of. The reference to *C* as "the thing that we want to know" is misleading: *C* itself is known, and that which we want to know is its counterpart (as made clear in the *Vedāṅgajyotiṣa*). But it would be the perfect translation of *icchā* or some corresponding

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[18] Idem sub aliis verbis. La regola del 3 vol che se guardi la cosa mentovata doi volte de le quali la prima è partitore, e la seconda se moltiplica per la chosa mentoata una volta. E quella tal multiplicatione se parta per ditto partitore. E quello che ne vien de ditto partimento sira de la natura de la cosa mentovata una volta. E tanto varrà la chosa che cercamo sapere {aponto}.

[19] In Campania, and thus outside the native ground of the abbacus school, which may be the reason that it replaces the standard formula by an explanation.

[20] Questa si è la regula del 3, la quale è fondamento di omne ragione mercatantescha. Et per trovare il partitore guarda sempre la cosa simigliante la quale si è nominata dua fiate e una di quelle si serrà il partitore, e dico che serrà quella cosa la quale non serra tua dimanda e quella tua dimanda l'averrai ad multiplicarc coll'autra cosa non simigliante et quella multiplicatione averrai ad partire per lo tuo partitore e verràne quello che dimanderai.

[21] Se vuoli chalculare, cioè fare ragione di vendita o di conpera; scrivi la materia di rinpetto al suo pregio, e lla simile sotto la simile; e poi multiplicha quegli due numeri che stanno alla schisa, e parti per lo numero ch'è nel canto senpre.

vernacular Indian term; a loan translation is thus possible, though not very likely as long as no intermediate evidence connects the two.

Does any such intermediate evidence link the Indian and the Italian references to the similar/dissimilar?

Arabic sources

A first impression of the earliest extant Arabic description of the rule of three – the chapter on commercial transactions in al-Khwārizmī's *Algebra* from c. 820 CE – does not support any idea of transmitted formulations. It uses no name for the rule which might correspond to the Sanskrit or Italian reference to three things (actually, no name at all), and according to the best known modern translations it seems to build on the theory of proportions of *Elements* VII.

Frederic Rosen [1831: 68] translates as follows:

> You know that all mercantile transactions[22] of people, such as buying and selling, exchange and hire, comprehend always two notions and four numbers, which are stated by the enquirer; namely, measure and price, and quantity and sum. The number which expresses the measure is inversely proportionate to the number which expresses the sum, and the number of the price inversely proportionate to that of the quantity. Three of these four numbers are always known, one is unknown, and this is implied when the person inquiring says *how much?* and it is the object of the question. The computation in such instances is this, that you try the three given numbers; two of them must necessarily be inversely proportionate the one to the other. Then you multiply these two proportionate numbers by each other, and you divide the product by the third given number, the proportionate of which is unknown. The quotient of this division is the unknown number, which the inquirer asked for; and it is inversely proportionate to the divisor.

Roshdi Rashed agrees in his French translation [2007: 196] with Rosen that the translation must be made in terms of proportion theory but disagrees with Rosen in how to make the connection:

> Sache que toutes les transactions entre les gens, de vente, d'achat, de change <de monnaies>, de salaire, et toutes les autres, ont lieu selon deux modes, et d'après quatre nombres prononcés par le demandeur, qui sont: quantité d'évaluation, taux, prix, quantité évaluée.
>
> Le nombre qui est la quantité d'évaluation n'est pas proportionnel à celui qui est le prix. Le nombre qui est le taux n'est pas proportionnel au nombre de la quantité évaluée, et, parmi ces quatre nombres, trois sont toujours évidents et connus, et l'un d'eux est inconnu, qui, dans les termes de celui qui parle, est « combien », et qui est l'objet du demandeur.
>
> On l'infère ainsi ; tu examines les trois nombres évidents; il est nécessaire que, parmi eux, il y en ait deux, dont chacun n'est pas proportionnel à son associé. Tu multiplies

22 The Arabic word is *mu'āmalāt,* referring to the economic transactions of social life in general, not only trade.

> les deux nombres évidents non proportionnels l'un par l'autre; tu divises le produit par l'autre nombre évident, dont <l'associé> non proportionnel est inconnu ; ce que tu obtiens est le nombre inconnu cherché par le demandeur, et qui n'est pas proportionnel au nombre par lequel tu as divisé.

Where Rosen finds "inversely proportionate", Rashed thus sees "not proportional". Neither makes much sense mathematically. The third modern translation, made by Boris Rozenfeld [1983: 45] therefore translates the critical term *mubāyin* neither as "not proportional" nor as "inversely proportional" but as *protiv,* "opposite", probably thinking of a graphical 2 × 2-scheme as described by Paolo dell'Abbaco. Both 12th-century Latin translations, due respectively to Robert of Chester [ed. Hughes 1989: 64] and Gerard of Cremona [ed. Hughes 1986: 255], do the same, and Robert refers explicitly to the scheme. In their time, indeed, this scheme was well known, as made obvious by its use in the *Liber abbaci* (see below, note 29).

Unfortunately, nothing in al-Khwārizmī's text suggests that he thought about such a scheme – but fortunately, a much more meaningful translation of *mubāyin* can be given [Wehr 1985: 131] – namely "different (in kind)" (or "dissimilar"), as also indicated by Mohamed Souissi [1968: 96], with reference to precisely this passage. Al-Khwārizmī's terminology is thus related both to what turns up in India from Bhāskara I and Brahmagupta onward and to what we find in late medieval Italy.

Quite a few later Arabic authors do refer to the Euclidean theory – sometimes integrating it with the presentation of the rule of three, sometimes keeping the two topics separate though as neighbours. Al-Karajī's *Kāfī fi'l ḥisāb* (c. 1010 CE) is an example of separate treatment [ed., trans. Hochheim 1878: II, 15–17, English JH]:

> Chapter XLII. *Proportions*. Of the four magnitudes of the proportion, the first relates to the second as the third to the fourth. If you have found this correlation, then you obtain through interchange of the members that the first relates to the third as the second to the fourth. Further you also obtain, when combining, a proportion: the sum of the first and the second member relates to the second member as the sum of the third and the fourth member to the fourth. Further you may form differences […].[23]
>
> If the first member is unknown, then you multiply the second by the third member and divide by the fourth. Similarly, if the fourth member is unknown, you divide this product by the first member. If the second or the third is unknown, then you multiply the first by the fourth and divide the product by the known one of the other two members.
>
> If three numbers form a proportion […]
>
> Chapter XLIII. *Commercial transactions.* Know that in questions about commercial transactions you must have four magnitudes, which are pairwise similar, the price, the measure, the purchase amount and the quantity.[24]

[23] The set of operations performed here presupposes that all four magnitudes are of the same kind. Al-Karajī thus has the good reasons of a good mathematician to keep proportions and rule of three apart.

[24] Corresponding, respectively, to Rosen's "price", "measure", "sum" and "quantity".

> The price is the value of a measuring unit that is used in trade […].
>
> […] Of these four magnitudes, three are always known, and one is unknown. You find the unknown magnitude by multiplying one of the known magnitudes, for instance the sum or the quantity, by that which is dissimilar to it, namely the measure or the price, and dividing the outcome by the magnitude which is of the same kind. What comes from it is the result.
>
> Or if you prefer, put one of the known magnitudes, for instance the quantity or the sum [called the "purchase amount" a few lines earlier] in relation to the one that is similar to it [i.e., find their ratio], and thereby search the relation of the non-similar magnitude.[25]

It is clear already from the order of the four magnitudes involved (and corroborated by the whole formulation) that al-Karajī does not copy from al-Khwārizmī's exposition. This corresponds well to the presentation of algebra later in the treatise, which appears to draw on a pre-Khwārizmian form of that technique.[26]

Ibn al-Bannā' 's concise *Talkhīs* (early 13th century CE) integrates the rule in the presentation of proportions (translated from [Souissi 1969: 87*f*]):

> The four proportional numbers are such that the first is to the second as the third to the fourth.
>
> The product of the first with the fourth is equal to the product of the second with the third.
>
> When multiplying the first by the fourth and dividing the product by the second, one obtains the third. […]
>
> Whichever is unknown among these numbers can be obtained by this procedure from the other three, known, numbers. The method consists in multiplying the isolated given number, dissimilar from the two others, by that whose counterpart one ignores, and dividing by the third known number. The unknown results.

While al-Karajī and ibn al-Bannā' are usually counted as "mathematicians", ibn Thabāt was primarily a legal scholar, and his *Ghunyat al-ḥussāb* ("Treasures of the Calculators", from around 1200 CE) is intended to teach such mathematics as could serve legal purposes. Even here, proportion theory and rule of three are integrated. The rule is stated thus (translated from [Rebstock 1993: 45]):

> The fundament for all *mu'āmalāt*–computation is that you multiply a given magnitude

[25] In our letter symbols, $Y = (^B/_A)\cdot X$. As we observe, this is not the rule of three but an alternative, here and in other Arabic works (e.g., ibn Thabāt, ed. [Rebstock 1993: 43]) called "by *niqba*", "by relation". It has the advantage of being intuitively easier to grasp, but the disadvantage of performing the division first, which will mostly increase rounding errors or entail difficult multiplications of fractional quantities.

[26] This follows in particular from his use of the key terms *al-jabr* and *al-muqābalah,* see [Høyrup 2007: 157], cf. [Saliba 1972].

> by one which is not of the same kind, and divide the outcome by the one which is of the same kind.

This rule, as we see, is quite close to the one that was taught in the Italian abbacus school. However, it precedes the earliest abbacus treatises by at least half a century, and in any case it is difficult to imagine that a scholar teaching in the Baghdad madrasa should have direct access to what went on in Italy. We may safely assume that the rule he knew was widespread in a commercial community spanning at least the whole region from Iraq to ibn al-Bannāʾ 's Maghreb, and almost certainly also the traders of the Mediterranean as well as the Indian Ocean. Everywhere, it tended to penetrate even erudite presentations of the same subject-matter as a secondary explanation. *However, what penetrated the presentations of Brahmagupta, Mahāvīra and Bhāskara I and II can hardly have been the language of Arabic traders; it must have been the ways of autochthonous Indian merchants and public officials speaking a Prakrit.*

Latin presentations

This part of my story – the one that may convey information about Indian usages – turns out in the end to be, or at least to look, quite simple. However, this simplicity results from disregard of those features of the process that point away from it. Taking them into account will not refute the simple story, but they will show that there is more to the matter.

As we have seen, the two Latin translations of al-Khwārizmī's *Algebra* made in Iberian area in the 12th century both misunderstand his reference to the similar and dissimilar, but apart from that they are faithful to the original. Two other 12th-century Latin works from the same area, the *Liber mahameleth* [ed. Vlasschaert 2010: II, 185, trans. JH] and the so-called "Toledan *Regule*" [ed. Burnett, Zhao & Lampe 2007:155] have an approach which I know from nowhere else.[27] Of four numbers in proportion, the first and the fourth are declared "partners" (*socii*), and so are the second and the third. If one is unknown, then its partner shall divide any of the other two, and the outcome be multiplied by the third number – that is, the *nisba* approach or the seemingly similar rule $Y = ({}^{X}/_{A})\cdot B$.[28] Afterwards, both specify differently (without observing that there is a

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[27] The two texts are generally related, see [Burnett, Zhao & Lampe 2007: 145]. Nothing therefore forces us to believe that this shared peculiarity of theirs reflects a widespread pattern. (There is a slight similarity but nothing more with Abraham ibn Ezra's *Sefer ha-mispar* [ed, trans. Silberberg 1895: 49].)

Even though the title *Liber mahameleth* clearly shows that the work is intended to present the *muʿāmalāt* genre, this is not the only point where this treatise seems to explore matters on its own.

[28] This rule, corresponding to what was done in Mesopotamia, in Ancient Egypt and in Greek practical mathematics, will be in conflict with the Euclidean formulation in all practical applications, since *A* and *X* will not be "similar". It was therefore avoided by those Arabic authors who wanted

difference), namely in agreement with the naked rule of three,

> thus, if three are proposed and the fourth is unknown, multiply the second in the third, and divide what results by the first, and what comes out will be the fourth.

After a new headline "Chapter on buying and selling", the *Liber mahameleth* [ed. Vlasschaert 2010: II, 186, trans. JH] repeats, but now recognizes that the methods are alternatives:

> When in buying or selling it is asked about something what is its price.
>
> Do thus: Multiply the middle [number] by the last, and divide the product by the first.
>
> Or divide the middle by the first, and what comes out of it multiply by the last, or divide the last by the first, and what comes out of it multiply by the middle. From all these modes results the unknown that is asked for.

The presentation of the matter in Fibonacci's *Liber abbaci* [ed. Boncompagni 1857: 83*f*, trans. JH] (from 1228, but at least this passage is likely to be close to the lost 1202 edition) looks like Fibonacci's personal way to describe what he has seen in use:

> In all commercial exchanges [*negotiationes – mu'āmalāt?*], four proportional numbers are always found, of which three are known, but the remaining unknown. The first of these three known numbers is the number of sale of any merchandise, be it number, or weight, or measure [explanatory examples]. The second, however, is the price of this sale [...]. The third, then, will be the sale of some quantity of this merchandise, whose price, namely the fourth, unknown number, will not be known. Therefore, in order to find the unknown number from those that are known, we give a universal rule for all cases, namely, in the top of a board write the first number to the right, namely the merchandise.[29] Behind in the same line you posit the price of the same merchandise, namely the second number. The third too, if it is the merchandise, write it under the merchandise, that is, under the first, And if it is the price, write it under the price, that is, under the second. In this way, as it is of the kind of that under which it is written, thus it will also be of the quality or the quantity, whether in number, in weight or in measure. This is, if the superior number, under which one is writing, is a number [of *rotuli*[30]], itself will also be *rotuli,* if pounds, pounds, [...]. When they are described thus, it will be obvious that two of those that are posited will always be contrary [*ex adverso*], which have to be multiplied together, and that if the outcome of their multiplication is divided by the third number, the fourth, unknown, will doubtlessly be found.[31]

to base their calculations on Euclid.

29 This prescription corresponds to inscription on an Arabic dust- or clayboard (*takht* respectively *lawha*) – in agreement with the start from the right. Robert's and Gerard's translation of *mubāyin* as "opposite" shows that they think of the same scheme.

Comparing with Paolo's *Regoluzze* (above, p. 139), we observe that rows and columns are interchanged.

30 A weight unit, see [Zupko 1981: 228].

31 In omnibus negotiationibus quattuor numeri proportionates semper reperiuntur, ex quibus tres

As we see, Fibonacci knows the reason for speaking of the similar and the dissimilar, but it does not enter his prescription (which it would strain normal language to call a "rule").

Much later in the work, namely within the long chapter 12 consisting of mixed problems [ed. Boncompagni 1857: 170, trans. JH], we find a problem which is solved by means of the rule of three but which *prima facie* seems to have nothing to do with a general presentation of that rule:⟦[32]⟧

> If it is asked about 6, to which number it has the same ratio [*proportio*] as 3 to 5, you do thus: Multiply 5 by 6, it will be 30; which divide by 3, 10 comes out of it, which is the number asked for; because as 3 is to 5, thus 6 is to 10. To be sure, we usually pose this question differently in our vernacular [*ex usu nostri vulgaris*]: namely that if 3 were 5, what then would 6 be? And just as it was said, 5 is similarly multiplied by 6, and the outcome divided by 3.[33]

A similar problem follows, also given afterwards in "vernacular" terms. A total listing of the occurrences of the terms *vulgaris/vulgariter* in the work leaves no doubt that it refers to the usage of the precursor-environment for the abbacus school, the community of commercial calculators working around the Mediterranean.

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sunt noti, reliquus uero est ignotus: primus quidem illorum trium notorum numerorum est numerus uenditionis cuiuslibet mercis, siue constet numero, siue pondere, siue mensura […]. Secundum autem est pretium illius uenditionis […]. Tertius uero quandoque erit aliqua eiusdem uendite mercis quantitas, cuius pretium, scilicet quartus numerus, ignoratur; et quandoque erit aliqua similis quantitas secundi pretii, cuius merces, scilicet quartus ignotus numerus, iterum ignorabitur. Quare, ut ignotus numerus per notos reperiatur, talem in onmibus tradimus regulam uniuersalem, uidelicet ut in capite tabule, in dextera parte scribas primum numerum, scilicet mercem; retro in eadem linea ponas pretium ipsius mercis, uidelicet secundum numerum; tertium quoque si fuerit mercis, scribe eum sub merce, scilicet sub primo; et si fuerit pretiun, scribe eum sub pretio, uidelicet sub secundo; ita tamen, ut sicut fuit ex genere ipsius, sub quo scribendum est, ita etiam sit ex qualitate uel ex quantitate ipsius in numero, uel in pondere, uel in mensura; hoc est si superior numerus, sub quo scribendus est, fuerit numerus ipsorum, et ipse similiter fiat rotulorum; si librarum, librarum; […]. Quibus ita descriptis, euidentissime apparebit, quod duo illorum positi erunt semper ex aduerso, que insimul multiplicentur, et summa multiplicationis eorum, si per reliquum tertium numerum diuidatur, quartus ignotus nimirum inuenietur.

32 ⟦This passage is also in what Enrico Giusti has now identified as the 1202-version of this chapter [ed. Giusti 2017: 30].⟧

33 Si queratur de 6, ad quem numerum eandem habeat proportionem, quam 3 ad 5, sic facies. Multiplica 5 per 6, erunt 30; que diuide per 3, exibunt 10, que sint quesitus numerus; quia sicut 3 sunt ad 5, ita 6 sunt ad 10. Solent enim ex usu nostri uulgaris hanc eandem questionem aliter proponere: uidelicet ut si 3 essent 5; quid nam essent 6: et cum ita proponitur, multiplicantur similiter 5 per 6, et diuiditur summa per 3.

The Iberian Peninsula and Provence

This may seem strange: so far we have encountered nothing similar to this presumed "vernacular" way. But this is only because we did not look at Ibero-Provençal material apart from what was written in Latin during the 12th century, nor at what is probably the very earliest Italian abbacus book.

Disregarding chronology, let us start in 1482 with Francesc Santcliment's Catalan *Suma de la art de arismetica*. It introduces the *regla de tres* in these words [ed. Malet 1998: 163, trans. JH]:

> It is called properly the rule of three, since within the said species 3 things are contained, of which two are similar and one is dissimilar. This said species is common to all sorts of merchandise. There is indeed no problem nor question, however tough it may be, which cannot be solved by it once it is well reduced.
>
> And in our vernacular [*nostre vulgar*] the said species begins: If so much is worth so much, what will so much be worth?
>
> The solution of this rule is commonly said: Multiply by its contrary and divide by its similar.[34]

First, of course, we observe the reference to the "vernacular" connected to almost the same phrase (though no longer "counterfactual", one thing "being worth" another one abstractly, not "being" a different thing). There are also references to the "similar" and the "dissimilar", but the "common" formulation of the solution "Multiply by its contrary and divide by its similar" does not coincide precisely with the Italian standard abbacus rule. In spite of the shared reference to the vernacular, everything remains so different from the text of the *Liber abbaci* that any copying or direct inspiration from that work can be excluded.

A thorough inspection of all known commercial arithmetics of abbacus type written in Ibero-Provençal area until 1500 will show that they share the counterfactual or abstract "being-worth" formulation of the rule (from now on in chronological order).

The earliest of these treatises is a Castilian *Libro de arismética que es dicho alguarismo,* known from an early-16th-century copy of an original written in 1393. Some aspects call to mind the *Liber mahameleth,* showing the *Libro … dicho alguarismo* to be partially rooted in an Iberian tradition going back to the Arabic period – especially

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[34] [S]egueix se la sisena specia: que s nomena regla de tres. Diu-se propriament pertant regla de tres: per quant dins la dita specia se contenen 3 coses. de les quals les dues son semblants e la una es desemblant. La qual specia es general en tota mercaderia. Car no es nenguna rao ne questio per fort que sia: que per aquesta specia essent be reduida no sia absolta.

E comenþa la dita specia en nostre vulgar si tant val tant: que valra tant.

La absolucio de aquesta regla que comunament se diu multiplica per son contrari e parteix per son semblant.

the use of "ascending composite fractions" ($^a/_n$ and $^b/_p$ of $^1/_n$ and…). Most aspects, however, and in particular the presentation of the rule of three [ed. Caunedo del Potro & Córdoba de la Llave 2000: 147, trans. JH] are wholly different. This presentation combines the counterfactual with the abstract "being worth", and has no hint of a graphical organization in a 2 x 2-scheme (instead, the same linear organization is used as in the Bakhshālī manuscript, but this is too close at hand to be taken as evidence of any link):

> This is the 6th species, which begins "if so much is worth so much, what will so much be worth".
>
> Know that according to what the art of algorism commands, to make any calculation which begins in this way, "if so much was so much, what would so much be?", the art of algorism commands that you multiply the second by the third and divide by the first, and that which comes out of the division, that is what you ask for. As if somebody said, "if 3 were 4, what would 5 be?", in order to do it, posit the figures of the letters[35] as I say here, the 3 first and the 4 second and the 5 third, 3, 4, 5, and now multiply the 4, which is the second letter, with the 5, which is the third, and say, 4 times 5 are 20, and divide this 20 by the 3, which stands first, and from the division comes $6^2/_3$, so that if they ask you, "if 3 were 4, what would 5 be?", you will say $6^2/_3$, and by this rule all calculations of the world are made which are asked in this way, whatever they be.[36]

Next in time comes the "Pamiers Algorism" from c. 1430 [Sesiano 1984: 27]. Jacques Sesiano offers a partial edition only, for which reason I cannot quote the whole introduction – but he does show [1984: 45] that it follows the pattern "$4^1/_2$ is worth $7^2/_3$, what is $13^3/_4$ worth?".

The anonymous mid–15th-century Franco-Provençal *Traicté de la praticque d'algorisme* also follows the same general pattern but is never so close to the others that direct copying can be suspected. Its presentation of the rule of three [ed. Lamassé 2007: 469, trans. JH] runs thus:

> This rule is called rule of three for the reason that in the problems that are made by this rule three numbers are always required, of which the first and the third should always be similar by counting one thing. And from these three numbers result another one, which

35 On p. 135 the author explains "the letters of algorism" to be the Hindu-Arabic numerals.

36 Esta el la 6 especia que comienca sy tantas valen tantas qué valdrán
tantas.

Sabe que segund manda la regla del alguarismo, para fazer qualquier cuenta que se pregunte por esta manera, en que pregunten asy, sy tanto fase tanto ¿qué sería tanto? Manda la regla del arte del alguarismo que multipliques lo 2« por lo 3« e partirás por lo primero e a lo que saliere a la parte, aquello es lo que preguntan, asy como quien dixiese, sy 3 fuesen 4 ¿qué sería 5?, para lo faser, pon las figuras de las letras como aquí dize, el 3 primero y el 4 y el 2« y el 5 el tercero, 3, 4, 5, y agora multiplica el 4 que es la segunda letra con el 5 que es la tercera e di 4 veses 5 son 20 e estos 20 pártelos por el 3 que están primero e verná a la parte 6 $^2/_3$, asy que sy te preguntaren que sy 3 fuesen 4 ¿qué sería 5?, dirás que 6 $^2/_3$, e por esta regla se farán todas las cuentas del mundo qualesquier que sean que se pregunte por esta manera.

> is the problem and conclusion of that which one wants to know. And it is always similar to the second number of the three. By some this rule is called the golden rule and by others the rule of proportions. The problems and questions of this rule are formed in this way: "If so much is worth so much, how much will so much be worth?". As for example, "if 6 are worth 18, what would 9 be worth?". For the making of such problems there is such a rule:
>
> Multiply that which you want to know by its contrary and then divide by its similar. Or multiply the third number by the second and then divide by the first.[37]

As we see, this version emphasizes the similar and the dissimilar, and combines the linear arrangement of the Castilian *Libro de arismé*tica with the formulation we know from Santcliment.

Closely connected to this *Traicté* is Barthélemy de Romans' *Compendy de la praticque des nombres.*[38] It says about the rule of three [ed. Spiesser 2003: 255–257, trans. JH] that it is "the most profitable of all", and gives two rules, one for finding Y from A, X and B, and one probably meant for finding B from A, X and Y,

> Multiply that which you want to know by its contrary, and then divide by its similar,[39]

and

> Multiply that which you know by that which is wholly dissimilar to it, and then divide by its similar,[40]

after which it goes on with the composites rules. The first of these rules, we see, is shared with the *Traicté* and with Santcliment; the second, by using the term dissimilar (*dissemblant*) instead of contrary, looks as if it was of Italian inspiration (it might thus simply be an alternative formulation of the rule for finding Y from A, X and B). The first example, however, is in purely Iberian tradition, "if 5 is worth 7, what is 13 worth?".

The final Ibero-Provençal treatise is Francés Pellos's *Compendion de l'abaco,* printed in Nice in 1492. It starts by a general introduction to the theme [ed. Lafont & Tournerie

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37 Ceste regle est appellee regle de troys pource que es raisons qui se font par ceste regle sont tousiours requis troys nombres desquelz le premier et le tiers doivent tousiours estre semblants en nombrant une chose. Et diceulx troys nombres en resulte ung autre qui est la raison et conclusion de ce que l'on veult savoir. Et est tousiours semblable au second nombre des troys. Ceste regle selon aucuns est appellee regle doree et selon autres regle des proportions. Les raisons et questions de ceste regle se forment en ceste maniere. Si tant vault tant que vauldra tant ? Comme par exemple : se 6 valent 18 que vauldront 9 ? Pour faire telles raisons il en est une telle regle.

Multiplie ce que veulz savoir par son contraire et puis partiz par son semblant. Ou multiplie le tiers nombre par le second et puis partiz par le premier.

38 Probably written around 1467 but only known from a revision made by Mathieu Préhoude in 1476.

39 Multiplie ce que veulz savoir par son contraire et puis partiz par son semblant.

40 Multiplie ce que sceiz par la chose qui luy est de tout dissemblant et puis partiz par son semblant.

1967: 101–103, trans. JH], that does not look in detail like anything else we have seen except in the concluding *General rule to find every thing,* and which in its entirety is likely to be Pellos's own description of the situation:

> In this chapter I want to give you a good mode and way in which you can always quickly and without great toil find all things that you want to buy or sell. And know that this chapter is called the chapter and rule of three things. In every computation of trade three numbers are indeed necessary.
> *The first number.*
> The first number is always the thing bought or sold, and you need to keep it well in memory.
> *The second number.*
> Know that the second number shall always be the value or the price of that which you have bought or sold.
> *The third example or number.*
> And the third number shall always be the thing that you want to know, that is to say, the thing that you want to by.
> *Remember that the first and the third numbers are always the same thing.*
> And know further that the first number and the third shall always be one thing. And if they are not certainly one thing, then you shall reduce them to a form where they speak of one thing, or matter, for in no way on earth they must not be different, as appears afterwards in the examples.
> *General rule to find every thing.*
> Always multiply the thing that you want to know by its contrary. And the outcome of this multiplication you divide by its similar, and that which comes out of such a division will be the value of the thing that you want to know.
> [section on reduction of units]
> *This is the way how you should say in matters that ask: if so much is worth so much, how much is so much worth? In this way, you may understand more clearly in the following examples.*[41]

[41] In aquest capitol yeu ti voli donar brava maniera he via per laqual tu l'augiarament en general podes prestament trobar sensa grand fatica totas causas che voles comprar aut revendre. Et sapias che aquest capitol s'apella lo capitol et regula de tres causas. Car en cascun rason de merchantias son necessaris tres numbres.

Lo prumier numbre.

Lo prumier numbre tostemps es causa comprada aut venduda, et es necessari che tengas ello ben en memoria.

Lo segont numbre.

Sapias che lo segont numbre deu tostemps esser lo valor aut lo pres de sso che aves comprat aut vendut.

Lo ters exemple ho numbre.

Et lo ters numbre tostemps deu esser la causa che demandas assaber so es a dire la causa che tu voles comprar.

Memoria per esser lo prumier et lo ters numbres tostemps
una causa.

Item mays sapias che lo prumier numbre et lo ters tostemps devon esser una causa. Et si non

The first examples that follow ask “if 4 are worth 9, what are 5 worth?”, “if 3 and a half is worth 6, how much are 4 worth?”, etc. After six similar examples follows a graphical scheme, deceptively similar to the one we know from Paolo dell’Abbacho and Fibonacci but actually used for reducing rule-of-three-type problems involving fractions into problems involving only integers (and too similar to many other schemes used in abbacus manuscripts to be supposed with any degree of certainty to be inspired by the traditional rule-of-three diagram).

Italy revisited

As we see, all Iberian and genuinely Provençal presentations of the rule use the counterfactual or abstract being-worth formulation; from the mid-15th century *Traicté* onward they also know the notions of similar/dissimilar – whether because of interaction with the Italian tradition or because of other inspirations cannot be decided.

Many Italian abbacus treatises, on the other hand, also know the counterfactual problem – and even “counterfactual *calculations*”, such as “If 9 is the 1/2 of 16, I ask you what part 12 will be of 25”, found in Gherardi’s *Libro di ragioni* [ed. Arrighi 1987b: 17, trans. JH]. But as was the case in the *Liber abbaci* (which by the way also contains counterfactual calculations), counterfactual problems and calculations are always found long after the presentation of the rule of three, or as illustrations of the rule following after many examples of the ordinary commercial kind.[42] They are clearly meant to be recreational, and do not belong to the basic didactical stock (for which reason they are invariably counterfactual, not of the abstract being-worth type).

There are *two* exceptions to this rule. The first of these is the apparently earliest Italian abbacus treatise, the “Columbia Algorism” [ed. Vogel 1977], the original of which is likely to have been written around 1285–90 (what we have is a 14th-century copy).[43] The rule of three is approached in two different ways.

eran certanament una causa, adunques deves ellos redure en forma che parlon de una causa, aut materia, car en nenguna maniera del munde non devon esser differens, coma apres appar en los exemples.

Regula general a trobar tota causa.

Multiplica tostemps la causa che demandas assaber per son contrari. Et la summa d’aquella multiplicacion partas per son semblant, et so che per tal partir ven, sera lo valor d’aquella causa che demandas assaber.

[…]

Aquesta es la mainera coma tu deves dire en causas que demandas : si tant val tant, quant val tant? Per aquesta mainera, tu podes intendre plus clar per aquest exemple sequent.

42 This is where Jacopo [ed. trans. Høyrup 2007: 238] asks the question “if 5 times 5 would make 26, say me how much would 7 times 7 make at this same rate”.

43 The dating of the treatise is discussed in [Høyrup 2007: 31 n. 20].

On one hand, there is a general presentation of the rule, not mentioning the name "rule of three" [ed. Vogel 1977: 39*f*, trans. JH]:

> Remember, that you cannot state any computation where you do not mention three things; and it is fitting that one of these things must be mentioned by name two times; remember also that the first of the things that is mentioned two times by name must be the divisor, and the other two things must be multiplied together.

This is followed by an example dealing with the exchange of money. Later this formulation is used a couple of times [ed. Vogel 1977: 48, 50] in examples which explicitly speak of the "rule of the three things". We recognize Pacioli's second formulation of the rule, which must thus have survived somewhere in the intervening two hundred years, even though I have not noticed it in texts I have looked at.

Mostly when the rule is used, however, a problem is reduced to a counterfactual [ed. Vogel 1977: 31*f*, 57, 61, 64*f*, 70 83, 80, 83, 86, 90, 109, 111, 123*f*] or an abstract being-worth [ed. Vogel 1977: 52, 112] formulation ("It is as if you said, 'if *a* were/is worth b, what ⋯' "). At times [ed. Vogel 1977: 52, 57, 64, 83], the rule is also mentioned by name in these connections.

Finally, a number of times the rule is called by name but without any reference to either the "mentioned", the counterfactual or the abstract being-worth formulations [ed. Vogel 1977: 52, 54*f*, 58, 110].[44] All in all, it is fairly obvious that the compiler of the treatise knows not only the Iberian "vernacular" way but also the idea underlying what was to become the Italian standard formula, expressing it however differently (the two "similar" things becoming that which is mentioned "two times").

The treatise stands at the very beginning of the Italian abbacus tradition, and it is thus not strange that it draws on discordant sources. In other respects too it has links to the Iberian tradition as we know it from the *Libro de arismética que es dicho alguarismo* – cf. for instance [Høyrup 2007: 85]. At the same time it makes use the Maghreb notation for ascending continued fractions (the *Libro ... dicho alguarismo* knows the fraction type but not the Maghreb notation).[45] However, there are few traces in later times of its

[44] The first folios of the treatise are missing, and so is the folio preceding the general introduction to the rule in "mentioned"-formulation. It is therefore impossible to exclude that a general introduction to the rule in one of the Ibero-Provençal formulations was present in the original. Nor must this necessarily have been the case, however.

[45] This notation had also been used by Fibonacci, but Fibonacci is not a likely source. Firstly, Fibonacci always writes these fractions from right to left, as do the Maghreb writers; the writing direction in the Columbia Algorism alternates. Secondly, the Columbia Algorism sometimes uses the notation when q is not a denominator but a metrological unit, a thing Fibonacci would never do (he knows that a denominator is a divisor, not a denomination). Thirdly, the Columbia Algorism has nothing else in common with the *Liber abbaci* (not to speak of Fibonacci's more sophisticated works).

idiosyncrasies: it was copied at some moment during the 14th century; in 1344, Dardi of Pisa shares its (mis)use of the notation for ascending continued fractions, writing $\frac{2\ 1}{\text{ç}\ 2}$ for "2 *censi* and ½ of a *censo*" [Høyrup 2010b: 23]; as we have seen, finally, the "mentioned" formulation of the rule of three turns up in two manuscripts from 1478 (Pacioli and Muscharello); but that is all I have observed.

Interestingly, Muscharello's treatise (written, we remember, outside the homeland of the abbacus tradition) is the other exception to the rule that the Italian treatises never use counterfactual problems to introduce the rule of three. Indeed, its first example [Chiarini et al 1972: 9] asks "if two were 3, what would five be?"[46] However, the exception is minimal – immediately afterwards, the treatise explains the meaning by an example of the usual kind, "it is as saying, if two palms of cloth are worth 3 *tari,* what will 5 palms be worth", and solves this problem, not the one it explains. It seems a good guess that the counterfactual introduction was inherited together with the "mentioned"-structure, if not from the Columbia Algorism then from a shared source – but then, as we see, almost discarded, in harmony with the integration of the "mentioned"-structure in the standard-formulation of the rule.

Apart from the Columbia Algorism, no Italian treatise I know of thus used the counterfactual or the abstract being-worth structures as the basic representation of or model for the rule of three.

What is the origin of the Ibero-Provençal representation?

As we have seen, Fibonacci refers to the counterfactual structure as the "vernacular" representation of the rule of three. Around 1200 it must hence have been widespread at least in some part of the Mediterranean environment he knew. Where?

We have no solids hints. For linguistic reasons, the counterfactual structure can hardly be Arabic: since the copula is not expressed in Semitic languages, "*a* is *b,* what is *c*?" should correspond to the opaque "*a b, c* what?". The abstract being-worth formulation, on the other hand, is obviously possible in Arabic, and we do have a couple of Arabic texts which come near to it. In the probably most faithful version of al-Khwārizmī's algebra – Gerard's translation [ed. Hughes 1986: 256] – the first example deals with a commercial problem, "10 *qafīz* are for six dragmas, what do you get for four dragmas?";[47] the second, however, is abstract "'ten are for eight, how much is the price for four?', or perhaps it is said, 'four of them are for which price'".[48] The words

46 Se doi fosse 3, che sserria cinquo?

47 Decem cafficii sunt pro sex dragmis; quot ergo provenient tibi pro 20 quattuor dragmis?

48 'Decem sunt pro octo; quantum est pretium quattuor?' Aut forsitan dicitur; 'Quattuor eorum quanti pretii sunt'.

"perhaps it is said" suggest that al-Khwārizmī quotes a common way of speaking – but we cannot be sure that this refers precisely to the abstract aspect of the formulation.

However, Robert's translation [ed. Hughes 1989: 65] as well as the extant Arabic version [ed. Rashed 2007: 198] present the first example already in abstract formulation. Both represent the text as it developed in use, and the change could thus reflect common parlance. On the other hand, the step from concrete to abstract formulation is easily made, and the very scattered occurrences of similar wordings in Arabic texts (and their absence from the *Liber mahamelet* and the "Toledan *regule*") do not suggest that they represent a widespread vernacular.[49] The safest assumption is that the abstract being-worth shape and its counterfactual variant were widespread at least somewhere in the Iberian world toward 1200;[50] before that we do not know.

So, whereas the first part of my story had few loose ends, these abound in the second part. Nobody should be surprised: loose ends are always there, and we can only create a simple coherent story by disregarding them – which does not necessarily make the coherent story untrue, only incomplete.

(And then I have not even touched at the presence of the
rule of three in China, another intriguing
but unapproachable loose end)

[49] Ibn al-Khiḍr al-Qurašī (Damascus, mid-11th-century) explains [ed., trans. Rebstock 2001: 64, English JH] the foundation for "sale and purchase" to be the seventh book of Euclid, after which he states (emphasis added) that "this corresponds to *your* formulation, 'So much, which is known, for so much, which is known; how much is the price for so much, which is also known?'". The reference to "your formulation" suggests that a general way of speaking about the matter is referred to. But was this general way already abstract, or has the abstraction been superimposed by al-Qurašī?

In [Høyrup 2010a: 13], I took an isolated quotation from A. S. Saidan's translation of al-Baghdādī as a hint of Persian counterfactual usage. Since then Mahdi Abdeljaouad has got access to the book and translated the whole surrounding passage (for which my sincere thanks!). In context, the use of the Persian expressions *dah yazidah,* "ten (is) eleven", and *dah diyazidah,* "ten (is) twelve" in calculations of profit and loss turn out to have no such implications. Cognitively, what goes on is rather an analogue of the medieval Italian (and modern) notion of *per cento,* "percent".

[50] It should be remembered that the 13th-century Vatican manuscript Palatino 1343 of the *Liber abbaci* refers (fol. 47^{r-v}, new foliation) to use of a work by a "Castilian master". Boncompagni knew [1851: 32], but since this early manuscript is incomplete, he used a later one (from where this observation is absent) for his edition.

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Chapter 7 (Article I.6) Geometrical Patterns in the Pre-classical Greek Area: Prospecting the Borderland between Decoration, Art, and Structural Inquiry

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

Many general histories of mathematics mention prehistoric "geometric" decorations along with counting and tally-sticks as the earliest beginnings of mathematics, insinuating thus (without making it too explicit) that a direct line of development links such decorations to mathematical geometry. The article confronts this persuasion with a particular historical case: the changing character of geometrical decorations in the future Greek area from the Middle Neolithic through the first millennium BCE.

The development during the "Old European" period (sixth through third millennium B.C., calibrated radiocarbon dates) goes from unsystematic and undiversified beginnings toward great phantasy and variation, and occasional suggestions of combined symmetries, but until the end largely restricted to the visually prominent, and not submitted to formal constraints; the type may be termed "geometrical impressionism"

Since the late sixth millennium, spirals and meanders had been important. In the Cycladic and Minoan orbit these elements develop into seaweed and other soft, living forms. The patterns are vitalized and symmetries dissolve. One might speak of a change from decoration into *art* which, at the same time, is a step *away* from mathematical geometry.

Mycenaean Greece borrows much of the ceramic style of the Minoans; other types of decoration, in contrast, display strong interest precisely in the formal properties of patterns – enough, perhaps, to allow us to speak about an authentically mathematical interest in geometry. In the longer run, this has a certain impact on the style of vase decoration, which becomes more rigid and starts containing non-figurative elements, without becoming really formal. At the breakdown of the Mycenaean state system around 1200 B.C., the "mathematical" formalization disappears, and leaves no trace in the decorations of the subsequent Geometric period. These are, instead, further developments of the non-figurative elements and the repetitive style of late Mycenaean vase decorations. Instead of carrying over mathematical exploration from the early Mycenaean to the Classical age, they represent a gradual sliding-back into the visual geometry of earlier ages.

The development of geometrical decoration in the Greek space from the Neolithic through the Iron Age is thus clearly structured when looked at with regard to geometric conceptualizations and ideals. But it is not linear, and no necessity leads from geometrical decoration toward geometrical exploration of formal structures (whether intuitive or provided with proofs). Classical Greek geometry, in particular, appears to have its roots much less directly (if at all) in early geometrical ornamentation than intimated by the general histories.

Contents

Dem lieben Freund
FRITZ JÜRß
zum baldigen siebzigsten
Geburtstag gewidmet

Preliminary remarks

How did mathematics begin? And why did the ancient Greeks develop their particular and unprecedented approach to geometry? Such questions are probably too unspecific to allow any meaningful (not to speak of a simple) answer; even if meaningful answers could be formulated, moreover, sources are hardly available that would allow us to ascertain their validity.

In the likeness of the grand problems of philosophy (Mind-Body, Free Will, an so forth), however, such unanswerable questions may still engage us in reflections that illuminate the framework within which they belong, thereby serving to develop conceptual tools that allow us to derive less unanswerable kindred questions. The pages that follow are meant to do this.

It does so by analyzing a collection of photographs which I made in the National Archaeological Museum and the Oberländer Museum in Athens in 1983, 1992 and 1996, representing geometrical decorations on various artefacts, mostly ceramics; those of them which are essential for the argument are reproduced below.[1] All the artefacts in question were found within, and thus connected to cultures flourishing within, the confines of present-day Greece (Crete excepted). The earliest were produced in the sixth millennium BCE (calibrated radiocarbon date); the youngest belong to the classical age.

General histories of mathematics often identify geometrical patterns along with counting and tally-sticks as the earliest beginnings of the field.[2] Mathematicians (and in this respect historians of mathematics belong to the same tribe) tend to assume that what *we* describe in terms of abstract pattern and shape was also somehow meant by its producers to deal with pattern and shape *per se*, or at least to be automatically conducive

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1 All items are already published and on public display. The photos used here are all mine. ⟦In 1983 I was told by the archaeologists at the National Museum that I might therefore use them freely.⟧

2 In a sample of eleven works which I looked at, six began in that way: [Smith 1923], [Struik 1948], [Hofmann 1953], [Vogel 1958], [Boyer 1968] and [Wußing 1979]. [Cantor 1907], [Ball 1908] and [Dahan-Dalmedico & Peiffer 1982] take their beginnings with the scribes of the Bronze Age civilization. So does [Kline 1972] on the whole, even though he does discuss pre-scribal mathematics on half a page, and mentions “geometric decoration of pottery, [and] patterns woven into cloth” in these eight words. Chapter 1 on “Numeral Systems” of [Eves 1969] contains half a page of speculations on “primitive counting”.

to interest in these; this is never stated explicitly, but it is an implied tacit presupposition. At least for members of our mathematical tribe it seems a reasonable presupposition.

When first running into the objects rendered in my photographs, I was indeed struck by the easily distinguishable trends in the changing relation of these patterns to geometrical inquiry and thought (what I mean by this beyond “interest in pattern and shape *per se*” will be made more explicit in the following); I also noticed, however, that development over time could as easily lead away from mathematical geometry as closer to it. Mathematics is no necessary, not even an obvious consequence of the interest in visual regularity (which, on its part, appears to *be* rather universal). Not every culture aims at the same type of regularity, and the interest in precisely *mathematical* regularity is a choice, one possible choice among several.

On the other hand, the universal human interest in regularity – that “sense of order” of which Gombrich [1984] speaks – *may* certainly lead to systematic probing of formal properties of symmetry, similarity, etc. Whether such inquiry is connected to some kind of proof or argument or not (which mostly we cannot know), there is no reason to deny it the label of “mathematics” (or, if we prefer this distinction and that use of the term, “ethnomathematics”, as an element of mathematical thought integrated in an oral or pre-state culture). In order to distinguish these cases from such uses of pattern and shapes whose intention and perspective we are unlikely to grasp through a characterization as “mathematics”, we need to develop concepts that reach further than the conventional wisdom (or, with Francis Bacon, “idols”) of our tribe.

My purpose is thus primarily a clarification of concepts which may permit us to look deeper into the relation between decorative patterns and mathematics; it is neither the history of artistic styles nor the links between cultures. For this reason I do little to point out the evident connections between, for example, the decorations found on Greek soil and the styles of the Vinča and other related Balkan cultures.

The gauge is deliberately anachronistic, and I make no attempt to interpret the artefacts which I discuss in their own practical or cultural context (although I do refer occasionally to their belonging within a specific framework – deliberate anachronism should never be blind to *being* anachronism). My purpose is, indeed, not to understand this context but to obtain a better understanding of the implications of that other blatant anachronism which consists in reading early decorations in the future-perfect of mathematics – an anachronism which can only (and should only, if at all) be defended as a way to understand better the nature *of mathematics* and the conditions for its emergence.

Though this was not on my mind when I took up the investigation, my approach can be described as a *hermeneutics of non-verbal expression* – “hermeneutics” being so far taken in Hans Georg Gadamer’s sense that the expression of “the other” is *a priori* assumed, if not to be “true” (obviously, expressions that do not consist of statements possess no truth value) then at least to be “true to an intention”. Whereas the habitual ascription of a “mathematical intention” to every pattern and symmetry can be compared

with that reading of a foreign text which locates it straightaway within the "horizon" of the reader, my intention here may be likened to Gadamer's *Horizontverschmelzung*, "amalgamation of horizons". In agreement with Gadamer's notion of the hermeneutic circle I presuppose that such an amalgamation is possible, that our present horizon can be widened so as to encompass that of the past "dialogue partners" (yet without sharing Gadamer's teleological conviction that this amalgamated horizon can also be said to be the *true* implied horizon of the partners; the wider horizon remains *ours*, and remains anachronistic).[3] As we shall see, this requires that our wider horizon transcends that of the mathematical tribe.

As affirmed emphatically by Gadamer, hermeneutics is no method, no prescription of the steps that should be taken in the interpretion of a foreign text. This is certainly no less true for a "hermeneutics of non-verbal expression". For this reason, the conceptual tools that emerge during the investigation – in particular a notion of "geometrical impressionism", and a particular (tentative) distinction between "art" and "decoration" – cannot be adequately explained in abstraction from the material and developments they serve to elucidate, however much they may afterwards reach beyond this particular material and these particular developments.

One key concept, however, must be confronted before we can begin the discussion of whether any development points toward mathematical geometry or not: that of "mathematics". Chronologically, mathematics may be said to begin at any point in time at least since the moment when the first mammals integrated sense impressions as representations of a permanent object, thereby bringing forth that *unity* which, according to ancient and medieval metamathematics, is the "root of number". Evidently, no meaningful precise cut in this continuum can be established; but I shall use as a heuristic delimitation the principle that mathematics presupposes coordination or exploration of formal relations, based on at least intuitively grasped understanding of these. Since my concern is whether developments lead "toward mathematics" or away from it rather than the decision whether a particular pattern *is* mathematics, the inescapable imprecision of the delimitation will be no severe trouble.

As far as the other aspect of the investigation is concerned – the roots of the particular Greek approach to geometry – no conceptual innovation is needed. The results – first of all that nothing in the "geometric" style of the ninth through seventh century BCE points toward the emergence of "rational geometry" – will emerge through the analysis.

[3] See [Gadamer 1972: 289*f* and *passim*]. The stance that the amalgamated horizon is the true implied horizon of the partners corresponds to that kind of historiography of mathematics according to which contemporary mathematicians, those who have insight into the tradition as it has now unfolded, are the only ones that are able to understand the ancient mathematicians and thus those who should write the history of mathematics.

Since the purpose of the investigation is the sharpening of conceptual tools (and, to a lesser extent, analysis of the historical process *within* the Greek area), I shall permit myself to date the items I discuss as done in recent years by the museums and in the catalogue of the Archaeological Museum [Petrakos 1981],[4] relative chronology being all I need. As far as the second millennium BCE and the late third millennium are concerned, the dates seem to be derived from Egyptian and Near Eastern historical chronology, and thus to be *grosso modo* correct. Earlier dates (presented in [Petrakos 1981: 11] as "generally accepted conclusions") appear to be uncalibrated radiocarbon dates, since they coincide with what other publications (e.g. [Gimbutas 1974]) give as uncalibrated dates for the same sites and periods; when asked, Dr. M. Vlassopoulou of the Museum confirmed my hunch.[5] Approximate translation into true historical date (as determined by dendrochronology) can be made by means of this table, based on [Watkins (ed.) 1975: 118-124] and [Ferguson et al 1976]:

Uncalibr. radiocarbon date	2000	2500	3000	3500	4000	4500	5000	5500
Approx. historical date	2500	3240	3720	4410	4880	5400	5900	6450

All dates are BCE; the translations of radiocarbon date into historical date are with a margin of±50 to 100 years, to which comes the imprecision of the radiocarbon dating itself.

Even in the naming of periods I follow the Museum Catalogue. This implies that what is here spoken of as the "Late Neolithic" will be spoken of as the "Chalcolithic" in the majority of recent publications.

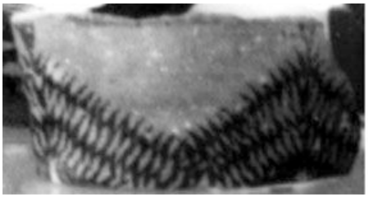
In all figure captions, ArchM stands for the National Archaeological Museum in Athens. ObM stands for the Oberländer Museum, Kerameikos, Athens. All dates are evidently BCE.

[4] In 1983, the displayed dates for the older period in the Archaeological Museum (not yet coordinated with the catalogue) were even younger than now.

[5] Dr. Vlassopoulou also procured me with the date and origin of artefacts which were displayed in the Museum without any such indications and by correcting dates that had been wrongly indicated in the exhibition. I use the opportunity to express to her my sincere gratitude.

"Old Europe"

The sequence #1 to #24 represents – at the level of generalization on which I move here – a fairly uniform development that passes through several stages but is never radically interrupted. Chronologically it spans the period from the early fifth through the late third millennium (uncalibrated radiocarbon dates). Since the third millennium items all belong to the Cycladic area, where the influence from the "Kurgan" intrusion and interruption of the more northerly branches of the Balkan culture was only weakly felt, the whole sequence must be connected to the culture of "Old Europe" and its Cycladic offspring.[6] Restriction to the Greek area has the added advantage that we avoid whatever particular effects may have been caused by the rise of large, more or less town-like settlements in the Vinča culture – cf. [Gimbutas 1974: 22].



Photograph # 1 (left). ArchM, Museum N° 5918. Middle Neolithic, "Sesklo style", 5th millennium.
Photograph # 2 (right). ArchM, Museum N° 5919. Middle Neolithic, "Sesklo style", 5th millennium.

Several sub-periods can be distinguished. Photographs #1–3 are representative of the Middle Neolithic Sesklo period. All items reflect interest in bands of acute angles, triangular organization and concentric rhombs (the latter in #2 and in other items not shown here). Only straight lines are made use of, and no attempt is made at integrating the order that characterizes the single levels into a total system, nor to correlate the pattern with the geometry of the object which is decorated – if we analyze #1 we first see a macro-level where three zigzag-lines run in parallel. The lower of these, however, goes beyond the inferior edge of the vase. Each segment of the zigzag line is in itself a zigzag-line, but made according to principles which differ from those of the macro-level; we may characterize it as a band of spines. These segments, furthermore, meet in a way which lets their spines cross each other. Each segment is clearly thought of in isolation.

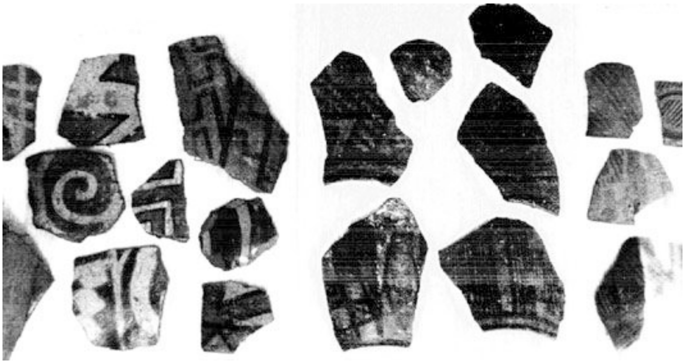
[6] See [Gimbutas 1973a, 1973b]. The more disputed aspects of Marija Gimbutas' description of the cultural sequence are immaterial in the present connection – thus whether her "Kurgan" pastoralists are identical with the Proto-Indo-Europeans (cf. [Mallory 1989: 233-243 and *passim*]).

Photograph # 3. ArchM, Museum N° 51918. Middle Neolithic, "Sesklo style", 5th millennium.

Photograph # 4 (left). ArchM, Museum N° 8051. Lianokladi, Middle Neolithic, "scraped ware", 5th millennium.
Photograph # 5 (right). ArchM, Museum N° 8066. Lianokladi, Middle Neolithic, "scraped ware", 5th millennium.

The beautiful, more or less contemporary "scraped ware" from Lianokladi is even less formal in its use of "geometric" decoration (see #4–5); both on the level of global organization of the surface and regarding the internal organization of each segment, irregularity is deliberately pursued. The fragments from the pre-Dimini-phase (#6) of the Late Neolithic ("Chalcolithic" would be better, copper being in widespread use in the Old European culture during this period) exhibit some more variation than the Sesklo specimens (spirals turn up), but convey the same overall impression.

The decorations belonging to the Dimini phase of the Late Neolithic (#7–13) are somewhat different. Now larger parts of the surfaces are covered by geometrically coherent decoration, but in most cases still only *parts* of even larger surfaces. In #7, a chessboard pattern is partly covered in two places by a series of parallel lines – lines which,

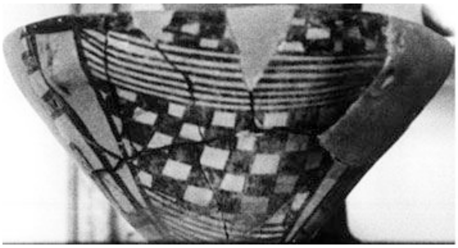


Photograph # 6. ArchM, various museum numbers. Fragments, Late Neolithic, pre-Diminian phase, 4300–3800.

furthermore, run in a direction which deviates slightly but unmistakably and deliberately from the closest axis of the chessboard. The outer edge of the chessboard is also wholly incongruous with both the sides and the diagonal of the pattern itself, even though inclusion of part of the blank space to the left would have permitted agreement with the diagonal. In #8 a spiral-system is clipped in a way which demonstrates that it is imagined as cut out from a larger spiral. No attempt is made to unite the spiral with the geometrical conditions offered by the vase – as in an amateur photo, the motif is one thing and the frame is another. Only #9 suggests that the conditions arising from the surface to be decorated and the geometry of the decorating motif are thought of as *one* problem.

The decorations of the Middle Neolithic were constructed from straight lines, we remember. The use of curved lines, especially in the form of spirals, is thus an innovation. The combination of two spirals in #9 – each of roughly the same type as in #8 – presents us with another kind of innovation: it can be understood as a geometrical restructuration of less complex material. It allows coherent decoration of the irregular surface, but only at the cost of eclecticism. The core of the lower spiral has been turned 90° with respect to its counterpart, allowing it thus to be flattened and broadened. Furthermore, regions where too much space is left uncovered by the meander are filled out by triangles. The purpose of the pattern is decorative, rather than geometrical exploration.[7]

7 Evidently, this dichotomy does not exhaust what can be said about a geometrical pattern. For instance, patterns may possess symbolic functions, but even if we follow Marija Gimbutas and interpret the meanders as snake symbolism, we have to observe that meanders meant as formalized snakes may be used in a way that suggests geometrical exploration, or they may be located eclectically, as suggested by decorative intuition.



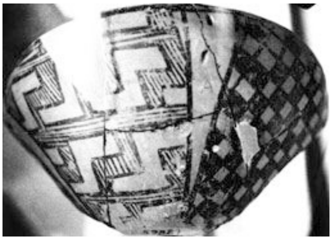

Photograph # 7 (left). ArchM, Museum N° 5925. Late Neolithic, "Dimini style", 4th millennium.
Photograph # 8 (right). ArchM, Museum N° 5932. Late Neolithic,

Other items confirm a tendency toward greater variation in comparison with those of the Middle Neolithic, but the patterns always remain decorative and eclectic, and nothing suggests that geometrical regularity is pursued for its own sake. In #10, the spaces left open by the bands of parallel lines are filled out by figures of highly heterogeneous character (spiral, circular segments, zigzag-lines in pointed pseudo-ellipses). In the bands of parallel lines, the number of lines varies from one band to another, and the suggested


Photograph # 9 (left). ArchM, Museum N° 5934. Late Neolithic, 4th millennium.
Photograph # 10 (right). ArchM, Museum N° 592. Late Neolithic, Dimini, 4th millennium.

mirror symmetry between the left and right pseudo-ellipses is contradicted by the translational symmetry of the zigzag lines which they enclose (which, given the general eclecticism of the composition, is not likely to represent a deliberate experiment with symmetry

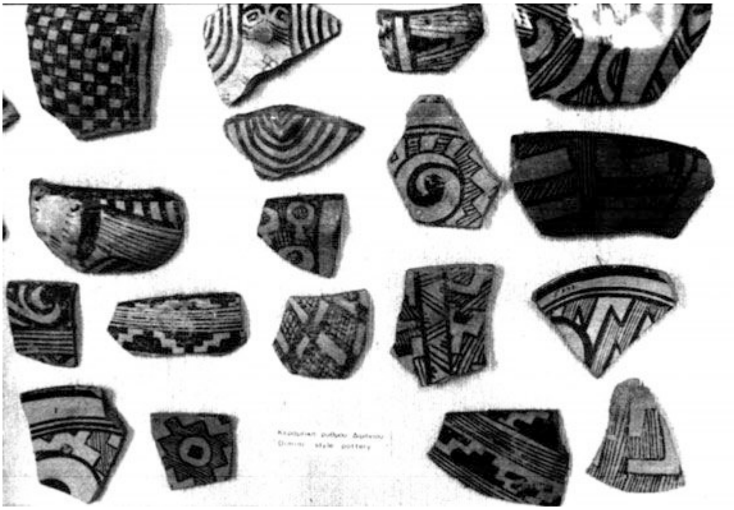
breaking). Geometrical regularity at the visual level furnishes the material, but the governing principles and the overriding concerns are different: leaving aside their further meaning for the artist we may say that aesthetic sensibility is more important than regularity even at the visual level. Moreover, symmetry appears to be disregarded when inconvenient, not first deliberately suggested and then consciously broken as (for example) in certain Persian carpets, which would still be a kind of sophisticated geometrical exploration.



Photograph # 11. ArchM, Museum N^o 5931. Late Neolithic, "Dimini style", 4th millennium.

Photograph # 12. ArchM, Museum N^o 5937. Late Neolithic, Sesklo, 4th millennium.

In #11 we find the same eclecticism as in #7 (it is only one of several specimens in the Museum that follow the same fundamental model): on the back, another chessboard pattern is partially covered by a band of parallel lines, even this time slightly slanted (see the photo in [Matz 1962: 28]); between the two chessboards, both #7 and #11 carry a two-dimensional in-law of the rectangular meander (with similar hatchings in both specimens). The geometrical eclecticism of the decoration is thus hardly a random phenomenon, it must be assumed to be governed by deliberate considerations that are external to the pattern itself – perhaps a symbolic interpretation given to the constituents and to their mutual relation.



Photograph # 13. ArchM, various museum numbers.
Late Neolithic, "Dimini style", 4th millennium.

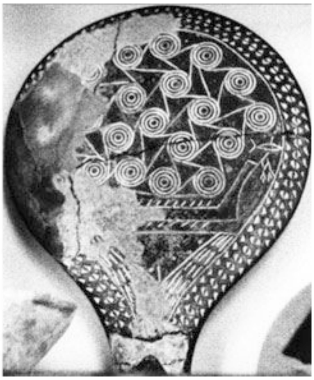
All the more striking is the geometrical carelessness demonstrated in the upper left corner of the chessboard of #11, where fields that should have been black have become white, and vice versa. Similar seeming carelessness is found in other cases (cf. the fragments in #13), and hence better understood as emphasis on visual impression and absence of formal mathematical constraint.

Comparing the Late Neolithic decorations with those of the Middle Neolithic we may conclude that greater geometrical phantasy and sensitivity makes itself felt. None the less, the visual effect remains the overriding concern, and the over-all impression which results from application of a geometrico-mathematical gauge is one of unworried eclecticism. Formal constraints – be they based on counting or on rotational, translational or mirror symmetry – are relatively unimportant as soon as they go beyond what is visually obvious

for the geometrically innocent mind. At the level of the visually obvious, on the other hand, they *are* important: the chessboard pattern is *almost* there even in #11, and the pattern in the left part of the same photo exhibits vertical and horizontal translational symmetry as well as symmetry against a rotation of 180° (disregarding rather strong metric distortions).

Decorative painting remains abstract, no figurative elements are involved even though figurative sculpture is well represented in the record – #12 shows a Late Neolithic piece from Sesklo which is itself covered by an abstract pattern (supposed by Gimbutas [1974: 144] to be possibly a snake symbolism, but even then a thorough abstraction[8]).

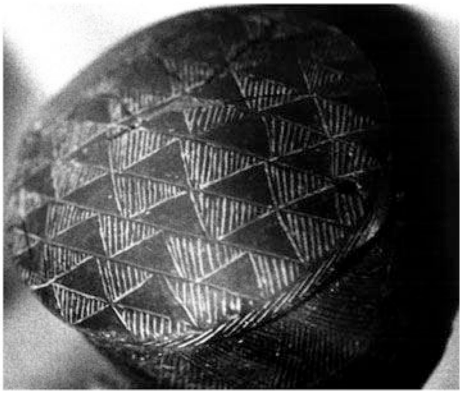
Photograph # 14. ArchM, Museum N^o 5698. Melos, Phylakopi I, 2300–2000.



Photograph # 15 (left). ArchM, Museum N^o 6140A. Naxos, Early Cycladic II, 2800–2300.
Photograph # 16 (right). ArchM, Museum N^o 5053. Syros, Early Cycladic II, 2800–2300.

[8] More thorough indeed than the abstraction of certain Cucuteni zigzag-lines provided with a snake's head [Dumitrescu 1968: pl. 42, 48].

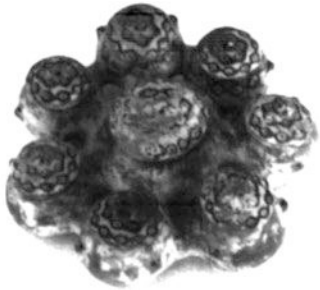
Photograph # 17. ArchM, Museum N° 5358. Naxos, Early Cycladic II, 2800–2300.



Photograph # 18 (left). ArchM, Museum N° 6180. Syros, Early Cycladic II, 2800–2300.
Photograph # 19 (right). ArchM, Museum N° 5358. Naxos, Early Cycladic II, 2800–2300.

In the photos from the third-millennium Cycladic Early Bronze Age culture (#14–24) certain deviations from this pattern become visible, but no fundamental changes can be traced. Figurative sculture is still found, at times decorated with abstract patterns (thus #14). Decorations themselves may now involve figurative elements. In #15, four fish and a sun enter an otherwise geometric composition, participating in its highly symmetric design; in #16 (and in many similar "frying pans"), a picture of a boat is surrounded by a geometric pattern,[9] while the whole scene stands on a female pubic triangle (drawn exactly as on female figurines). Interwoven spirals become a major theme in the decora-

[9] That this pattern is thus likely to represent stylized water is immaterial for the actual discussion, since it is anyhow geometrical.



Photograph # 20 (left). ArchM, Museum N° 5171.
Taphos, Early Cycladic II, 2800–2300.
Photograph # 21 (right). ArchM, Museum N° 6185.
Early Cycladic II, 2800–2300.

tions,[10] as can be seen in #17 (where the pattern is fully systematic) and in #15 (where close inspection of the lines reveals a lack of regularity). The importance of this complex pattern is perhaps most clearly seen in #16 and #18 (shown as representatives of a large class of similarly decorated pieces), both of which present us not with spirals but with an easy counterfeit: systems of concentric circles (so uniform that they are likely to have been made by means of a multiple compass) connected by straight or slightly curved lines. Only one item, however, can be said with some justification to explore the possibilities of a geometrical pattern formally, *viz* #17, an engraved steatite box. In #15, on the contrary, the real symmetry is less than the one suggested by immediate inspection (see the centres of the spirals).

Other items exhibit in stronger form this contrast between seeming regularity at the level of immediate visual impression and random irregularity below this level – henceforth I shall speak of "geometrical impressionism". In #19 the apices of the black triangles of one band are sometimes adjusted to the band above, sometimes they move without system with respect to the bases of the triangles in this band; the number of strokes in the hatchings varies – in some triangles they run parallel to the left edge (at least ideally speaking), in others they cut it obliquely, in still others they are vertical. In the similar though cruder pattern of #20, the hatching is mostly parallel to the right edge but

[10] Once more, we need not pursue the possible inspiration from cultures with which the Cycladeans may have been in contact. Interconnected spirals were also popular in the Megalithic Culture(s) to the West, from Malta to Ireland. The critical question is on which level of geometry the motif was used within the Cycladic culture. The Megalithic monuments themselves vary in this respect, from strict organization – a specimen from Tarxien, Malta, is reproduced in [Guilaine 1981: 970] – to arrangements even more loose than #16 – a specimen from Newgrange, Ireland, is in [Mohen 1984: 1536].

occasionally (and without system) to the left edge. In #16, the single systems of concentric circles have been located as best they could, in order to fill out the space left open between the border and the boat; in the interior part of the pattern, moreover, most of the systems are connected to six but some to five or seven other systems: no idea is obviously present that systems of circles "should" be arranged with hexagonal symmetry – cf. also #21, where a central "circle" is surrounded by seven, not six other "circles".

Similarly, the suggested star of the "frying-pan" of #22 is, at most, *a suggestion* of stellar symmetry. In #23, the number of hatching lines in the cross-hatched triangles is sometimes 8 and sometimes 9. Even #24, apparently fully symmetric (apart from some metric distortion) turns out not to be so when we start counting the dots.

Photograph # 22 (left). ArchM, Museum N° 5153. Syros, Early Cycladic II, 2800–2300.
Photograph # 23 (right). ArchM, Museum N° 8874. Raphina, Early Helladic, 3d millennium.

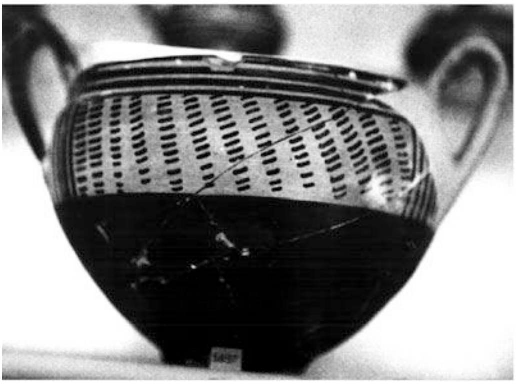
Seen as a whole, the development of geometric patterns in the Old European period is thus one from unsystematic and rather undiversified beginnings in Middle Neolithic Sesklo toward great phantasy and variation and even sophisticated combined symmetries in the third millennium, but throughout the period largely restricted to the visually obvious, and – with at most a single exception in the material shown above (*viz* #17) – never formally carried through: geometric structure is and remains subservient to other purposes, where we are unable to extricate aesthetics or decoration from symbolization.[11]

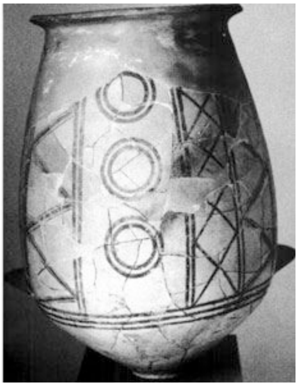
During the whole span of the Neolithic and the third millennium, decorative *painting* remains almost exclusively abstract and non-figurative, even though figurative *sculpture*

[11] Evidently, questions presupposing that these occur in additive and thus separable combination are probably misguided – who would ever claim that a *Pietà* carries less religious feeling or meaning because the painting has a strong aesthetic impact?

Photograph # 24. ArchM, Museum N^o 5225. Early Cycladic II, 2800–2300.

is known from all periods. Only the "frying pans" of the Cycladic third millennium contain figurative elements, at times (#15) integrated in the geometric symmetry of the design, at times (#16) an independent condition to which the geometric structure has to submit.



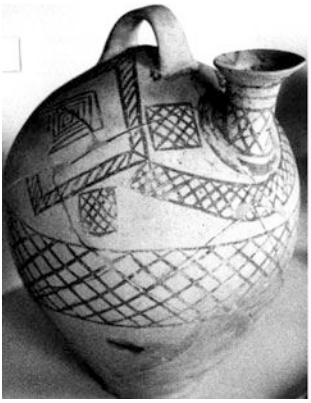


Photograph # 25 (left). ArchM, Museum N^o 5857. Orchomenos, Middle Helladic, 2000–1500.
Photograph # 26 (right). ArchM, Museum N^o 5876. Orchomenos, Middle Helladic, 2000–1500.

In Minoan orbit

The Middle Cycladic culture of the first half of the second millennium was a continuation of earlier Cycladic cultures [Christopoulos & Bastias (eds) 1974: 140], and distinct from the culture of Minoan Crete. From the point of view of the present investigation, however, the *transformation* of earlier practices as well as the Minoan affinities soon become evident.

The period is presented by photos #25–36. #25–26 are from pre-Mycenaean Orchomenos in Boeotia; they are included in contrast to the following and as supplementary examples of impressionistic geometry. In #25, the number of strokes in each band is almost constant – but not quite; moreover, the two bands of 11 strokes (against the normal 12) could easily have contained 12 strokes – the bands are apparently made from above, and in the lower end the distance between the strokes is augmenting in these two bands. In #26, nothing is done in order to harmonize the apices of the two sets of adjacent zigzag lines.



Photograph # 27 (left). ArchM, Museum N° 5286. Melos, Phylakopi, between I and II. C. 2000.
Photograph # 28 (right). ArchM, Museum N° 5759. Melos, Phylakopi II, early 2nd millennium.

#27 – the first Cycladic specimen – differs from the Orchomenos samples by a free experimentation with cross-hatching of squares and ribbons that is unconstrained by attempts to achieve symmetry even at the immediate visual level (the hidden side contains the same elements as the one that is shown, but in a very different arrangement); the global organization is as eclectic as it is pleasant to a contemporary eye.

The first striking characteristic of the following Cycladic decorations is the presence

Photograph # 29 (left). ArchM, Museum N° 5740. Melos, Phylakopi II, early 2nd millennium.
Photograph # 30 (right). ArchM, Museum N° 5804. Melos, Phylakopi II, early 2nd millennium.

of figurative painting. This seems alien to the “native tradition” of the area as we know it from the preceding section, and could be interpreted as an indication of cultural diffusion. Diffusion may, indeed, be part of the “efficient cause” of the change which took place.[12] But diffusion is, as always, a rather empty word which hides at least as much as it explains. In the present case it tends to veil the problem why figurativeness was learned from the Egyptians (if we suppose Egypt to be the source – other sources would raise similar problems) while the “canonical system” of Egyptian figurative art[13] was certainly not. The diffusionist explanation, furthermore, leaves aside the question how the diffused cultural element became part of an integrated cultural system: which specific character

[12] I leave aside the question why Paleolithic cultures tend to have figurative drawing and painting, whereas Neolithic ceramics is almost always abstract and “geometric”, and “civilizations” reintroduce the figurative element. Since Paleolithic and Mesolithic cultures as a rule *have* no ceramics but may use abstract decorations on other surfaces (examples in [Otto 1976: 45–49]), and since Neolithic societies may produce figurative sculpture, the real issue is more complex than the oft-repeated three-step scheme might make us believe. That part of the answer which goes beyond the fate of materials (ceramics survives, wooden tools rarely, tattooings almost never) may have to do with the social division of labour.

In any case, the development of civilization in the Cycladic area is in itself correlated with cultural contact and learning. Diffusion of artistic styles thus cannot be separated from the effects of the civilizing social process.

[13] See [Iversen 1975]. Central elements of the canonical system are the use of the square grid and the observance of strict proportions between the single parts of the human (or animal) body – elements which together contribute strongly to the formal character of Egyptian art and which sets it decisively apart from anything Cycladic and Minoan.

was “figurativeness” to acquire in the Cycladic context?[14]

Photograph # 31 (left). ArchM, Museum N° 5758. Melos, Phylakopi II, early 2nd millennium.
Photograph # 32 (right). ArchM, Thera collection N° 58. Thera, 1550–1500, Minoan import.

Photograph # 33 (left). ArchM, Museum N° 5757. Melos, Phylakopi III, 1600–1400. Local pottery with Minoan influence.
Photograph # 34 (right). ArchM, Museum N° 5803. Melos, Phylakopi III, 1600–1400. Probably Minoan import.

A characteristic example of this specific character is found in #28. Spirals are essential, as in so many decorations since the fourth millennium – but the spirals are undergoing a process of dissolution, they have become aquatic plants growing out of the sea floor.

[14] These points are commonplace objections to diffusionism. They are repeated because they arise specifically in the present context.

Figurative painting does not come in as a complement or substitute (as it was to do in the Late Geometric period); *instead, the character of the traditional decorative pattern* (already giving up the quest for symmetry even at the visual level in #27) *is transformed and becomes figurative itself.*

The same feature can be observed in #29. The spirals are more geometrical, but they are growing out of a common floor, and they are deliberately differentiated (we notice that they are seven in number, cf. #21). The edging that surrounds them, moreover, is no repeated abstract shape but a band of not too similar leaves.

Photograph # 35 (left). ArchM, Museum N° 5789. Melos, Phylakopi III, 1600–1400. Probably Minoan import.
Photograph # 36 (right). ArchM, Museum N° 1838. Thera, 16th c.

Other items are not as easily identified as missing links between geometry and vegetation. The aquatic plants of #30 are not derived from a geometric figure. Yet if we compare #30 with for instance #18 and #20 we shall still encounter evidence for a transformation of the geometrical principles. The latter two are repetitive, in principle they exhibit rotational symmetry. The decoration of #30 is also constructed from a repetitive basis, but now the symmetry is intentionally broken – not, as in the preceding period, in a way which can be characterized as a secondary deviation from a suggested principle, but in a way that cannot avoid being noticed and which must have been meant to be part of the immediate impression.

The upper part of the decoration is non-figurative, consisting of connected systems of concentric circles. Whereas the concentric circles of #16 and #18 were drawn with a multiple compass (which is to return in later periods), those of #30 are not drawn with precision; nor are they, for that matter, always complete circles (cf. also #31–32). They could be described as *living* patterns.

A similar conclusion can be drawn if we compare #33 with #27. In both cases cross-hatched ribbons are seen; but whereas the spaces which are left open between the ribbons

are filled out with squares in the early Middle Cycladic piece, plants are used to hold the *horror vacui* aloof in #33. We may also observe that one of the ribbons of #33 is twined, while those of #27 are not.

For comparison with later developments, finally, the paintings of #34 (the freely swimming octopus) and #35 (stylized ivy etc.) should be taken note of.

Most of the rare figurative motifs of the earlier period were artefacts (boats) or, if living beings, made as stiff as artefacts (the fish of #15) (the female pubic triangle of #16 and other "frying pans" seems to be symbolic rather than really figurative). Most Middle Cycladic and Minoan figurative motifs, on the other hand, are plants (and among these, softly waving aquatic and twining plants dominate); animals do occur, but the octopus of #34 as well as the duck of #36 are drawn with soft, almost vegetative lines. Human artefacts with their sharply cut contours are avoided in ceramic decorations (though not in the Thera *frescoes*).

In the perspective of the present study, the basic characteristic of the decorations of this period is thus the transformation of geometrical patterns and motifs: the patterns are vitalized, they are re-conceptualized as living creatures or quasi-living, moving lines.[15] Symmetry is upheld as an underlying idea but only to be deliberately broken, becoming the symmetry of a garden rather than that of fully planned human creation.[16] In spite of the inherent danger of the anachronism, one is tempted to describe the development of the geometrical pattern as one from *decoration* into *art*.[17]

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[15] The development is thus a reversal of that stylization of snakes into abstract lines which Gimbutas suspects in #12, and which is unmistakable in certain Cucuteni decorations (cf. note 12).

[16] An editor objects to this metaphor that "un jardin de Le Nôtre" seems to belong to the category of the fully planned. Actually, this exception to what gardens are in most human cultures illustrates the point: Le Nôtre's gardens came out of his studies of perspective theory and architecture, and they try to avoid the spontaneity of trees planted symmetrically but growing in asymmetric ways – or at least to reduce asymmetry to the level where it goes unnoticed, as in pre-Cycladic "impressionistic geometry".

[17] This distinction could be thought of in terms of the classical dichotomy where art – *poiesis* – was expected to be characterized by some kind of *mimesis* whereas decorative friezes were not; in this sense, the development in question is evidently but trivially pointing toward art. Less trivial and more pertinent in Kandinsky's century would be the observation that Cycladic decoration, as we move from the third into the second millennium, becomes increasingly bold when dealing with the tension between regularity and irregularity – in the end assigning to regularity the role of a decisive but hidden governing principle. (Better perhaps, hidden but decisive, *viz* if anything superficially mimetic shall be more than a heap of haphazard ingredients – the "classical" and the contemporary view of art are certainly more intimately connected than a naive reading of the above formulations reveals.)

I am grateful to my former colleague Paisley Livingston for forcing me to give the reasons for what started as a too facile intuition. I use the opportunity to thank him also for linguistic control. (Already because the text he read was a preliminary version, he is obviously not responsible for


Photograph # 37 (left). ArchM, Museum N° 1428. Mycenae, grave circle A, shaft grave 5, 1580–1550.
Photograph # 38 (right). ArchM, Museum N° 20. Mycenae, grave circle A, shaft grave 3, 1550–1500.


Photograph # 39 (left). ArchM, Museum N° 81. Mycenae, grave circle A, shaft grave 3, 1550–1500. A similar balance carries the bee of Photograph # 54.
Photograph # 40 (right). ArchM, Museum N° 669. Mycenae, grave circle A, shaft grave 5, 1580–1550.

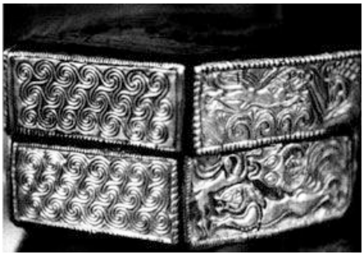
whatever clumsy phrases I may have produced later in the process).

The two faces of Mycenaean Greece

The photos from Mycenaean Greece are ordered in two separate sequences, #37–56 ("sequence I") and #57–70 ("sequence II"). Sequence I represents decoration of non-ceramic artefacts; sequence II shows what happened to pottery decoration. Whereas the latter sequence derives originally from Minoan, Cycladic and closely related styles, and therefore shows the gradual transformation of a borrowed aesthetics, the former is, since its beginnings, completely different from these (and no less different from the Orchomenos decorations #25–30). It may hence legitimately be regarded as an expression of a "native" style of the Mycenaeo-Greek tribes.

#37–55 come from the "Grave Circle A" excavated by Schliemann (but similar artefacts have been found in other Mycenaean contexts, for example in 15th-c. Aïdonia). Except for the stone stela of #37, all of them must be characterized as *Kleinkunst*.

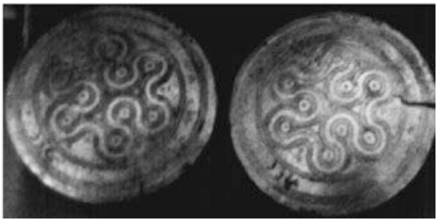
The geometry of this sequence differs in character, not only from the Middle Cycladic and Minoan but also from the Old European style, concerned as the latter had been with visual impression, often geometrically regular at the level of immediate perception but imprecise below that.



Photograph # 41 (left). ArchM, Museum N° 261. Mycenae, grave circle A, shaft grave 4, 1580–1550.
Photograph # 42 (right). ArchM, Museum N° 808. Mycenae, grave circle A, shaft grave 5, 1580–1550.

The gold roundel of #38 may serve to highlight this difference. The circular edge is made by means of a compass (by leaving the central point unerased, the artist has taken care that we do not overlook this fact); so are the circular arcs drawn inside it. The pattern is the one which arises when you try to structure the whole plane homogeneously by means of a compass with constant opening (as done in tree planting *in quincunx*, as it was called in Roman Antiquity), or when you draw longer arcs than needed during the construction of a regular hexagon. Even if not concerned with any kind of scientific geometry, the pattern of the roundel is more mathematical in its geometry than anything we have seen

thus far, representing a systematic exploration of the properties of the circle.[18] Exact measurement also shows that the six small circles are centred precisely with respect to the equilateral triangles inside which they are drawn.



Photograph # 43 (left). ArchM, Museum N° 682+685. Mycenae, grave circle A, shaft grave 5, 1580–1550.
Photograph # 44 (right). ArchM, Museum N° 334. Mycenae, grave circle A, shaft grave 4, 1580–1550.

The roundel in question comes from one of the later graves. If we compare it with the roundel of #40, or the gold belt of #41 (both of which are about one generation older), we find the same hexagonal symmetry and, in #40, almost the same circular arcs. But the arcs are not compass-drawn, and they are not drawn through the centre – any attempt to do so would indeed reveal their imperfection. These early specimens already demonstrate a search for the mathematical symmetry of #38, but the final result has not yet been achieved.[19]

Other pieces from the earliest shaft graves demonstrate a similar interest in geometrical perfection, yet not always as firmly based on mathematical regularity as #38. The square grid of connected spirals in the upper part of #37 is as regular as that of the Early Cycladic steatite box of #17 (exceptional, we remember, in its own time). Its 90° rotational symmetry, however, is determined from the rectangular frame which surrounds the grid and has nothing to do with the intrinsic geometrical properties of the spiralic pattern – as betrayed by the left side of the box of #42, where the virtual hexagonal symmetry of the pattern is allowed to unfold.

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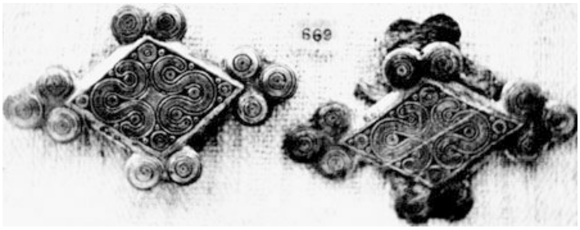
[18] The use of the same roundel type for the scales of the balance in #39 reminds us once more that this geometrical investigation is coupled inextricably with other concerns: that the device is purely symbolic is obvious – is is made from gold foil and thus not fit for carrying the slightest weight; its presence in a grave suggests some kind of religious meaning. This observation, however, does not preclude analysis of its geometrical character.

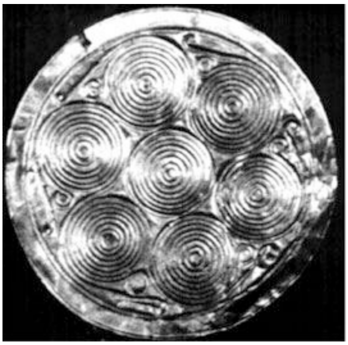
[19] The idea that circles have a hidden affinity to hexagonal symmetry is difficult to get at unless one makes experiments with a compass with constant opening. We may hence guess that the pattern of #40–41 presupposes inspiration from something like #38 but made in a different medium – possibly rope constructions of regular hexagons or related figures (or familiarity with regular tree-planting, for that matter).

The buttons of #43 carry two different patterns, both of which combine quadrangular symmetry and circles in a sophisticated way. On one set (buttons labeled 685) a pattern of concentric circles is transformed into a kind of meander, symmetrical about two mutually perpendicular axes and unchanged when an inversion (in naive formulation, an "inward-outward-reflection") is followed by a rotation of 90°. On the other set (labeled 682) a square is filled out by a combination of smaller and large circles (the latter combined two by two into incomplete "figures of eight"). Due to the skilful combination the completion of the square becomes mathematically coherent, even though the inscription of the square in an outer circle remains eclectic. The buttons in #44 exhibit hexagonal symmetry, but possess the same invariance under inversion+rotation as the first type in #43 (here under a rotation of 30°, not 90°).



Photograph # 45. ArchM, Museum N^o 669. Mycenae, grave circle A, shaft grave 5, 1580–1550.

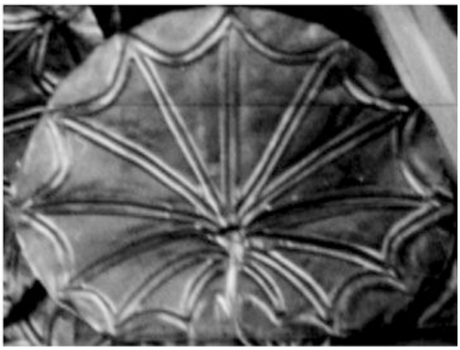
The two pieces of #45 show a three-circle variant of the "incomplete figure of eight", adapted to inscription in a double triangle (the lower piece is indeed composed of two



Photograph # 46 (left). ArchM, Museum N^o 10. Mycenae, grave circle A, shaft grave 3, 1550–1500.
Photograph # 47 (right). ArchM, Museum N^o 14. Mycenae, grave circle A, shaft grave 3, 1550–1500.

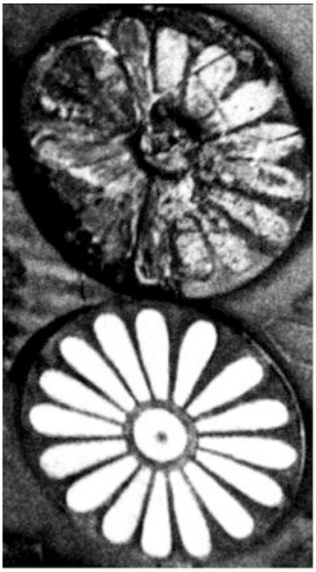
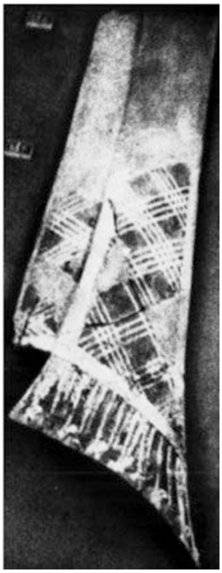
very precise equilateral triangles). Probably for reasons of material stability and aesthetic harmony, the diameters of the circles which are added externally deviate from what could be expected if triangular symmetry had been the sole and overriding concern. The deviations seem to be mainly *a posteriori*, however, and not *a priori* as in the third millennium items: high mathematical symmetry is the starting point.

Photograph # 48 (left). ArchM, Museum N° 18. Mycenae, grave circle A, shaft grave 3, 1550–1500.
Photograph # 49 (right). ArchM, Museum N° 18. Mycenae, grave circle A, shaft grave 3, 1550–1500.



Photograph # 50 (left). ArchM, Museum N° 4. Mycenae, grave circle A, shaft grave 3, 1550–1500.
Photograph # 51 (right). ArchM, Museum N° 13. Mycenae, grave circle A, shaft grave 3, 1550–1500.

The same relative but not absolute primacy of the mathematical structure over non-mathematical aesthetic considerations is seen in a number of gold roundels from Shaft Grave III (belonging to the "second" generation). With #38 (our starting point), #46, #47, #48, #49, #50 and #51 form a continuum in this respect. #38 was purely "mathematical",

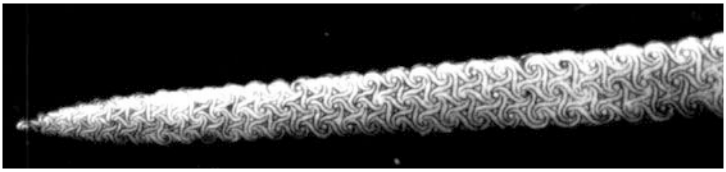
Photograph # 52 (left). ArchM, Museum N° 556. Mycenae, grave circle A, shaft grave 4, 1580–1550.
Photograph # 53 (right). ArchM, Museum N° 564+557+562. Mycenae, grave circle A, shaft grave 4, 1580–1550.

being built on what could be extended to a geometrical ordering of the complete plane by means of intersecting circles. The systems of concentric circles of #46 are arranged according to the same hexagonal symmetry, but the outer circles of the six systems in the periphery are opened in order to make the whole configuration fit harmonically within the circular border. #47 presents us with another solution to this problem, reminding of the triple circles of #45. In #47, however, the deviation from the basic mathematical pattern becomes more important than in #46: there is no longer any simple relation between the diameter of the concentric systems and the diameter of the circle which surrounds them, while those of #46 have a ratio of 1:3, and while the diameter of the small circles of #38 equalled the width of the "petals".

#48 conserves the hexagonal rotation symmetry and remains abstract, but mirror symmetry has been given up, and the six figures forming the pattern have no simple mathematical description. With #49 we enter the realm of figurative decoration – yet the octopus is more symmetric and regular than any octopus seen before or after, and indeed almost as symmetrical as at all permitted by the motif. Firstly, the axis of the body is a perfect symmetry axis; secondly, the spiraling arms are arranged in an almost regular octagon, and the centre of the outer circle and of this octagon coincides as precisely as can be measured with the foremost point of the body.

The bee of #50 exhibits only simple mirror symmetry, which is all the motif allows –

yet closer inspection of the figure reveals unexpected hidden mathematical regularities (quite the reverse of the "geometrical impressionism" of the third millennium); in this sense, the motif only serves as a pretext. Once more, the centre coincides with the foremost point of the abdomen (the only visible part of the body). The abdomen, furthermore, is fitted into the right angle formed by the hind edges of the wings; the front edges of these are curved but approach the prolonged hind edges asymptotically, and the whole configuration is thus determined by a pair of mutually perpendicular axes through the centre of the circle; finally, the insect is provided with ten wings in order to make all this possible. Only the stylized leaf on #51 accepts the requirements of the motif and relinquishes central symmetry completely.



Photograph # 54. ArchM, Museum N^o 744. Mycenae, grave circle A, shaft grave 5, 1580–1550.

The symmetry of the octopus and the ten-winged bee is not only found on the gold roundels. In #52 we see a rosette with 16 petals (most clearly to be seen on the reconstruction below the original), and in #53 a piece of a greave decorated with mutually perpendicular sets of parallel lines. As in the case of the bee, these are drawn at an inclination of 45° from the "vertical" line of the greave, and unlike seemingly related patterns from the fifth through the third millennium they are drawn with exactly four lines in each set, and with the distance between the sets equal to the width of each set. The use of precise geometrical relationships was obviously no prerogative of goldsmiths and jewellers.

Goldsmiths, however, have provided us with the most astonishing examples of geometrical attention. #54 is a sword blade decorated with the same pattern as #42, but under particular geometrical conditions. In contrast to many of the eclectic pieces from earlier periods, the pattern is adapted coherently without losing its own character (thus in a generalized sense, *conformally*) to these conditions which come to function as genuine *boundary conditions* not only in the direct but also in the mathematical sense. In #55, the tip of another sword blade, a series consisting of three lions is subjected to the same treatment, showing that even this series is basically dealt with by the artist as a geometrical pattern, irrespective of the naturalistic appearance of the single lion.

The final specimen from sequence I is a fragment of a 13th-century fresco from Mycenae. The picture (#56) does not show it clearly, but the original in colour demonstrates that the Mycenaean artists would have some kind of abstract notion (whether explicit

Photograph # 55. ArchM, Museum N° 395. Mycenae, grave circle A, shaft grave 5, 1580–1550.

or not we cannot know[20]) of the invariance under an inversion+rotation discussed in connection with #43–44. Both the necklace and the bracelets of the woman consist of red and yellow pearls; in both, the pearls are ordered in groups of three. Yet while the colour sequence of the necklace is …-y.r.y-y.r.y-…, that of the bracelets is …-r.y.r-r.y.r-… . In this case, switching both the two colours and the two positions thus leaves the system unaltered. The mathematical coherence of the geometrical decorations of #37–55 thus appears to reflect a more general mode of thought.

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[20] Exactly the same commutative group can be dug out from Adalbert von Chamisso's "Canon" [*Werke* I, 85]:

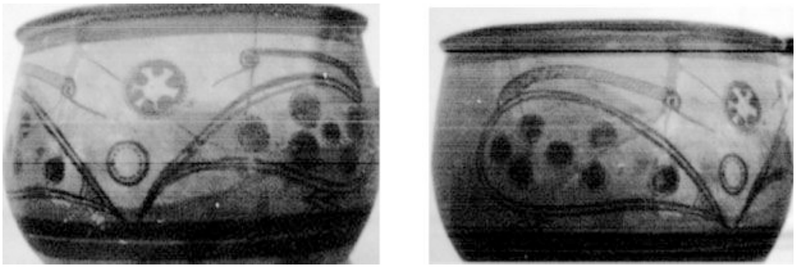
Das ist die Not der Schweren Zeit!
Das ist die schwere Zeit der Not!
Das ist die schwere Not der Zeit!
Das ist die Zeit der schweren Not!

– but nobody would suspect Chamisso of having thought of this elegant game as a piece of mathematics.

Photograph # 56. ArchM, Museum N° 11671.
Fresco, Mycenae, 13th c.

Photograph # 57 (left). ArchM, Museum N° 199. Mycenae, grave circle A, shaft grave 1, 1550–1500.
Photograph # 58 (right). ArchM, Museum N° 7107. Mycenaean ware, "palace style", 15th c.

Sequence II, #57–70, shows that the case of Mycenaean ceramic decorations was different.

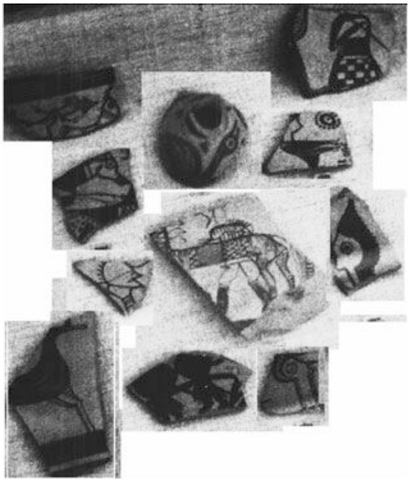


Photograph # 59. ArchM, Museum N° 1275.
Mycenae, Acropololis, 14th–13th c.

The 16th-century vase in #57 is very close to the style of the various pieces from Melos from the same or slightly earlier periods; the style is imported, if not the vase itself, and almost as different as can be imagined from that of sequence I.

Photograph # 60 (left). ArchM, Museum N° 6193. Attica, Perati, 13th c.
Photograph # 61 (right). ArchM, Museum N° 9151. Attica, Perati, 13th c.

The further development of Mycenaean ceramic decoration presents us with an increasing interaction with the geometrically regular tradition. Already the 15th century palms and ivys of #58 have lost some of the free movement of former times, organized as they are within an approximate mirror symmetry; each of the two birds of #59 are still very soft in their lines, but once again the composition as a whole is tendentially symmetric. Much more constrained by symmetry are the octopus figures of #60 and #61,



Photograph # 62 (left). ArchM, Museum Nº 3559. Mycenae, chamber tomb E, 15th–13th c.
Photograph # 63 (right). ArchM, various museum numbers. Mycenae, Acropolis, fragments, "pictorial style", 13th–12th c.

the contrast of which to the octopus of #34 is about as great as can be. Beyond the symmetry of the animals we also notice the emergence of non-figurative elements: hatchings and zigzag-lined ribbons, as well as spirals and systems of concentric circles used as eyes.

At closer inspection, however, these elements of "geometric" decoration turn out to be widely removed from the mathematical geometry of the 16th-century items discussed above. The number of lines in the bands of parallel lines between the arms of the octopus in #61 varies, it seems, according to nothing but the aesthetic sensibility of the artist (certainly a most pertinent criterion in an object which must somehow have been meant to be beautiful, and a better choice than obsession with arithmetical uniformity); the spiraling eyes of #60 do not follow the symmetry of the figure – both approach the centre in a clockwise movement.[21] The "mathematical" experiments of the 16th-century Mycenaean court appear to have been left behind; once more the geometrical regularity has become one of immediate visual impression (but no hidden governing principle, cf.

[21] It is true that the two fish below the octopus exhibit the same translational symmetry; in contrast to what could be argued in the case of #10, it is therefore not to be excluded that this piece presents us with an intentional clash between two irreconcilable symmetries.

However, two similarly oriented fish can be found in the kindred #61, in which the pattern of hatchings between the arms exhibits no similar translation symmetry; intentional symmetry breaking in #60 therefore remains an unconvincing possibility.

note 17). It is tempting to see this stylistic simplification as a symptom of the decline of courtly wealth and of the disintegration of the Mycenaean social system. Some of the elements (e.g., the particular constitution of the systems of concentric circles) are so close to Early Cycladic specimens that we must presume a survival of these forms outside the courtly workshops; such survival is even more obvious in #62, as impressionistic as anything similar from the third millennnium, and clearly akin to #23.

Photograph # 64 (left). ArchM, Museum N° 1511+10549. Tiryns, Acropolis, 13th c.
Photograph # 65 (right). ArchM, Museum N° 1426. Mycenae, Acropolis, mid-12th c.

"Decline" is also visible in the drawings of humans and animals in #63–65, if one compares them with the pictorial representations of hunting and war scenes of the 16th century (one example can be discerned on #42; a better reproduction is [Marinatos 1976: Plate 220]) – and the continuation of certain stylistic features indicates that comparisons can legitimately be made.[22] Inside the drawings of living creatures, many of the fragments of #63 show hatchings, chessboard patterns and other features reminding of the Neolithic decorations discussed above. The same holds for the Tiryns vase (#64), which at the same time shows hints of that repetitiveness which has become the dominant characteristic of the "vase of warriors" (#65) and of the non-figurative #66 (the repeated element of which can be compared with the constituents of #46). Since repetitiveness is in itself an elementary but visually obvious variant of geometrical regularity, the whole tendency of Late Mycenaean figurative decoration can be seen as a gradual sliding-back

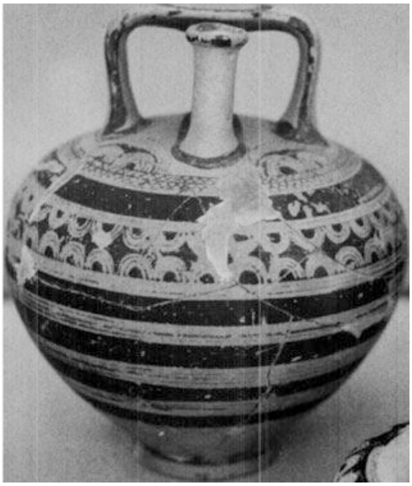
[22] Thus, the Donald-Duck faces and the thighs of the Tiryns-vase (#64) are both developments of less abnormal characteristics of the hunters portrayed on the blade of a 16th-century dagger (Museum number 394; photo in [Petrakos 1981: 30]). Other items with figurative decorations from the early period are so close to Near Eastern styles, it is true, that they must be considered as borrowings and to be thus less relevant to a diachronic comparison.

into primitive geometry – as could perhaps be expected in a situation where the social division of labour itself became less complex (cf. note 12), but which does not preclude that certain pieces are very finely made and very beautiful – as is #67.

Photograph # 66 (left). ArchM, Museum N° 12163. Mycenae, Acropolis, 14th–13th c.
Photograph # 67 (right). ArchM, Museum N° 7626. Mycenae, "house of the oil merchant", 13th c.

From "geometric style" toward, and away from, geometry

The "Geometric" style that develops from the Submycenaean age (12th to earlier 11th century) onwards is thus, in a way, the ultimate consequence of developments which had started in the outgoing Mycenaean era; #66–70 demonstrate this clearly. The extreme geometrization of pieces like #75–77 is, so to speak, the point toward which the Late Mycenaean stylistic changes (as continued during the Submycenaean and Protogeometric phases, #71–74) are directed if they should end up in a fully coherent style; that they should end up so was of course no historical necessity, and eventually it turned out to be only an ephemeral phenomenon).



Photograph # 68 (left). ArchM, Museum N° 3493. Mycenae, chamber tomb, 15th–13th c.
Photograph # 69 (right). ArchM, Museum N° 5197. Attica, end of 13th c.

The Submycenaean and Protogeometric phases are represented by a few photos only, but some essential points can be described in words.[23] The broad black bands so characteristic of Early Geometric pottery go back to "Mycenaean IIIB" (c. 1300 BCE) – some characteristic specimens are depicted in [Finley 1970: 64]. The systems of concentric circles, too, have Mycenaean antecedents, as demonstrated by #70. They are immensely popular on Protogeometric pottery (c. 1050 to c. 900), but they are also found on earlier 11th and 12th-century ware. In the tenth century they are, as a rule, drawn by means of a multiple-brush compass, as on the Mycenaean #70. During the 12th and most of the

[23] The description is based on the exhibitions of Attic ware in the Oberländer and Agora Museums in Athens. Some of the plates in [Whitley 1991] allow similar observations.

11th century, on the other hand, this “professional” technique is absent, and the circles are drawn by hand. Throughout the period the centres are often located at the edge of a black band, and only semicircles are drawn, as in #72, left, and #73, right. Inside the inner circle or semicircle, a straight-line figure (an “hour-glass” or a cross) is often found.

Photograph # 70 (left). ArchM, Museum Nº 3198. Mycenae, chamber tomb, 15th–13th c.
Photograph # 71 (right). ObM, Athens, Submycenaean, Grave Nº 136, 11th c.

Many constituents of the Early Geometric style – zigzag lines, meanders, hatchings etc. – are evidently comparable to elements of earlier styles. Their reappearance after their virtual absence during the Submycenaean and early Protogeometric periods looks like a consequence of an inner logic of the style and/or its cultural context. At the same time, several of these constituents are so specific that we must suppose them to represent a surviving tradition – thus the hexagonal rosette of #38 and #78, or the eight-fold “flower” at the top of #80, which is already present in this precise form on a 14th-century gold roundel in the Agora Museum. Revivals of waning or rarefied traditions, however, only come about if these become adequate once again within a changing technological, social or cultural horizon.

Even the incomplete repetitiveness of abstract as well as figurative decorations comes so close to the idea of certain late Mycenaean items that we may guess at the existence of a continuous undercurrent which, under new conditions, rose to prominence once again (compare #64–65 with #78 and #79; 16th-century examples are found, e.g., in [Marinatos 1976: Plates LII and 218]).

Photograph # 72 ObM, Athens, Early Proto-geometric, grave N° hs 130, 11th c.

The 16th-century Mycenaean court style and the geometric splendour of the eighth century *may* thus be sophisticated manifestations, in different cultural and social contexts, of certain basic traditions, practices or ideas. Yet in spite of this possibly shared background the two are conspicuously different. Whereas the Mycenaean court style had pursued mathematical regularity, regularity below the level of the visually obvious is a minor concern in Geometric pottery. Attentive scrutiny of the repeated elements of a repetitive pattern makes their apparent identity fall apart: the number of strokes in a hatching or a herring-bone pattern varies; one vertical zigzag line begins in the upper left corner, one to the right, and one in middle; one has 21 apices, another 24; etc. The underlying geometry is different from that of the outgoing third millennium, it is true; the emphasis on visual impression rather than precisely controlled regularity or deliberate breach of symmetry, however, is the same. *If* the Mycenaean court style and the Geometric style are manifestation of a common background vision or aesthetics, then the Mycenaean inclination toward genuine mathematization seems not to belong to this shared background, at least not as it survived in the early first millennium.

Photograph # 73. ArchM, Athens, Museum N° 18076. Late protogeometric, undated in the Museum

Photograph # 74. ObM, Athens, Transition Protogeometric–geometric, cremation burial of a warrior, 875–850.

Photograph # 75 (left). ArchM, Museum N^o 216. Attica? 850–800.
Photograph # 76 (right). ArchM, Museum N^o 185. Middle Geometric II, 800–760.

Photograph # 77 (left). ArchM, Museum N^o 812. Dipylon, Athens, Late Geometric I A, 760–750.
Photograph # 78 (right). ArchM, no museum number indicated. Dipylon, Athens, Late Geometric 1B, 750–735.

Photograph # 79 (left). ArchM, Museum N° 17935. Attica? 720–700.
Photograph # 80 (right). ArchM, Museum N° 77.(19.762). Attica? Archaic period, 700–600.

As at the turn of the third millennium, the further development of the Geometric style was to change geometric into more living forms. Already in the eighth century certain rosettes consist of indubitable leaves, and a culmination of this trend is seen in #80 (seventh century). Still, the “life” of this amphora is characterized by being a transformation not of spirals (as the second millennium aquatic plants etc.) but of a stiff linear geometry. With or without influence from the “orientalizing” style (see #81), however, further development through repetitive (#82) and gradually less repetitive (#83) human and animal forms was to lead to the free artistic form of Classical vase painting, in which the geometrical knowledge of the artist is only used silently for balancing the picture and making it dynamic (if we disregard the meanders which occasionally border the figurative paintings).

A corresponding development is seen in the sculptural arts. The *kouroi* of the early Archaic age appear to be strongly inspired by a late variant of the Egyptian canonical system. Not only are the numerical proportions between the parts of the human body observed, but the body’s whole posture is determined so as to correspond to a specific square grid (see [Iversen 1971: 75–77]; compare in particular #84 with the *kouros* inscribed in a square grid on p. 77). At this moment, mathematical regularity (here primarily

proportion) is thus not only a governing principle but also a visually outstanding feature. Sculptures from later periods are still made according to those proportions which were deemed harmonious and therefore beautiful. Yet the system became less fixed; being now a subservient means to achieve the artistic end, geometry became an underlying regulative force. Sculptures like #85 and #87 demonstrate to which extent the posture of the human body was made an expression of character and emotion, freed from all *visible* mathematical constraint. Even when Nature was stylized into abstract pure form (as in #86, from the Poseidon temple in Sounion) the shapes which occur are quite different from those simple curves – the circle and the straight line – which were canonized in theoretical geometry precisely during the epoch when the Sounion temple was built. At the time when mathematics evolved into an autonomous intellectual pursuit, and when Oenopides and Hippocrates started the development which was to end up as axiomatization, the artists for their part stepped into a realm of forms far beyond the reach of scientific geometry. At least one reason for this emerges from Vitruvius' discussion of the dimensioning of columns (*De architectura* III.iii.10 – ed., trans. [Granger 1931: I, 176-181]): these have to be narrower at top than at bottom in proportions depending on their height; they have to swell in the middle; those in the corners have to be a bit thicker than the rest – and all because "what the eye cheats us of, must be made up by calculation". This purpose was served much better by concrete rules based on experience and reduced to elementary numerical formulas than by any geometrical theory. Which mathematical theory would ever be able to tell the artist that the line defined by the three heads in #87 should descend

Photograph # 81. ArchM, Museum N° 12130 and 12077. Eretria, "Orientalizing style", 7th c.


Photograph # 82 (left). ArchM, Museum N° 530. Attica, 600–550.
Photograph # 83 (right). ArchM, no museum number indicated. Pharsala, Thessaly, c. 530.

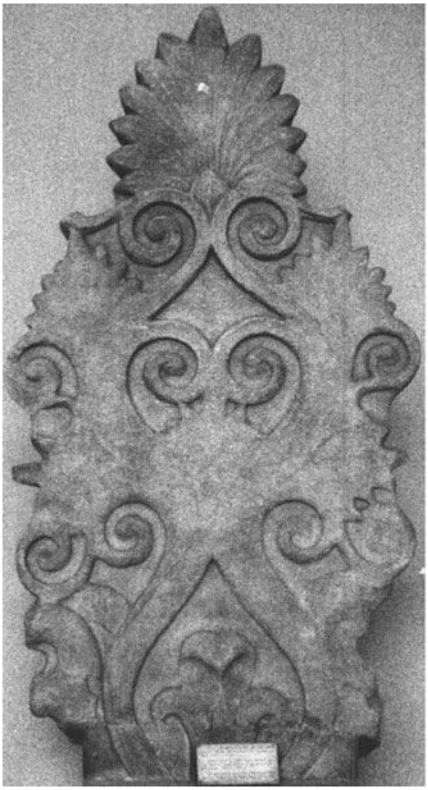
toward the right (as it does indeed) in order to confer the feeling of calmed passivity involved in deep sorrow, while descent toward the left could have had quite inappropriate implications?

Photograph # 84 (left). ArchM,Museum N^o 3645. Cape Sounion, Attica, 600–590.
Photograph # 85 (right). ArchM, Museum N^o 15101. Attic bronze, c. 460.

A moral?

If we are to learn any lesson from our story, a bird's-eye view of the development may be useful. The Old European Middle Neolithic confronts us with simple patterns: zigzag lines, rhombs, etc. No effort is made to achieve geometrical coherence between the various parts of a decoration. Further on greater fantasy manifests itself in the choice of forms, and various symmetries and other invariants are explored. From a start in pure decoration the geometrical pattern develops toward structural experiments.[24] This development, however, is never carried to its mathematical consequence: eclectic decoration endures, the style remains one of geometrical impressionism. With the partial exception of #17, no attempt is ever made to explore the inherent formal ("mathematical") properties of the shapes and symmetries dealt with. Throughout the period decorative and artistic

[24] It may be worthwhile repeating once again that this distinction only concerns one axis in the multi-dimensional grid in which the character of the decorations can be located; in particular it does not anticipate the answer to questions concerning their possible symbolical function.



Photograph # 86 (left). ArchM, Museum N° 1112. Poseidon Temple of Sounion, c. 440.
Photograph # 87 (right). ArchM, Museum N° 723. Athens, early 4th c.n

concerns are overriding (together probably with symbolic and similar concerns which, however, could equally well express themselves one way or the other).

As the Middle Cycladic offspring of Old Europe falls under the influence of Minoan Crete (itself largely an Old European offspring), this dominance of artistic concerns undergoes a qualitative leap: instead of introducing figurative painting as a supplement to the old geometrical decoration, the geometrical design itself is changed and vitalized. The pattern is transformed into naturalistic or quasi-naturalistic art; what remains of geometrical principles is mainly the use of deliberately broken symmetries that serve to balance the composition while keeping it tense.

The “native” Mycenaean tradition is different. We first met with it in the shaft-graves of Mycenae where, over one or two generations, a high level of regularity developed into genuine mathematical structuring. Later Mycenaean art becomes less mathematical, as we see it in the ideals which are pursued in the “normalization” of the borrowed Minoan

vase painting style; the development through Protogeometric and Geometric art suggests, however, that the early Mycenaean bloom is a high-level manifestation of a general cultural substrate where straight lines, circles, and quadratic, hexagonal and octagonal (and even abstract) symmetries are important. Even though the geometrical impressionism of the Geometric period never evolves into structural mathematical inquiry (but eventually, like the impressionism of the third millennium, into "art"), this second bloom of professional art among the Greek-speaking tribes shares some fundamental characteristics with its Mycenaean predecessor. For some reason Greek culture maintained an interest in circles, squares, hexagons and octagons for more than a thousand years *before* theoretical geometry emerged.

That geometry was one of the fields that were made the objects of *theory*, along with more obvious fields like cosmology and health, may perhaps owe something to the existence of such a substrate. The name given to the subject, it is true, demonstrates that the "metric" component of geometrical thought was assumed by the Greeks themselves to be its essence. As suggested by Wilbur Knorr [1975: 6*f* and *passim*] and others, however, the strand leading to *Elements* II etc. did not constitute the whole rope, and *Elements* III and its kin could be the ultimate outcome of theoretical reflection inspired by favourite shapes – just as the "metric" component may be the outcome of theoretical elaboration of Near Eastern mensuration geometry (this is not the place to investigate the interaction and mutual fecundation of the two currents).

Even so, although this kind of inspiration is indisputably possible, there is no path leading from the decorations of Geometric vases to theoretical geometry. This is evident already from plain chronology, since Geometric vases disappear long before anybody imagines theoretical geometry to have arisen; moreover, it is hardly possible to find any element in mature Geometric art which points to the specific interests of Greek (metric or non-metric) geometry. Geometric art reflects interest in geometrical shapes and symmetries, but in contrast to Mycenaean art it is not a medium through which these are submitted to further formal (and hence "mathematical") scrutiny or experiment.

Similar conclusions can be drawn regarding any geometrical impressionism. Geometrical impressionism demonstrates the presence of an aesthetics of visual order and (generalized) symmetry, but it also proves that the artist is satisfied by fulfilling the requirements of this aesthetics, and is not interested in further investigation of formal properties.

Decorative patterns are not always impressionistic, and the decorations of many cultures not discussed here can be regarded in full right as expressions of formal investigation and experiment.[25] The moral of the present tale is, firstly, that we should

[25] Indeed, much of the decorative art of Subsaharan Africa contains such formal investigation and experiment. This has been amply demonstrated by Paulus Gerdes and his collaborators in a number of books, as I have pointed out in reviews which I quote here:

be careful not to extrapolate from *every* piece of geometrical decoration to such extensive symmetries which may be superimposed on its pattern but which are not needed to explain it (and, worse, may be contradicted by its details); secondly, that no necessity leads from an *aesthetics of forms* to *formal investigation of forms*, nor from *formal investigation of forms* to *integration with mensurational geometry* or *into mathematics as a broader endeavour* (whether provided with proofs or not).

We should respect that not everybody prefers the ideals expressed in #37 (even if we disregard the warrior on his chariot) to those inherent in #34.

"All the examples explored by Gerdes (and sub-Saharan geometrical decoration in broad average as far as the reviewer is aware) belong to the […] type" which "bears witness of deliberate explorations of symmetries and other formalizeable properties of figures; its actual drawings need not be very precise, but they contain an underlying formal structure" ([Høyrup 1996], review of [Gerdes 1994]).

"The […] weavers" of "sipatsi: handbags woven from white and coloured straw exhibiting geometrical strip patterns" are "very conscious of the numerical principles underlying the patterns and very critical of irregular patterns arising from sloppy counting or insufficient mental calculation. Mathematical regularity is thus anything but a mere result of the constraint inherent in the technique" ([Høyrup 1997a], review of [Gerdes & Bulafu 1994]).

"[…] the specialists in question do not look at themselves as 'mathematicians', a role for which traditional society has no space; but many of the patterns shown in the book exhibit symmetries that bear witness of intense reflection on formal properties of patterns. These are not restricted to invariance under the combination of reflections in lines and points, translations, and rotations, but also involve abstract invariances under combinations of spatial transformations and colour inversion (or even switches between monochrome and hatched) and symmetry breakings that arise when locally symmetric configurations are inserted in a global pattern with a different symmetry." ([Høyrup 1997b, review of [Gerdes 1996]).

The *sona*, line drawings made in the sand, "represent specific objects, situations, proverbial sayings, or even stories, and they were an essential part of the teaching surrounding the adolescential circumcision. All adults would therefore be familiar with some of the simpler patterns, but the more complex ones would only be known by a restricted group of specialists who kept them jealously as secrets. In consequence, the tradition is almost lost today, and the first part of the book therefore aims both at presenting and analyzing a large number of sona documented in the literature, and at reconstructing the algorithms and composition principles which allowed the masters to perform them (as required) without the least hesitation" ([Høyrup 1999], review of [Gerdes 1997]).

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Chapter 8 (Article I.7) Broad Lines – A Forgotten Geometrical Ambiguity

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A few additions to the translation in ⟦…⟧

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Abstract

The following pages explore the traces of a way of thinking of areas and lengths that seems strange to us but which in a number of mathematical cultures was so familiar that there was no need to explain it: so strange and so familiar, indeed, that modern historians have declared passages in the sources that reflect this conceptual structure to be erroneous, confused or meaningless. I refer to the habit to imagine lines as carriers of a virtual breadth equal to one length unit, and areas as composed of such unit strips. This habit was widely spread in many environments of practical mensuration. In these it was easy to agree on using the basic unit of length as standardized breadth: land was, so to speak, measured as cloth is still sold nowadays, with its physically determined breadth. But it is also reflected in texts which are more theoretical in origin though still inspired or coloured by practical mensuration and its language.

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In memoriam
A. P. YOUSCHKEVITCH, 1906–1993
and
IVOR BULMER-THOMAS, 1905–1993

Italian mensuration, from Fibonacci to Pacioli

In the introduction to his *Pratica geometrie* [ed. Boncompagni 1862: 3–4], Leonardo Fibonacci explains linear and surface measures. In the first lines, nothing will surprise a modern mind: a *cubita superficialis* is a square whose sides equals a *cubita linealis;* the same relation holds for the *ulna superficialis* with regard to the *ulna linealis* and for the *pertica superficialis* with regard to the *pertica linealis.*

Next, Fibonacci proceeds with the system used in his hometown Pisa – which is distinct from what we are accustomed to. A *pertica* ("rod") consists of 6 feet, and a foot of 18 points or ounces; and even a square or surface rod consists of 6 surface feet since, as Fibonacci explains, a surface rod has the length of a rod and the width of $^1/_6$ rod (that is, a foot). The measure of a foot in square is instead called a *denarius.* A surface *ounce* is similarly a rectangle 1 rod×1 ounce.

Larger areas are measured in the units *scala* (= 4 rods), *panorum* (= $5^1/_2$ rods), *stariorum* (= 12 *panori*) and *modiorum* (= 24 *staiori*). All of these are primarily surface measures, but they are also understood as linear measures – where 1 linear *panorum* is the length which, when provided with a width of 1 rod, corresponds to an area of 1 surface *panorum.* They are used, Fibonacci informs us, in the trading of fields, building sites and houses – that is, in practical life.

We find as much when Luca Pacioli explains in the geometrical part of his *Summa de arithmetica* [1523: II, fols 6^v–7^r] the metrology used in Florence when land is sold. Here, the recurrent breadth is the *braccia* ("cubit"):

> Multiplying cubit by cubit: makes square cubit.
> Multiplying cubit by *pugnora* makes *pugnora.*
> Multiplying cubits by *panora* makes *panora:* multiplying by *staiora* makes *staiora.*
> … ⟦[1]⟧

(1 *staioro* = 12 *panori* = 12^2 *pugnori* = 12^3 *braccia*). The whole structure of the exposition, as well as the metrology that is involved, show that in this passage of the text Pacioli does not depend on Fibonacci. He thus speaks of a system which was really used in his times in Florence.

1 ⟦Multiplicando bracia per pugnora fanno pugnora.
Multiplicando bracia per panora fanno panora: multiplicando per staiora fanno staiora.
….⟧

Pharaonic Egypt

This way to think of areas as composed of strips of standard breadth is found in other practical metrologies. A well−known example comes from ancient Egypt [Peet 1923: 24−25]. In the measurement of land, the linear measure *khet* ("rope") equal to 100 cubits was used. The basic surface unit of school texts was the *setat,* a square *khet.* Practical mensuration, however, more often made use of the "cubit-of-land" and the "thousand-of-land", with one side equal to a cubit respectively 1000 cubits, and the other equal to a *khet.*

The metrology described by Peet belongs to the second millennium BCE. Even though the mathematics of Hellenistic and Roman Egypt was eclectic in character, however, with influences of Babylonian, Persian or Aramaic origin, the feature which interests us here recurs in Demotic papyri. The "cubit-of-land" thus turns up in a papyrus from the Roman epoch [ed. Parker 1972: 71]. Even more remarkable is another passage from the same papyrus, in which the *aroura* (the square *khet*, the square with a side equal to 100 cubits) is used as a length unit, equal to 100 cubits (without indication of the name of the unit, it is true, but no other unit of 100 cubits existed at the epoch) [ed. Parker 1972: 72].

Old Babylonian mathematics

The presence of the same structure in Old Babylonian mathematical thought is less conspicuous in the metrology and hence less familiar. Familiar is instead the role of an analogous understanding of the measure of volumes. For horizontal distances, the basic unit was the n i n d a n or "rod" (equal to 12 cubits, thus roughly thrice as long as the Roman *pertica*). For vertical distances, on the other hand, the basic unit was the cubit. Volumes were measured in surface units, understood simply as provided with a height of 1 (that is, 1 cubit). In other words, when volumes were thought of, the surface units were seen as slices whose thickness equalled 1 cubit.

In order to see how the same idea enters into the conception of surfaces we must analyze the terminology that was used to describe their calculation. Among the terms habitually translated as multiplication, two seem to be linked to this calculation: *šutākulum* (or, rather, *šutakūllum,* probably meaning "cause the two 'factors' to hold together", namely as the sides contain a rectangle) and *našûm,* "to raise", both with synonyms[2]. However, *šutakūllum* does not refer to the calculation but to *the construction* of the rectangle. Usually, this construction implies a calculation of the area of the rectangle, which is stated immediately after the construction. At times, however, the calculation is

[2] The various "multiplicative" terms of Old Babylonian mathematics and their interpretation are discussed in [Høyrup 1990a: 46−49 and passim]; the origin and development of the concept of "raising" is addressed in [Høyrup 1992: 351−52]. ⟦Also in article II.7.⟧

spoken of in a separate clause; moreover, the area of a rectangle which has been constructed previously is determined by "raising". Areas of triangles, trapezia and irregular quadrangles are always found by "raising".

In general, "raising is used for all calculations of a concrete magnitude via multiplication. Usually, the order of factors depends on purely stylistic considerations; for instance, it is usually the magnitude which has just been determined that is "raised" to the other one. There is an exception, however, but only one: in the determination of volumes, it is invariably the base B that is "raised" to the height h. The term thus represents a live metaphor when the determination of volumes is concerned, and a dead one in all other contexts – which means that the origin of the metaphor is exactly in the calculation of volumes: a prism $h \times B$ is obtained when the "virtual height" 1 cubit of a surface B understood as slice is "raised" to the actual height h. The idea corresponds to the definition of multiplication given in Euclid's *Elements* (VII, def. 15): the product of h and B contains B as many times as h contains unity.

The transfer of the metaphor to the measurement of surfaces presupposes that these are conceived as composed from strips (of breadth 1 n i n d a n, as volumes are composed of slices of thickness 1 cubit). Then, a rectangle $a \times b$ is obtained by "raising" a strip $1 \times b$ to its true breadth a.

The idea of a "virtual breadth" also manifests itself in the so-called "algebraic" texts. It is an old observation that in these texts, lengths are added to or subtracted from areas – operations that have always been seen as geometrically absurd, which has been one of the arguments to understand this "algebra" as a purely numerical technique, its geometrical vocabulary notwithstanding.[3] As I have shown elsewhere,[4] this conclusion is mistaken: a numerical interpretation explains the numbers that appear in the texts, but neither the structure of the terminology nor the details of the verbal exposition, nor the order of mathematical operations; instead, these levels of the text impose a geometrical reading.

Indisputably, certain Babylonian texts regard the addition of lengths and surfaces as ambiguous. In order to speak of it they choose a term that allows the addition of the measuring numbers of the two magnitudes without any reference to their concrete signification, and in order to transform the question into a geometric problem they then provide the length with a "projection 1", a breadth that transforms the line s into a rectangle $1 \times s$. This device eliminates the ambiguity.

However, when it comes to the subtraction, for instance of a side from the area of a square, no similar distinction between numerical and concrete operation is at hand. Other texts, moreover, make the addition of lengths and areas without recourse to this distinction,

[3] "It is true that they illustrated unknown numbers by means of lines and areas, but they always remained numbers. This is shown at once in the first example, in which the area xy and the segment $x-y$ are calmly added, geometrically nonsensical" [van der Waerden 1962: 72].

[4] See [Høyrup 1990a].

and without introducing the “projection”. Instead, they assume that the lengths are strips that can be joined with or cut away from the surfaces.[5]

Even though the metrological evidence is not quite clear, we thus find Old Babylonian mathematical thought (in particular when we approach geometrical practice) to presuppose a notion of lengths as carriers of a “virtual width”.

Euclid – “Heron” – Plato

Euclid declares that a line is a length without breadth (μῆκος ἀπλατές – *Elements* I, def. 2, [ed. Heiberg 1883: 1]). Neither Heron’s *Definitions* [ed. Heiberg 1912: 14–16] nor Proclos’s commentary [96–100; trans. Morrow 1970: 79–82] suggest this delimitation (the etymological meaning of ὅρος, translated “definition”) of the concept should be meant to fend if off from the idea of a strip-line. At this level of Greek mathematics – that of mature theory – there is no trace of the idea of lines carrying a virtual breadth.

At other levels – those closer to practical mensuration or polemicizing against its habits – at least traces do exist.

Firstly, there are two pseudo-Heronian anthologies, *Stereometrica* and *De mensuris* [ed. Heiberg 1914], in which it is explained how to find “how much results from 1 foot above (ἐπί) 1 foot” (*Stereometrica* II.69 [ed. Heiberg 1914: 160–162]; slightly different *De mensuris* **27** [ed. Heiberg 1914: 182]).

The *Stereometrica* version discusses first 1 foot above 1 foot, multiplying 16 by 16 (since “1 foot contains 16 fingers”). Then, in order to find and explain $1^1/_2$ foot above $1^1/_2$ foot it multiplies 24 by 24, dividing afterwards the resulting number 576 by 16; that is, it transforms the square into a strip with breadth 1 foot, and finds it length to be 36 [fingers] or $2^1/_4$ feet.

That we are really dealing with an intuition of strips and not with a mere calculational scheme is shown by the third calculation, that of $(^1/_2+^1/_4)$ foot above $(^1/_2+^1/_4)$ foot. The text multiplies (8+4) by (8+4) and finds 144. But instead of dividing by 16 (which would produce a somewhat counter-intuitive “strip” with a breadth exceeding the length) it relates the product directly to 16×16 = 256 and finds the ratio to be $^1/_2+^1/_{16}$.

Until this point, *De mensuris* does the same; but while *Stereometrica* stops here, *De mensuris* goes on with 2 feet above 2 feet and $(2+^1/_2+^1/_4+^1/_8+^1/_{16})$ feet above $(2+^1/_2+^1/_4+^1/_8+^1/_{16})$ feet; in both cases it returns to the transformation into a strip, confirming thus that this is the method to be used when a genuine strip is produced.

[5] The texts that presuppose a virtual breadth seem to be those which are closest to the origin of the discipline, the first elements of which appear to have been adopted by the scribe school from a surveyors’ environment, perhaps around 1800 BCE – see [Høyrup forthcoming] ⟦more precise dates can be found in article II.8⟧. The terminological distinction and the explicit notion of a “projection” are thus secondary phenomena, repercussions of a schoolmasters’ critique of the ambiguities that had initially been taken over together with the techniques of the practical geometers.

De mensuris **25** [ed. Heiberg 1914: 180] – an (either erroneous or corrupt) alternative determination of the capacity of a theatre – presents us with a second example. It finds the area reserved for the spectators as the product of the external and the internal perimeter (100 feet and 80 feet, respectively). This is also the number of spectators, since “on one foot sits a man, that is, on 16 fingers”. Here Heiberg, thinking as a modern mathematician observes that this should be “256 fingers”, since an area is involved. However, the words of the text are justified if the area is perceived as strips 1 foot large and of total length 8000 feet, in particular if the breath of the steps is 1 foot; but since the text seems to find the latter argument to be superfluous, the idea to conceive an area in this way was apparently felt natural.[6]

Less ambiguous than these is an example that is found in the *Geometrica,* yet another pseudo-Heronian anthology (**24**,3 [ed. Heiberg 1912: 418])[7]. Here, a problem similar to those of Old Babylonian “algebra” (and indeed a problem that appears to be rooted in that very agrimensorial tradition which had inspired the Old Babylonian scribe school – cf. [Høyrup, forthcoming] and note 5): It deals with a square, whose area (A) together with its perimeter (4ℓ) amounts to 896 feet, and asks for the separation of the area from the perimeter. The 4 units are “put outside” (εκτίθημι) the square. Half of the 4 is taken, and 2 *feet* result – which implies that the side is really conceived of as a rectangle with length ℓ and width 1 foot. The [line of] 2 feet is put above itself, and 4 feet result. If these are joined to the 896, 900 feet result, and as side of the square thus 30 feet. Since from the 4, one half has been “taken away below” (ὑφαιρέω), two feet result [as remainder]; [as side of the original square] 28 feet remain. The area is thus [28×28 =] 784 feet, and the perimeter [4×28 =] 112 feet. If everything is put together, 896 result, which is the area together with the perimeter. The procedure can be followed on Figure 1.

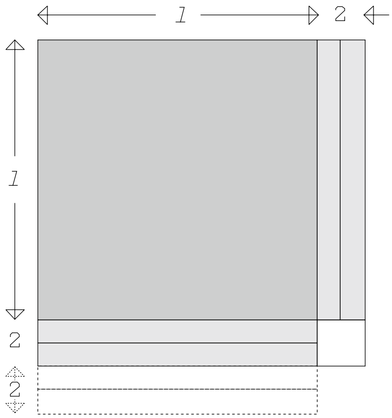

Figure 1.

[6] The width of the steps of real theatres is of no use. In the preceding chapter (whose calculation is mathematically correct), the distance between steps is around 10 cm! Even here, each person occupies a length of 1 foot.

[7] Heiberg’s translation hides part of the argument; I provide a close paraphrase of the Greek text.

Heiberg claims the compiler to have copied without understanding. This is of course possible, but Heiberg's argument seems to be mistaken.[8] But even if he has copied, he has copied a Greek text written by somebody who understood. He neither translated a text written in another language, nor did he copy a tradition created without understanding; this is shown not only by the use of Greek metrology (the foot) but also by stylistic divergence with respect to possible sources (the Babylonian-Aramaic-Arabic tradition as well as the eclectic mathematics of Demotic papyri). In general, the traces of the idea of a virtual breadth that turn up in the pseudo-Heronian corpus are linked to Greek concepts and practices; we may conclude that they reflect mental habits that were widespread even among Greek calculators.

Lengths as well as areas are already measured in feet in Plato's dialogues. It happens in the two passages, famous among historians of mathematics, where Plato refers to the *dýnamis* concept: *Theaetetus* 147D and *Politicus* 266B. The examination of the pseudo-Heronian texts suggest that the Platonic expressions δίπους, τρίπους and πεντέπους should not be interpreted as abstract measures, that is, as 2, 3 and 5 square feet, but rather that the areas spoken about are equal to strips of length 2, 3 and 5 feet (and breadth 1 foot).

Another Platonic passage, however, illustrates better the idea of standardized strips: *Laws* 819D–820B [trans. Bury 1926 105–109]. "The Athenian" speaks of "a certain kind of ignorance [..] naturally inherent in all men", more specifically among all Greeks (ordinary Greeks, that is – the context shows that mathematicians are not included) concerning the belief that lengths, breadths and depths⟦[9]⟧ "are commensurable with another"⟦[10]⟧. Afterwards this erroneous belief is specified in two points, namely

(1) "length is commensurable with length, breadth with breadth, and depth with depth",

and

(2) "as regards the relation of length and breadth to depth, or of breadth and length to each other – do not all we Greeks imagine that these are somehow commensurable with one another?"

---

[8] Heiberg does not follow the geometric process, and does not understand that in order to find the side ℓ one has to detract from the 30 exactly those 2 that remained when the half was taken away and "put above". He therefore sees this passage as evidence of failing understanding.

[9] ⟦Bury translates as "lines", "surfaces" and "solids", against the actual meanings of μῆκος, πλάτος and βάθος but anticipating what can be derived from the context, as done presently.⟧

[10] ⟦The Greek text says that they are "all measurable one by another" (πάντα μετρητὰ πρὸς ἄλληλ). The technical concept of commensurability is thus an illegitimate imposition on the part of Bury. Actually, according to the text it is supposed that the magnitudes can be measured by each other be either "absolutely" (σφόδρα) or "moderately" (ἠρέμα), which makes no sense if we think of the concept proper.⟧

The first error certainly refers to the discovery of irrational ratios, for example (Aristotle's paradigm) the ratio between the side and the diagonal of a square; it is not true that every line can measure any other line. The second is more obscure (cf. the discussion in [Mueller 1992: 94–95]). If length, breadth and depth are all understood as linear extensions, then the second error is no different from the first one. A different possibility is to understand "breadth" and "depth" as ellipses for "quantity which *also* possesses a breadth" (thus a surface) and "quantity which *also* possesses depth" (thus a solid, since this dimension is defined here and elsewhere by Plato as "the third").

In favour of this second interpretation speaks not only that it allows (ii) to be really another error than (i) but also the habitual shaping of the mathematical idiom. We encounter a similar ellipsis when the young Theaetetos defines *square* numbers (*Theaetetus* 147E–148A) as such numbers as can be generated as products of equal factors (but also, there is no need to explain it, of unequal factors) and as *oblong numbers* the others – those that can *only* be generated as products of different factors. The pattern is repeated when he speaks a little later of lines that are *only* measurable *dýnamei* (that is, when they are understood as parameters of a square) as *dýnameis* and of the others – those that *also* have a measure when they are understood directly as lengths – as *lengths.*

Closer to the substance of the presumed ellipsis of the *Laws* is the one that is contained in the definition of the line in the *Elements.* As Proclos explains in his commentary (96–97 [trans. Morrow 1970: 79]), there is no need to say "without breadth and without depth" since "everything that is without breadth is also without depth" – "denying breadth of [the line] he has also taken away depth".

None the less, Ian Mueller prefers (with a slight doubt) to stick to the first interpretation. The error to suppose that lengths, surfaces and solids are commensurable simply because they are all measured in feet seems to him to be so trivial that then one cannot speak of (ii) as an error.

However, as we have seen, the shared measure in "feet" is more than the simple absence of symbols allowing a distinction between "feet", "feet^2" and "feet^3". A whole mode of thought is involved, and if a length carries a virtual breadth (that can be actualized when needed), then it is no trivial error to assume that lengths and surfaces can be measured by each other – actually no error at all.

Yet it is an error from the point of view of those who had transformed geometry into a science and no mere technique. For those, as for Plato, it was not only an error but an error that had to be eradicated, as was to be eradicated the error that all lines are commensurable. We may remember the "projection" of the Old Babylonian school, which appears to have served the same purpose (note 5).

Conclusion

Let us return to the definition of the line. Neither Heron (the true Heron) nor Proclos seems to understand that "without breadth" is meant to bar the idea of the strip-line; the difficulty was probably already forgotten when Euclid wrote his *Elements.* But the definition was not invented by Euclid. It is quoted verbatim (as an already current definition) in Aristotle's *Topica* 143^{b}11 [ed. Tredennick & Forster 1960: 591]; possibly it was also the definition used by Plato's mathematician-friends, and maybe it was even older. What is interesting is Aristotle's argument: the definition presupposes that the genus (the lengths) is divided into two species, the lengths *provided with a breadth* and those *deprived of it.* Indeed, thus Aristotle, both species exist. This shows that (Platonic) ideas cannot exist: either the *idea of the length* would have breadth, or it would not; but in both cases, the existence of both species belonging to the same genus would be impossible.

We cannot be sure that the "lengths provided with a breadth" are the strips of practical mensuration; they might simply be surfaces. Nor is it excluded, however. It remains a possibility that the delimitation "without breadth" was originally meant as a demarcation excluding the strip-lines.

The passage from the *Laws* is more conclusive. Only the idea of strip-lines (and slice-surfaces) allows us to distinguish two errors that are genuinely different as well as substantial.

As regards the Old Babylonian mathematical texts, the failing understanding of the strip-line carries main responsibility for the erroneous interpretation of their "algebra" as a purely numerical technique. As we have seen, "error" (ii) has been so efficiently uprooted that Heiberg, when editing the pseudo-Heronian *Geometrica,* saw a blunder where the text presents us with a precise and correct description.

Any functioning system of mathematical thought contains an assortment of ambiguities. In Euclidean geometry, for instance, a square is *a figure* (a σχῆμα – *Elements* I, def. 25), thus something contained by one or more *limits* (ὅροι – def. 14). But a square may also be equal to one or more other figures (for instance, *Elements* II, prop. 14), in which respect it is thus reduced to its area. *Within* the discourse in question, these conceptual ambiguities normally remain hidden, regulated by the space of operations on which the concepts depend, and of which the terminology speaks: if a square is cut by a diagonal, *the figure* is obviously referred to; if it is five times another one, *the area* is necessarily meant.

This absence of pedantry contributes to the efficacy of thought. However, when mathematicians – be it mathematician-historians or the mathematician-teachers – believe that only the discourse of others and not their own contains ambiguities; when they do not understand that also the ambiguities of the others are virtual, not actual, since they are regulated by *their* operational space; when they thus forget that these *different ambiguities* are privileged keys for understanding the different thinking of the other –

then the absence of pedantry from their own discourse becomes pedantry and intolerance toward the other, and an obstacle for understanding.

Maybe discovering this intolerance in historical studies and the effort to overcome it may contribute to seeing it and overcoming it within the teaching of mathematics: a field which – the historian has to admit it – is much more important in this world than the pure history of the discipline.

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Chapter 9 (Article I.8) Concerning the Position of "Heron's Formula" in the *Metrica* (With a Platonic Note)

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"Sulla posizione della «formula di Erone»
nei *Metrica* (con una nota platonica)".
Bollettino di Storia delle Scienze Matematiche **17** (1997), 3–11

Additions to the translation in ⟦…⟧

J. Høyrup, *Selected Essays on Pre- and Early Modern Mathematical Practice*,
https://doi.org/10.1007/978-3-030-19258-7_9

Abstract

The article compares the way "Herons's formula" for the area of a triangle is presented in *Metrica* I.7–8, with context *Metrica* I.4–6, with the way it appears in *Dioptra* 30, and also with the way circular segments are dealt with in *Metrica* I.27–32. Both sequences in the *Metrica* present obvious stylistic incongruities, seemingly indicating that both "Heron's formula" and the Archimedean calculation in I.32 are interpolations – the former borrowed from *Dioptra* or a close source. At closer inspection, however, both insertions can be argued to have been made by Heron himself, within a treatise which is otherwise a re-elaboration of a technical manual. Only an initial numerical example in I.8 may have been inserted by a later editorial hand, together with the renowned "Heronian" method for approximating the square roots of non-square numbers

A final note suggests a possible link between, on one hand, Heron's replacement of μοῖραι ("lots" or "shares") by μονάδες (units) when he transfers the "formula" from the *Dioptra* to the *Metrica;* and, on the other, Plato's rejection of the splitting of unity into πολλὰ μόρια ("many parts") in vulgar computation.

Contents

In memoriam
OLAF SCHMIDT

The "formula" in the *Metrica* and in the *Dioptra*

"Heron's formula" for the area of a triangle is explained twice in the *Opera omnia:* in *Metrica* I.7–8, and in *Dioptra* 30 [ed. Schöne 1903: 16–24, 280–284]. In his times, Hultsch had doubts about the authenticity of the second version – doubts which, however, seem unfounded, as Schöne [1903: xix*f*] has shown. No deep analysis is needed, on the other hand, to discover that the former version has a distorted relation with the passages that surround it in the text.

Chapters 5 and 6 of *Metrica* I deal with scalene triangles, Chapter 5 with acute-angled, Chapter 6 with obtuse-angled triangles. They proceed by finding first the projection of a side on the base (using *Elements* II.13 and II.12, respectively), and next the corresponding height by means of the Pythagorean theorem. In the end of Chapter 6 (in a passage which we may designate 6A) it is stated that so far the results have been found by means of computation (ἐπιλογίζομαι), while in the following they will be obtained by means of analysis (κατὰ ἀνάλυσιν). After this first preamble to the formula follows in Chapter 7 the lemma that the "side" (the square root) of the product of the squares on the segments AB and BΓ equals the rectangle contained by these same segments – in symbols,

$$\sqrt{\square(\mathrm{AB})\cdot\square(\mathrm{B\Gamma})} = \sqsubset\!\sqsupset(\mathrm{AB,B\Gamma})$$ [1]

The proof makes use of the proportion technique, proving that AB:BΓ is equal to □(AB):⊏⊐(ABΓ) as well as to ⊏⊐(ABΓ):□(BΓ), and that ⊏⊐(ABΓ) is thus the mean proportional between □(AB) and □(BΓ). Therefore, ⊏⊐(ABΓ)·⊏⊐(ABΓ) = □(AB)·□(BΓ), whence

[1] The texts speaks of AB and BΓ as *numbers,* but much in the vocabulary remains geometrical, just as do of course the geometric entities that appear in the formula. It may be generally true that the Greek mathematicians distinguished precisely between numbers and segments, for which reason an arithmetico-algebraic reading of for instance Apollonios is illicit, as maintained with emphasis by Unguru & Rowe [1981]; even Heron knows that this distinction should be respected, which is probably the reason that he speaks of AB and BΓ as numbers, even though they *are* segments in the proof that follows. But he does not succeed, neither here nor elsewhere, to maintain the distinction. In III.xx it becomes particularly evident that he does not keep apart arithmetical and geometrical operations: in the calculation he omits a factor 1 (the "distance" between 4 and 5), for which reason he has to add magnitudes of different dimensions. The sharp distinction between geometry and arithmetic in Greek geometry is no consequence of failing understanding of the possibility to express arithmetical operations geometrically; it results from theoretical and philosophical purism ⟦cf. also article II.2⟧. In a context like that of the *Metrica,* this purism becomes too burdensome; Heron, as already the Babylonian scribes, sees the objects of his geometry in the "modern" way, as segments, areas and volumes that can be, and often already *are,* measured.

$$\sqsubset\!\sqsupset(\mathrm{AB\Gamma}) = \surd\{\square(\mathrm{AB})\cdot\square(\mathrm{B\Gamma})\}.$$

Chapter 8 begins with a second preamble, "There is, however, when the three sides are given, a general method to find the area of an arbitrary triangle without the perpendicular".[2] A numerical example follows, with sides 7, 8 and 9 units (and area thus $\surd 720$, the reduction of which with arbitrary precision is taught), and then a geometric proof, which begins with a third preamble:

> The geometric demonstration of this is the following: when the sides of a triangle are given, to find the area. It is certainly possible to find the area of the triangle if a perpendicular is drawn and its magnitude has been provided, but what is needed is to provide the area without the perpendicular.

Chapter 8 closes with another numerical example, this time with sides 13, 14 and 15 units – and the first sentence of Chapter 9 says that "after we have thus learned to find the area of a triangle with given sides when the perpendicular is rational", we shall find it in the case the perpendicular is irrational. It thus goes on from the precise point where Chapter 6 proper closes, as if neither the "first preamble" 6A, nor Chapters 7 and 8 had been there. The whole passage 6A+7+8 is thus in some way an interpolation. Beyond that, the internal structure of the passage, with three preambles, suggests the we are not confronted with a simple interpolation.

A confrontation with *Dioptra* 30 is informative. It begins like this:

> When the sides of a triangle are given, to find the area. It is certainly possible to find the area of the triangle if a perpendicular is drawn and its magnitude has been provided, but what is needed is to provide the area without the perpendicular.

Even in the Greek, the two texts are identical, letter for letter; but what is a third and thus rather misplaced preamble in the *Metrica* is a fully adequate explanation in the *Dioptra.*

The two proofs are also almost identical; compared to the version in the *Dioptra*, that of the *Metrica* adds a few explicative notes; when the order of letters is not alphabetic, it is often inverted (sometimes also when it *is* alphabetic in the *Dioptra*). There is thus no doubt that one of the texts is copied from the other, or that they follow a close shared model; the character of the differences indicates that the version in the *Dioptra* is, if not necessarily the original, at least much closer to the original than that of the *Metrica.* This observation fits the conclusion that follows from the introductory passage, a misplaced rudiment in the *Metrica* but functional in the *Dioptra.*

Dioptra 30 closes by a numerical example – the same as in the *Metrica.* Even here it is clear that the passage in the *Metrica* is copied with stylistic emendations, either from the *Dioptra* or from an original to which the *Dioptra* is very close. Among the emenda-

[2] As those that follow, this translation is mine – though made with an eye to Schöne's German.

tions, one is noteworthy: in the *Dioptra,* the sides are not 13, 14 and 15 *units* (μονάδες) but 13, 14 e 15 *lots* or *shares* (μοῖραι).

No less interesting than the confrontation with *Dioptra* 30 is one with *Metrica* I.27–29, 32. I.27 (which concerns the sum of numbers in continued proportion with ratio 4:1) is presented as a preliminary to the measurement of circular segments; I.28–29, which consider perpendiculars and triangles inscribed in circular segments, are characterized in the same way.

After these preliminaries, Chapter 30 begins as if they had not existed: "It is true that the ancients neglected the measurement of circular segments smaller than a semicircle", namely, calculating the area as $\frac{(c+f)}{2} \cdot f$ (c being the chord, f the arrow). The author suggests that the formula was due to those who took the perimeter to contain thrice the diameter, since the rule becomes exact for the semicircle if this is presupposed. Chapter 31 goes on, presenting the rule of those who instead had made "more precise investigations"[3] namely $\frac{(c+f)}{2} \cdot f + \frac{1}{14}(\frac{h}{2})^2$. This rule is ascribed to those who assumed the perimeter to be "the triple of the diameter, and the seventh part more" – once again because the two rules agree in the case of the semicircle.

In the end of Chapter 31 we find the observation ("31A") that this supposedly more precise rule should not be applied if $c > 3f$, and it is shown to lead to absurd results for segments that are too small. In such cases one must instead use "the following approach" – the one which is taught in Chapter 32.

There it is shown, on the basis of the preliminaries, that the area of a circular segment is larger than $1^1/_3$ times the area of the triangle with the same base and the same height (that is, as explained with a reference to Archimedes, larger than the corresponding parabolic segment). For circular segments whose chord exceeds thrice the arrow, this limit is recommended as an approximation to the area.

Chapter 33 deals with segments larger than a semicircle, and shows how to calculate the diameter and thus to find the complementary segment. As example it gives a segment with base 14 and arrow 14, and finds the arrow of the complement to be $3^1/_2$, that is, $^1/_4$ of the base. None the less, the area of the complement is found by means of the rule given in Chapter 31, as if neither the warning of 31A nor Chapter 32 had been there.[4]

Even this is thus somehow an interpolation – one may even think of a scholium which has been written on two consecutive pages, and therefore inserted into the text in two

[3] The contrast to which refers the particle μέν/"it is true that" in Chapter 30 is thus the one between "the ancients" and this group, not between the neglectful formula of the ancients and the Archimedean formula prepared in I.27–29.

[4] Admittedly, the error, which is c. 4% for the complement, is much less important relatively to the requested area, that of the major segment (c. 0.7%). This, however, is no consideration within the horizon of the *Metrica;* if it is not explained, we can be sure that the author of the passage has not thought about it.

different places. Anyhow, the presentation of Chapter 27 seems to show that the author of the preliminaries (and, in consequence, of 31A+32) saw them as part of the text; moreover, the reference to Archimedes is so similar to the references made in I.26 (where no interpolation is to be expected[5]) that the two passages appear to have been written by the same hand. Since this hand seems to be the most competent among those which have left their traces in the *Metrica,* it should be Heron's own hand.

The same hand must be responsible for the totality of the passages I.27–29,31A–32, since these do not exhibit the incongruities that characterize I.6A–8. The image what emerges is thus that of a Heron who, in *Metrica* I, uses previously written material without worrying too much about the global coherence of the text he produces – obviously material taken from the practical tradition, not from that of geometrical theory. As regards the area and the perimeter of the circle, Heron sees that the traditions are either erroneous ("$\pi = 3$") or equivalent to what he derives himself from Archimedes ("the triple of the diameter, and the seventh part more").[6]

When dealing with circular segments, Heron does not want to throw away the traditional rules – perhaps because they offer him an occasion to display his own perspicacity in the discussion of their origin, but also because what he offers in Chapter 32 is a supplement and cannot be an alternative – for a semi-circle, even the rule of "the ancients" is much more precise than the parabolic approximation.

In the case of triangles, the traditional material is fully valid; there is thus no reason to discard it, also because it is much more intuitive than the formula explained in Chapter 8.[7] This formula is therefore inserted after I.5–6.

Is seems, however, that Heron has not inserted the complete passage I.6A–8. If I.27–29,31A–32 can serve as a model, then it is probable that the original interpolation consisted of 6A, the demonstration borrowed from the *Dioptra* (or an immediate source for this work) without stylistic smoothing, together with the lemma I.7, whose contents may be intuitive and thus acceptable without proof within the context of the *Dioptra,* but which is difficult to defend within the metamathematical framework of theoretical geometry – a framework which Heron takes care to respect in the *Metrica.* The first numerical example may be a secondary interpolation, introduced by an editor as an

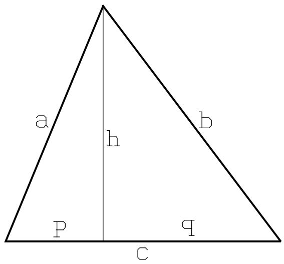
[5] I.26 contains formulae for the area and the perimeter of the circle, and only formulae derived from Archimedes's works.

[6] That the practical traditions had already adopted Archimedes's results well before Heron's time is pointed out by Heron himself in *Metrica* I.31 through his reference to those anonymes who assumed the perimeter to be "the triple of the diameter, and the seventh part more"; the tangle of practical rules derived from that constitutes Chapter 17 of the conglomerate which Heiberg [1912] put together as *Geometrica.*

[7] For the characterization of the material of I.5–6 as "traditional", see presently.

introductory appetizer to a proof whose complexity many students will no doubt have found repulsive.

This hypothesis has the interesting consequence that the anonymous editor and not Heron will be responsible for explaining the method to approximate irrational roots; this explanation is indeed linked to the first numerical example, chosen without the usual care to produce integer or at least rational solutions. ⟦Admittedly, *Metrica* I.18 approximates √5 as $2^1/_4$, which would follow from application of the method taught in I.8. Firstly, however, this approximation was known since ages, though probably not argued in the same way; secondly, I.18 not only gives no cross-reference – instead, it chooses a different way, replacing 5 by the close approximation $^{81}/_{16}$, a square number.⟧



That it is legitimate to use I.27–29,31A–32 as a model is suggested by an analysis of the passage I.4–6. As stated above, I.5–6 finds the area of right- and obtuse-angled triangles through determination of the projection of a side on the base (or its extension), effectuated by means of *Elements* II.13 and II.12 – in the acute-angled case,

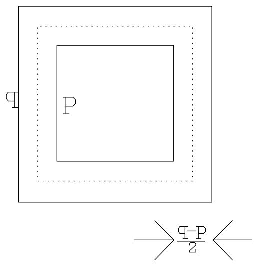
$$q = \frac{b^2+c^2-a^2}{2} \div c$$

(see the figure). Analogous calculations turn up in many medieval (primarily Arabic) treatises on practical geometry – see [Høyrup 1996]. In these, the internal projections are almost always determined by means of formulae equivalent to but different from *Elements* II.13,

$$q = \frac{c}{2} + \frac{b^2-a^2}{2} \div c\,, \quad p = \frac{c}{2} - \frac{b^2-a^2}{2} \div c\,.$$

The argument behind these formulae seems to belong to the genre of “geometric algebra”. Initially it is seen that

$$\square(b)-\square(a) = \{\square(q)+\square(h)\}-\{\square(p)+\square(h)\} = \square(q)-\square(p)$$



However, as explained in ibn Thabāt’s treatise [ed. Rebstock 1993: 119] (cf. *Metrica* I.26), $\square(q)$–$\square(p)$ is also the border contained between the two squares, the mean “length” of which is $\frac{4p+4q}{2}$ $(=2c)$ and whose width is $\frac{q-p}{2}$; in consequence,

$$\square(q)-\square(p) = \sqsubset\!\sqsupset\left(\frac{q-p}{2}, 2(q+p)\right) = \sqsubset\!\sqsupset\left(\frac{q-p}{2}, 2c\right)$$

The medieval treatises often offer the “Euclidean” formula as an alternative.[8]

When finding external heights in an obtuse-angled triangle, in contrast, all treatises use the formula derived from *Elements* II.12. The only coherent explanation for this odd distribution seems to be that the practical tradition had already invented the “geometrico–algebraic” way to determine the height of a scalene triangle before the intervention of the Greek theoreticians, but that it had used it only for the determination of internal heights (a very reasonable choice if one wants to minimize the influence of measuring errors). The Greek theoreticians generalized the notion of a height, to encompass the perpendicular from an apex on the opposite side *or its extension,* and also reformulated the already known result about internal heights so as to connect it to the Pythagorean theorem (without changing the essence of the proof). Subsequently, the practical tradition adopted the new invention of the theoreticians, that is, the determination of the external projection (as it adopted the Archimedean approximation to π).

On this background, *Metrica* I.4–6 become interesting. Chapters 5 and 6 find the internal and external projections of a side on the base, and derive from them the heights and the areas of the triangles. Both calculations, however, use the Euclidean formulae. Even though we do not know at which moment the Euclidean formula for the external projection was adopted by the practical tradition, the use of the Euclidean formula for the internal projection leaves little doubt that Heron adjusted the details of the treatise of practical geometry which served him as inspiration.

Yet he did not adjust its structure; this is the reason that I.6A–8 and I.27–29,31A–32 look like interpolations (and in a sense *are* interpolations). But even before I.5–6 he introduces a preliminary. Indeed, I.4 teaches how to distinguish whether the perpendicular falls inside or outside the triangle. It presupposes as already known that if the angle A is acute, then $\square(\mathrm{B}\Gamma) < \square(\mathrm{BA})+\square(\mathrm{A}\Gamma)$; if it is right, then $\square(\mathrm{B}\Gamma) = \square(\mathrm{BA})+\square(\mathrm{A}\Gamma)$; and if it is obtuse, then $\square(\mathrm{B}\Gamma) > \square(\mathrm{BA})+\square(\mathrm{A}\Gamma)$; in order to show that the implications can be inverted, he uses a double reduction to the absurd. This is certainly done by a theoretically educated author; it is out of the question that the practical tradition should care about such logical subtleties, and also that it should use this kind of argument. Already the coincidence of the structure of the whole passage I.4–6 with what we have seen in connection with the interpolations makes it reasonable to assume that the author of all three passages is the same – that is, Heron.

We may thus add some shades to the picture of Heron as author of *Metrica* I: He uses a model, an existing manual of practical geometry, and he follows it chapter for chapter.[9] When he finds it suitable, however, he replaces contents which is unsatisfactory

[8] Certain treatises even give a second alternative, which presupposes *Elements* II.6 or an equivalent idea. However, this intricacy does not change the general conclusions that can be derived from the distribution of the “Euclidean” and the “geometrico-algebraic” formulae.

[9] It is possible, and even plausible, that he has changed the order of the general themes – see

from a theoretical point of view (or simply with regard to the habits of theoretical geometry) by something which is more acceptable (for instance, in I.6, the formulae for the area and the perimeter of the circle, and in I.5, the internal projection); he also inserts theoretical and metatheoretical preliminaries (for instance, I.4).

Within this basic structure he also inserts results that have no counterpart in the model (I.6A,8, I.26A,32); even these he provides with preliminary lemmas if it seems adequate for didactical or metatheoretical reasons (I.7, I.27–29). But he does so without worrying about the global coherence of the text he produces. This creates the singular phenomenon of "author's interpolations", made by the author in person but none the less interpolations with regard to the coherence of the text.

We know nothing about Heron as a person, except that he worked in Alexandria around 62 CE.⟦[10]⟧ We thus know nothing about the production of the *Metrica*, nor about their intended or actual use by the author. It is quite possible that he made a first version without the interpolations, and inserted them when returning to the text at a later moment. It is indeed difficult to explain without this presupposition the words of Chapters 9 and 33, which refer to that which precedes the interpolations as if it was immediately preceding. In this case the interpolations become interpolations even in the usual sense, and not only with respect to the overall logic of the text.

A Platonic Note

In Plato's *Republic* there is a passage which is rather famous in discussions about Plato's view of mathematics (525D–526A) [ed. Shorey 1930: 162–164]. Here, Socrates explains the importance of understanding the nature of the unity (τὸ ἕν) and the role of arithmetic in conveying this insight. The contrast to this doctrine of "numbers in themselves", a doctrine which "directs the soul upward", is presented by vulgar computation, which cuts up (κερματίζω) the unit in many parts (πολλὰ μόρια).

Τὸ ἕν is *not* the μονάς of Heron (nor that of Euclid), and Plato's μόρια are not μοῖραι. It is not to be excluded that Plato thinks of the use of fractions and of the subdivision of metrological and monetary units. But Heron's correction of his own text when he transfers it from a "mechanical" treatise – the *Dioptra* – to one with mathematical pretensions, changing μοῖραι into μονάδες, suggests that Plato might instead (or also) have thought of something else: the use of shares in the vulgar computation of profits and costs, hopelessly embroiled in that "arithmetical justice" which the ancient philosophers always rejected. Heron at least seems to suspect as much.

[Høyrup 1996]. For instance, manuals of practical geometry normally deal with trapezia and irregular quadrangles before triangles; Heron, with his Euclidean upbringing, gives priority to the triangles.

10 ⟦Even this date has now been subjected to some doubt, see "Heron of Alexandria", pp. 283*f* *in New Dictionary of Scientific Biography* III. Detroit: Scribner, 2008.⟧

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Chapter 10 (Article I.9) Heron, Ps-Heron, and Near Eastern Practical Geometry: An Investigation of *Metrica, Geometrica,* and other Treatises

Originally published in
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Trier: Wissenschaftlicher Verlag Trier, 1997.

Corrections of style (not least regarding the reference system) made tacitly
A few additions touching the substance in ⟦…⟧
Translations, if not otherwise identified, are mine

J. Høyrup, *Selected Essays on Pre- and Early Modern Mathematical Practice*,
https://doi.org/10.1007/978-3-030-19258-7_10

Abstract

See "Introduction"

Contents

In memory of Alistair Crombie
whose *Augustine to Galileo* first
introduced me to medieval science

Introduction

In the following I intend to show, firstly, that Heron's geometry depends to a greater extent than usually assumed on Near Eastern practical geometry or its descendant traditions in the classical world, and that the conventional image (also suggested by Heron himself in the initial passage of *Dioptra*[1]) as the transformer of theoretical into applied mathematics is only a half-truth; secondly, that much of what is shared by Heron's *Metrica* and the pseudo-Heronic collections assembled by Heiberg as *Geometrica* are shared borrowings from the same tradition, and that it is misguided to speak of *Geometrica* as "essentially" another version of the *Metrica* I.[2] Concomitantly I shall argue following Heiberg that it is already misguided to speak of *Geometrica* as one treatise unless this be understood explicitly as Heiberg's construction.

A Near Eastern practitioners' tradition

It is of course essential for my argument that there *was* such a practitioners' tradition; that Heron and the compilers of the constituent manuscripts of *Geometrica* had access to it; and that we can find its representatives and show that their material is not borrowed from Heron.

This is a project of its own, which I have dealt with in other connections;[3] here I shall only point to a few sources that prove the existence of a long-lived tradition not dependent on the literate level of Greek geometry, going back at least to c. 2000 BCE and still influential in the Islamic Middle Ages:

The first source to be quoted is an Old Babylonian[4] quasi-algebraic[5] problem,

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1 Ed. [Schöne 1903: 188].

2 [Mahoney 1972: 315].

3 For instance in [Høyrup 1996] ⟦and in much greater depth in article I.3⟧.

4 The Old Babylonian period goes from 2000 BCE to 1600 BCE; the extant mathematical texts belong to its second half. The present text may be from the 18th century BCE, i.e., Hammurabi's times.

5 "Algebraic" because the method is analytical: the unknown is treated as if it were a normal identifiable quantity, and then extricated from the complex relationship in which it is originally involved; "quasi" because the method is not arithmetical as in Modern algebra but a "naive" cut-and-paste geometry, where the correctness of the steps is immediately seen but not argued. The present problem, moreover, operates directly on the unknown quantities themselves; the Old Babylonian

deliberately held in archaizing non-school formulations:[6]

> [If somebody asks you thus] about a surface: the four fronts and the surface I have accumulated, 41′40″.
> 4, the four fronts, you inscribe. The reciprocal of 4 is 15′.
> 15′ to 41′40″ you raise: 10′25″ you inscribe.
> 1, the projection, you append: 1°10′25″ makes 1°5′ equalside.
> 1, the projection, which you have appended, you tear out: 5′ to two
> you repeat: 10′ n i n d a n confronts itself.

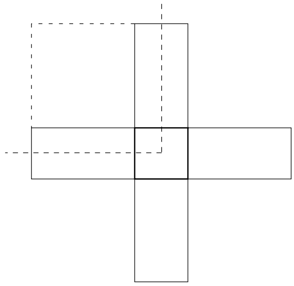

Figure 1. The procedure of BM 13901 N° 23.

Numbers translate the Babylonian sexagesimal place value system according to Thureau-Dangin's system, where ′, ″ etc. render decreasing and ‵, ‶ etc. increasing sexagesimal order of magnitude, and where ° is used when necessary to render "order zero" (15′ thus stands for $^{15}/_{60}$ and 1°5′ for $1+^{5}/_{60}$). Figure 1 shows what goes on: The central square represents the area, while the four "wings" that "project" from the central square – rectangles whose width coincides with the "front" or side of the square, and whose length is 1 – represent the four fronts. One fourth of this configuration is a gnomon with area 10′25″, which is completed as a square when the "projection 1" (representing the square, as the Babylonian *mitḫartum* and the Greek δύναμις represent the square configuration of which they are the defining side[7]. The resulting area 1°10′25″ has the "equalside" or squaring side (corresponding to the Greek πλευρὰ τετραγωνική) 1°5′, from which the projection is removed, leaving 5′ as half the front; 5′, when "repeated to two", gives 10′ n i n d a n as the front (the n i n d a n or "rod" of c. 6 m is the basic length unit).

In the *Liber mensurationum* – written in Arabic by an otherwise unidentified Abū Bakr, probably from around 800 CE and translated into Latin shortly after 1150 by Gerard of Cremona[8] – the following problem is found:

> And if somebody has said to you: Concerning a square, I have aggregated its four sides and its area, and what resulted was 140, then how much is each side?

school, however, also used the technique to solve non-geometric problems, using its line segments to *represent* areas, prices and numbers, as medieval and later applied algebra makes numbers represent geometric or physical magnitudes, prices, etc.

[6] BM 13901 N° 23, first published in [Thureau-Dangin 1936]. My translation differs radically from those of François Thureau-Dangin and Otto Neugebauer; the need for this revised translation is explained, e.g., in [Høyrup 1990a] – as regards the present text, see 271*f*.

[7] This parallel (and possible calque) is the topic of [Høyrup 1990b].

[8] Critical edition in [Busard 1968]. Discussion, including tentative dating in [Høyrup 1986].

> The working in this will be that you halve the sides which will be two, thus multiply this by itself and 4 result, which you add to 140 and what results will be 144, whose root you take which is 12, from which you subtract the half of 4, what thus remains is the side which is 10.

Even here, a geometric procedure is involved; since it is described quite clearly in a problem from *Geometrica* to be quoted presently, I shall omit analysis.

In the present millennium, the problem turns up in Savasorda's early 12th-century *Liber embadorum:*[9]

> If, in some square, when its surface added to its four sides, you find 77, how many cubits are contained in the surface? Taking the half of its sides, which is two, and multiplying it with itself, you find 4. If you add this to the given quantity, you will have 81, whose root (which is 9) you take; and when you subtract from this the half of the addition that was mentioned already, 7 remain. This is the side of the square in question, whose surface contains 49.

Further, in Leonardo Fibonacci's *Pratica geometrie* from 1220:[10]

> And if the surface and the four sides [of a square] make 140, and you want to separate the sides from the surface. …,

in Piero della Francesca's *Trattato d'abaco:*[11]

> And there is a square whose surface, joined to its four sides, makes 140. I ask what is its side. …,

and finally in Luca Pacioli's *Summa de arithmetica:*[12]

> And if the 4 sides of a square with the area of the said square are 140. And you want to know how much is the side of the said square. …

As we see, there is some variation. Savasorda's side is 7, but the others have 10, as the Old Babylonian problem (there transposed into the order of minutes, as mostly in Old Babylonian quasi-algebraic texts); Fibonacci (followed by Piero) normalizes the order of the members, but Pacioli corrects Fibonacci (whom he follows closely on other accounts) and reintroduces the original unusual order; Fibonacci thus cannot be his only source (and other corrections show that he is not just using Gerard's version of the *Liber mensurationum*).

It should be obvious that some sort of continuous tradition is involved. It has also left its footprint in a treatise which Heiberg included in his *Geometrica* (cf. Figure 2):[13]

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9 II.12, ed. [Curtze 1902: I, 39].

10 Ed. [Boncompagni 1862: 59].

11 Ed. [Arrighi 1970: 122].

12 [Pacioli 1523: fol. 15^{r}].

13 *Geometrica* 24.3, ed. [Heiberg 1912: 418]. Heiberg's translation and notes to the problem are

> A square surface having the area together with the perimeter of 896 feet. To get separated the area and the perimeter. I do like this: In general [i.e., independently of the parameter 896 – JH], place outside (εκτίθημι) the 4 units, whose half becomes 2 feet. Putting this on top of itself becomes 4. Putting together just this with the 896 becomes 900, whose squaring side becomes 30 feet. I have taken away underneath (ὑφαιρέω) the half, 2 feet are left. The remainder becomes 28 feet. So the area is 784 feet, and let the perimeter be 112 feet. Putting together just all this becomes 896 feet. Let the area with the perimeter be that much, 896 feet.

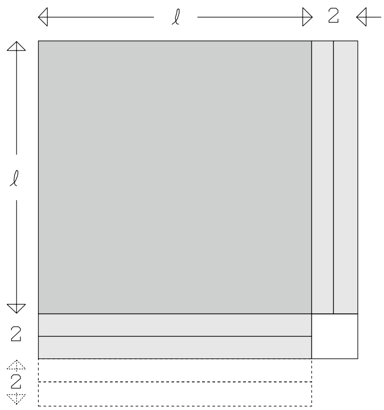

Figure 2. The procedure described in *Geometrica* 24.3.

"In general"/καθολικῶς corresponds to *semper,* oft-repeated in Abū Bakr's treatise, and indicates that the step undertaken does not depend on the number 896 (but certainly on the fact that precisely one area and one perimeter are involved). "To get separated"/διαχωρίσαι corresponds to *berûm* is Old Babylonian texts of similar riddle character (adding, e.g., workers, days, and the bricks produced), and to Leonardo's *separare.*

These problems, of course, are not "practical". They are riddles, elements of that cultural superstructure by which the craft of practical geometers upheld its professional identity – problems that allowed the practitioner to prove himself "a keen and swift enquirer", in Savasorda's words.[14] Such riddles are found wherever a community of practical reckoners exists, always connected to the kind of reckoning in which they are engaged but always (which is what allows us to speak of a "cultural" superstructure) somehow oblique in their relation to genuine practical problems. In 18th-century naval officers' notebooks, they deal with navigation problems that could never present themselves in real life;[15] 19th-century engineers, well trained in technical drawing and descriptive geometry, constituted an eager public for the triangle geometry of *Journal des mathématiques élémentaires.*[16] Al-Khwārizmī characterized such problems as "brilliant" (*latīf*), and the Arabic tradition in general referred to them as "rarities" (*nawādir*). In the perspective of the modern mathematical institution they have become "recreational".

misleading, imputing his own faulty understanding on the ancient copyist.

14 *Liber embadorum* II.7, ed. [Curtze 1902: I, 32].

15 Exemplified in [Meyer & van Maanen 1991].

16 Eduardo L. Ortiz, personal communication.

"The four sides and the area" is one of several quasi-algebraic riddles circulating among non-scholastic practical geometers in the earliest second millennium BCE.[17] The Old Babylonian school was inspired by them and developed them into a whole discipline of "naive-geometric algebra" with freely varied coefficients, using lines and areas to *represent* entities of other kinds – for example prices or pure numbers. The practitioners' riddles, on the other hand, were always "natural", referring to the single area, *the side* or *all four sides* of a square, etc. Another riddle of the kind asks to find the diameter, the perimeter and the area of a circle when their sum is given – relevant in the present connection because it is also found in several of the manuscript traditions aggregated in *Geometrica,* and because it is often cited as evidence that (and how) Heron dealt with mixed determinate second-degree equations.

Geometrica

That this use of the text is unwarranted follows already if we recall what Heiberg wrote about *Geometrica* and the two manuscript groups:[18]

(1) *Geometrica* "was not made by Heron, nor can a Heronic work be reconstructed by removing a larger or smaller number of interpolations" (p. xxi).
(2) Mss AC represent a book which, with additions, changes and omissions, only reached the present shape in Byzantine times; it was not meant to serve field mensuration directly but was for use "in [commercial and legal] life" and in general education (p. xxi).
(3) Manuscript S, with the closely related ms V, was intended to serve youth studying "architecture, mechanics and field mensuration" in the "University of Constantinople" and thus "more familiar with theoretical mathematics" – a use which in Heiberg's view agrees with the presence of Heron's (more or less) genuine *Metrica* in the same manuscript (p. xxiii).
(4) Both versions of the work merge (each in its own way) "various problem collections together with Heronic and Euclidean excerpts" (p. xxiv).

One may wonder whether it was really a sound editorial method to fuse the two "versions" into one. Since Heiberg took over the task after the departed Wilhelm Schmidt, the choice may not have been his. As things are, only very careful observation or reading of the Latin preface to *volume V* of the *Opera omnia* will reveal that a work contained in *volume IV* is a modern conglomerate of two (indeed more) ancient conglomerates. But Heiberg knew, and he *does* tell. He also seems to have known, but does not say it too directly, that the origin of the "problem collections" was neither Heronic nor Euclidean.[19]

17 Since the culture of this practitioners' environment was oral or semi-oral, the evidence for the existence of these riddles is indirect though fairly rich, as discussed in [Høyrup 1996] ⟦and in particular in article I.3⟧.

18 [Heiberg 1914: xxi].

19 Heiberg may not have noticed that his chapters 22 and 24, found in ms S but there squeezed

Levels of comparison

Comparison between ancient mathematical texts (in the present case, texts representing the practitioners' tradition with the Heronic and pseudo-Heronic texts) can be made on at least five levels:

(1) The overall organization of the material, reflecting the writer's sense of "natural progression".
(2) The methods seen from the stance of modern mathematics, supposed to reveal the *mathematics* involved.
(3) The procedures in their actual detail – we are in a world where multiplying an area by 11 and then taking the 14th part was "another way", ἄλλως, with regard to the subtraction of $^1/_7$ and $^1/_{14}$ from the area (*Geometrica* 17.15, mss AC).
(4) The numerical values involved – as we have seen, the solution to the "four sides and the area" remained 10 for 3200 years; that successive doublings are exactly 30 in number was the compulsory standard from 1800 BCE until the earlier Middle Ages, when 64 arose as an alternative possibility.
(5) The phraseology; even on this point, indeed, the conservatism of our tradition is amazing to a modern mind.

The organization

Let us first look at the global organization of the subject-matter.

For two reasons, the cuneiform record tells us little about the standard progression of the early practitioners: firstly, what we find in the clay tablets has mostly already gone through a process of scholastic normalization; secondly, those texts that are so extensive that they *might* tell a progression through themes are instead either rather disorganized "anthology texts" or organized around a particular theme. The only exceptions – the so-called Tell Ḥarmal compendium[20] and the text BM 80209,[21] both belonging to text groups close to the practitioners' tradition – tell us that squares precede rectangles, which on their part precede circles.

The Arabic Middle Ages, on the other hand, present us with a whole array of treatises still close to the tradition – thus Abū Bakr's *Liber mensurationum,* al-Karajī's *Essentials of reckoning*,[22] and ibn Thabāt's *Reckoner's Wealth* (c. 1200 CE).[23] Al-Khwārizmī's

in between *Stereometrica* and the *Metrica*, are independent treatises. Cf. note 29.

20 [Goetze 1951].

21 Ed. [Friberg 1981].

22 *Kāfī fī'l ḥisāb,* ed. trans. [Hochheim 1878].

23 Ed. trans. [Rebstock 1993].

chapter on geometry in the *Algebra,*[24] Savasorda's *Liber embadorum,* and Leonardo Fibonacci's *Pratica geometrie* are *a priori* suspicious because of the explicit orientation of their authors (Euclidean in the latter two cases, idiosyncratically al-Khwārizmī's own in the former) – but as we shall see, their stylistic efforts are not always reflected in what they bring forth. The treatises in question arrange the material on plane geometry as follows:[25]

Abū Bakr, *Liber mensurationum*

Square; rectangle; rhomb; trapezia (isosceles / acute-angled / right / obtuse-angled).
Triangles (equilateral / isosceles / acute-angled / right / obtuse-angled).
Circle; semicircle; circular segment (> semicircle); circular segment (< semicircle).

Al-Karajī, *Kāfī fī'l-ḥisāb* ("Essentials of Reckoning")

Square and rectangle; rhomb; parallelogram; trapezia.)
Triangles (right / equilateral / isosceles, acute-angled / scalene, acute-angled / isosceles / scalene, obtuse-angled).
Circle; segments (semicircle / major /minor).
Regular n-gons (ex. $n = 6$)

Ibn Thabāt, *Ghunyah al-Ḥussāb* ("Reckoner's Wealth")

Square and rectangle; rhomb; parallelogram; trapezia (right / isosceles / scalene, acute-angled); other quadrangles (with "surveyors' formula"); "staircase-" and "drum-form" figures.
Regular n-gons, n>4 (ex. $n = 6$).
Acute-angled triangles (equilateral / isosceles / scalene); right triangles (isosceles / scalene); obtuse-angled triangles (isosceles / scalene) – *rūmī*-method (= "Heron's formula").
Circle (incl. diameter+perimeter+area-"algebra"); circular sector; segments (semi-circle / major / minor) – "egg-shaped" (doubled minor segment).
Figure within figure, including concentric squares.

Al-Khwārizmī, *Algebra* (chapter on geometry):

Square; equilateral triangle; rhomb; circle; circular segment; volumes. Pythagorean theorem.
Square; rectangle; rhomb; parallelogram; irregular quadrangle.
Triangles (right; acute-angled; obtuse-angled).
Circle.
Frustum; square inscribed in isosceles triangle.

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24 Ed. trans. [Gandz 1932].

25 These are rough outlines, and make some of the works look more orderly than they are – in particular those that have gone through the Byzantine mill.

Savasorda, ***Liber embadorum***:

Square; rectangle; rhomb; "quasi-algebra" on squares and rectangles.
Triangles (equilateral / isosceles / acute-angled / right / obtuse-angled); parallelogram; trapezia (isosceles / acute-angled / right / obtuse-angled); irregular quadrangles.
Circle; semicircle; segments (minor / major); ellipse; chord table.
Regular polygons; fields on mountain slopes.

Leonardo Fibonacci, *Pratica Geometrie:*

Square; rectangle.
Triangles (equilateral; isosceles; acute-angled; obtuse-angled).
Square; rectangle; rhomb; parallelogram; trapezia (right / isosceles / acute-angled / obtuse-angled); irregular quadrangles; polygons, primarily regular.
Circle; semicircle; chords with table; sectors, segments, "egg-" and other mixed shapes; fields on mountain slopes.

It is natural that any treatise on mensuration begins with the square – more precisely with the unit square, which provides the area unit; going on with rectangles is almost as natural, since rectangles provide the basis for area computation. After that point, a mind trained in the Euclidean tradition will tend to go on with triangles, from there to quadrangles of increasing irregularity, etc. There would be no reason to treat the semicircle as a particular configuration, but the use of approximations may ask for separate treatment of minor and major circular segments. This sequence corresponds to what we see in the beginning of al-Khwārizmī's and Fibonacci's treatises (italicized above; Fibonacci goes no further than the triangle). Both, however, have a second start from the square, and then follow a sequence that *grosso modo* coincides with what we find in Abū Bakr, al-Karajī and ibn Thabāt, representing the "normal progression" of themes as seen by the practitioners:

After squares and rectangles (both with quasi-algebraic problems) follow rhombs, at times treated at great length with trivial variations of the quasi-algebraic problems on rectangles. Parallelograms may come next, but much more important is the treatment of trapezia. Then irregular quadrangles may follow, either split into triangles or treated by means of the "surveyors' formula", average length times average width (always yielding a result which is too great, except for rectangles where it is trivially correct; avoided by the "mathematicians" but found with ibn Thabāt, and mentioned by other Arabic authors as a practitioners' formula). If included, regular polygons may follow next.

Only then come the various triangles, followed by circle, semicircle, segments (Abū Bakr, al-Karajī and ibn Thabāt treat the major segment first, all others have it last). If not dealt with after quadrangles, regular polygons may come after circles and segments.

A corresponding analysis of the *Metrica* gives the following result:

Unit square; rectangles.

Triangles (right / isosceles / acute-angled / obtuse-angled do. / [“Heron’s formula”] / with irrational height)

Trapezia (right / isosceles / acute-angled / obtuse-angled) / irregular quadrangles.

- regular *n*-gons, 2<*n*<13.
- circle; two concentric circles; segments (minor / major).
- ellipse; parabola.
- cylindrical surface; conic surface; spherical surface; segment of this.

The order of *Geometrica*/AC is as follows:[26]

Metrology; square; rectangle.

Triangles (right / isosceles / acute-angled / obtuse-angled / “Heron’s formula” / obtuse-angled, continued / isosceles with inscribed square).

Rhombs; rectangle; various triangles; parallelograms; trapezia (right / isosceles / acute-angled / obtuse-angled); irregular quadrangles (“Heron’s formula”, and triangulation).

Circle (incl. 1.-degree “algebra”); semicircle; segments (minor / major); 2 concentric circles; circles from “another book” (including diameter+perimeter+area-“algebra”).

Regular *n*-gons, 4<*n*<13; irregular polygons; …

The *Geometrica*-part of ms S exhibits this structure:[27]

Metrology; square; rectangle; irregular quadrangle (surveyors’ formula).

Triangles (right / isosceles); circles inscribed in and circumscribed around isosceles and scalene (13-14-15) triangle.

Circle; semicircle;[28] segments (major / minor / major by subtraction / minor).

Then follows *Stereometrica,* after which come two separate treatises that Heiberg has inserted in *Geometrica.*[29]

26 This is the order of Heiberg’s edition, which follows ms A apart from an inversion of 15.17–19 (first in the ms) and 15.15–16 (all five sections deal with parallelograms, but the wording suggest that section 15 and not section 17 be first). I omit the various metamathematical discussions contained in the work (the origin of geometry, etc.), as irrelevant for the present purpose.

27 On the whole, ms V depends so strongly on ms S that S can be taken to represent the better version.

28 After the semicircle follows the volume of a semicircular wall, in practice only a calculation of its base, moved by Heiberg to *Stereometrica.*

29 Probably because he had no other work in which to put them; they are not coherently in mss A+C (isolated pieces are, e.g. the problem on circular diameter+perimeter+area, though in words that do not suggest use of precisely this treatise, cf. below); they should certainly not have been merged with neither the modern nor the ancient conglomerates. In references to *Geometrica*/S below they are hence not included. Instead, I shall refer to the treatise which Heiberg inserted as chapter

The first (inserted as 24.1–51; henceforth thus S:24) contains, among other things, determinate and indeterminate problems about geometrical configurations, but also a section on circles inscribed in and circumscribed around triangles, which were already dealt with after triangles. Here we find the problem on the square diameter+perimeter, and that of the circular area+diameter+perimeter.

The other (22.1–24) contains, after a section on metrology, the areas of regular *n*-gons, $2<n<13$, followed by circle and semicircle, in abstract formulation (everything else in the *Metrica* and either *Geometrica* is set forth through numerical examples).

For comparison, the section on mensuration from Columella's *De re rustica* V (a close contemporary of the *Metrica*) may be of interest:[30]

Metrology; square; rectangle; trapezium.
Triangles (equilateral / right).
Circle; semicircle; minor segment; regular hexagon.

Heron obviously deviates most clearly from the established pattern; he makes triangles follow directly upon the rectangle; he omits the semicircle (obviously considering it trivial), and replaces the "egg-shape" (if it was indeed part of the heritage, which Old Babylonian texts suggest) by a "scientific" figure – the ellipse.

Mss AC is not far from the Heronic pattern; but the semicircle appears as a configuration of its own. Ms S, on the other hand, shows closer affinities with the practitioners' tradition, as revealed both by the presence of the "surveyors' formula" and the place where it occurs. In all its brevity, Columella's treatment of the subject is also close to the traditional and well away from the scientific and Heronic order; it shows that the characteristic order of the medieval handbooks was not the result of a later transformation of a tradition derived from Heron.

Methods, considered abstractly

On this level, one feature is usually referred to as indubitably Heronic in *Geometrica* as well as *De re rustica:* The use of the Archimedean value for π. Strangely enough once again, since Heron tell us that the evidence is not valid. Two passages in the *Metrica* are decisive:

(1) I.xxx explains that "the ancients" – οἱ ἀρχαῖοι – measured the minor circular segment as $[½(c+h)]\cdot h$, where c is the chord and h the height. As Heron argues, these "ancients" seem to have "followed those who took the perimeter to encompass the

24 as S:24, and to the other as S:22.

According to the text edition, S:22.3–24 is also contained in ms A; however, as can be seen from the manuscript description of the preface (p. xi), it is not part of its *Geometrica*-text.

[30] Ed. trans. [Richter 1981].

triple of the diameter"; if $\pi = 3$, indeed, the formula is exact for the semicircle. The argument implies that Heron had no direct information about the provenience of the formula, but that he knew about the use of $\pi = 3$ (the traditional Babylonian value, reappearing in the Ancient Testament and hence probably current in the Syrian Near East, and also reappearing in Demotic mathematics).

(2) I.xxxi tells that "those who made more precise investigations" add $^1/_{14}(^c/_2)^2$, and argues with a similar argument that they must have followed the other course according to which the perimeter is the triple diameter and in addition $^1/_7$ of the diameter. Even here, we see, the use of the Archimedean value (transformed from the ratio 22:7 into the number $3+^1/_7$) was already established practice, and had been so for so long that Heron does not find its traces; it had also generated an approximate formula which is even worse for narrow segments, in all probability found via fitting by practitioners with neither theoretical mathematical training nor propensity.[31] The formula is *post-Archimedean,* but certainly not Archimedean.

The whole organization of the discussion of segments is clear evidence that Heron worked on the basis of an earlier treatise which he emended but did not rewrite consistently ⟦cf. article I.8⟧: The text includes the theorem that a segment is greater than $^1/_3$ times the triangle with the same base and the same height, and suggest that this limit – the exact area of the corresponding parabolic segment, as the text tells with due reference to Archimedes – be used to approximate the area of segments whose chord is more than three times the height. A lemma and the first two theorems leading to this result open the whole treatment of circular segments (I.xxvii–xxix). Then the two received approximation formulae are given in I.xxx and I.xxxi; in the end of I.xxxi comes the demonstration that the formula used by "those who made more precise investigations" yields absurd results when the base is more than thrice the height. Quite appropriately, the rest of the proof begun in I.xxvii–xxix is given in I.xxxii – after which I.xxxiii goes on as if neither I.xxxii nor the final warning of I.xxxi had existed, and applies the formula $A = [^1/_2(c+h)]\cdot h+^1/_{14}(^c/_2)^2$ to a segment where it has just been shown not to apply, *viz* $c = 4h$.

Instead of accusing Heron of inconsistent emendations one might of course suggest that somebody else produced the text we know by adding the warning and the parabolic approximation to Heron's original text, which will then have contained nothing but the received approximations; without other arguments than a general stylistic impression I find this explanation of the textual inconsistencies unlikely. Slightly more plausible is the possibility that Heron did not write the Archimedean approximation into the text but

[31] The approximation is indeed so much worse that Heron's reconstruction is almost certain; mathematical reflection would have shown (in modern terms) that the relative error goes to infinity when the arc goes to zero; Heron's text does not make this inference, but it does show that the formula is impossible when the height is less than $^1/_3$ of the chord. The idea to add $^1/_{14}(^c/_2)^2$ must come from fitting to a particular case – and probably to a semicircle with diameter 14.

as a marginal scholion to a borrowed treatise – a scholion that was so long that he distributed it over several manuscript pages – and that a later copyist inserted the scholion in the running text.[32]

In all three cases, Heron's debt to the extra-theoretical tradition remains obvious. The presence of the Archimedean π in other treatises is thus no indication that they depend on Heron; and in so far as they do not mention the Archimedean approximation formula for segment areas (and neither Columella nor any *Geometrica* component does so[33]) they are more likely *not* to use Heron (unless of course the insertion be post-Heronic).

[32] A similar argument shows that "Heron's formula" for the area of a triangle does not belong straightforwardly in the text but is an interpolation – either made by Heron in a source which he does not correct in details, by some later editor in Heron's text, or both in combination ⟦article I.8 treats of this question in detail⟧. I.vii proves a quasi-algebraic lemma (κατὰ ἀνάλυσιν, "by analysis"); the lemma tells that $\sqrt{\sqsubset\!\sqsupset[\square(a),\square(b)]} = \sqsubset\!\sqsupset(a,b)$, if $\square(d)$ designates the square on d and $\sqsubset\!\sqsupset(f,g)$ the rectangle contained by f and g. I.viii goes on with a numerical example, introduced independently of I.vii; this example is followed by the sentence "The geometrical demonstration of this is the following", after which it goes on with the words in which *Dioptra* treats the same matter – words which fit the *Dioptra* context but which constitute a pointless repetition in the *Metrica.* The whole proof follows the *Dioptra* version almost verbatim, inserting a couple of extra explanations, changing a few formulations, and changing the order of letters designating lines in order to make it alphabetic (this alphabetic order prevails in *Metrica* I). The exposition in the *Metrica* closes with the numerical example of the *Dioptra,* which is different from its own initial example (replacing μοῖρα with μονάς as the name of the unit, another instance of stylistic adjustment) – and then goes on in I.ix as if I.vii–viii had not been there. That I.vii–viii is an interpolation is obvious. The confusion of numerical examples and the double introductions of I.vii and I.viii speaks against an ascription of the passage in its entirety to a single hand. If we remove the introduction of I.viii and the ensuing numerical example, the whole structure – an introductory lemma (here algebraic, there dealing with the sum of a geometric progression) followed by a geometric proof – is similar to that of I.xxvii–xxix,xxxii, the Archimedean approximation to the segment. The most likely explanation may be that Heron made both interpolations apart from the introduction and first numerical example of I.viii (either as marginal scholia or without smoothing the text consistently), following *Dioptra* closely in the demonstration but expanding a few difficult points; and that a later editor added the opening of I.viii and the first numerical example in order to motivate the ensuing proof.

While "Heron's formula" is thus in some way an interpolation in the *Metrica,* there seems to be no reason to doubt that it belongs originally in the *Dioptra* – cf. Schöne's rejection [1903: xix*f*] of Hultsch's contrary opinion in the preface. Nor is there any serious reason to doubt that it was discovered within the Greek world, even though those Arabic writers are almost certainly mistaken who ascribe it to Archimedes – Heron, when we know for sure that he has reasons to do so, always gives credit to the great Syracusan. But neither the *Dioptra* nor the *Metrica* do so on this occasion.

[33] Both, moreover, give the post-Archimedean formula for the minor segment in the impossible case $c = 4h$.

The procedures in their actual detail

Only procedures of a certain complexity allow so much variation that the question of dependence or independence can be discussed – it is difficult to invent more than one reasonable way to calculate a rectangular area. But the procedures used to find the heights of scalene triangles and trapezia do offer significant contrasts, and so do the ways the circle, the semicircle and the segments are dealt with. Heights first.

These are calculated from a side and its projection on the base by means of the Pythagorean theorem. For inner heights, three different formulae for the projections of the sides turn up in the material (see Figure 3): One is "Euclidean", based on *Elements* II.13; one is "algebraic", based on the principle of *Elements* II.8; one, finally, is a variant of the "algebraic" formula, replacing II.8 with II.6. All formulae are of course algebraically equivalent, but it is clear that the sources consider them different,[34] and that they were handed down consistently as different methods.

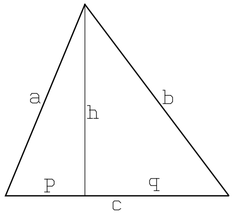

Figure 3.

The first, as stated, is based on *Elements* II.13 (the "Extended Pythagorean theorem"),

$$\square(a)+2\sqsubset\!\sqsupset(q,c)=\square(b)+\square(c),$$

whence

$$q=\frac{b^2+c^2-a^2}{2}\div c$$

The "main algebraic formula" makes use of semi-sum and semi-difference, as the quasi-algebraic tradition had always done:

$$\frac{q-p}{2}=\frac{b^2-a^2}{2}\div c,\quad \frac{q+p}{2}=\frac{c}{2}$$

whence

$$q=\frac{c}{2}+\frac{b^2-a^2}{2}\div c$$
$$p=\frac{c}{2}-\frac{b^2-a^2}{2}\div c$$

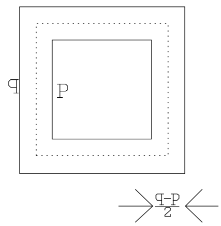

Figure 4.

The probable argument behind this formula runs as follows (see Figure 3):

$$\square(b)-\square(a)=[\square(q)+\square(h)]-[\square(p)+\square(h)]=\square(q)-\square(p)$$

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34 Thus very explicitly in Fibonacci's *Pratica geometrie* III.2 [ed. Boncompagni 1862: 34*ff*].

$\square(q)$–$\square(p)$, however, is the difference between two squares, most likely to be understood as the band between concentric squares (see Figure 4):

$$\square(q) - \square(p) = \sqsubset\!\sqsupset(\frac{q-p}{2}, 2(q+p)) = \sqsubset\!\sqsupset(\frac{q-p}{2}, 2c)$$

(This argument, a "naive" version of *Elements* II.8, is found in ibn Thabāt's treatise and in *Metrica* I.xxvi). Therefore,

$$\frac{q-p}{2} = \frac{b^2 - a^2}{2} \div c$$

The "algebraic alternative" has an analogous proof, but presupposes *Elements* II.6 (or "proto-*Elements*-II.6") instead of "proto-*Elements*-II.8" (see Figure 5):

$$p = ½(c - \frac{b^2 - a^2}{c})$$

since

$$\sqsubset\!\sqsupset(c, q-p) + \square(p) = \square(q)$$

whence

$$\sqsubset\!\sqsupset(c, q-p) = \square(q) - \square(p) = \square(b) - \square(a),$$

and since, moreover

$$2p = c - (q-p).$$

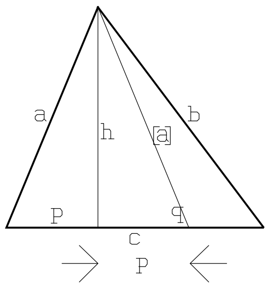

Figure 5.

For the outer heights of an obtuse-angled triangle, two methods occur (see Figure 6). One is based on *Elements* II.12 ("Extended Pythagorean theorem"):

$$\square(a) = \square(b) + \square(c) + 2\sqsubset\!\sqsupset(p, c)$$

whence

$$p = \frac{a^2 - b^2 - c^2}{2} \div c$$

The other, analogous to the above "main algebraic formula",

$$p = \frac{a^2 - b^2}{2} \div c - \frac{c}{2}$$

$$q = \frac{a^2 - b^2}{2} \div c + \frac{c}{2}$$

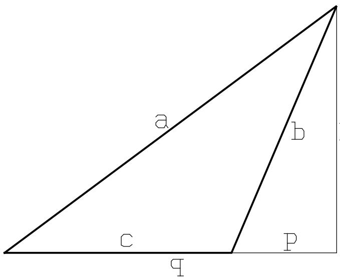

Figure 6.

is only found in one case (*viz* in *Liber mensurationum*), anomalous also for other reasons and probably Abū Bakr's own invention (apart from the equilateral triangle it is the only polygon in the work that is not composed from Pythagorean triangles).

The distribution of the different formulae can be summarized, using the following abbreviations:

Alg: "Main algebraic formula" based on semi-sum and semi-difference and proto-*Elements* II.8.

ExtP: Elements II.13 or 12 ("Extended Pythagorean theorem").

II.6: The variant of the "algebraic" method that refers to *Elements* II.6.

Abū Bakr, *Liber mensurationum*

Acute-angled trapezium: *Alg;* alternative, *ExtP*
Obtuse-angled trapezium: *Alg* (Abū Bakr's own construction)
Acute-Angled triangles: *Alg;* alternative, *ExtP*
Obtuse-angled triangles: Inner height *Alg;* outer heights *ExtP*

Al-Khwārizmī, *Algebra*

Acute-angled triangle: determination by *al-jabr*
Obtuse-angled triangle: Only one height exists, cross-reference

Al-Karajī, *Kāfī fī' l-ḥisāb*

Acute-angled triangle: *Alg*
Obtuse-angled triangle: *ExtP*

Ibn Thabāt, *Ghunyah al-Ḥussāb*

Acute-angled trapezium: *Alg*
Obtuse-angled trapezium: absent
Acute-angled triangle: *Alg;* alternative, *II.6*
Obtuse-angled triangle: *ExtP*

Savasorda, *Liber embadorum*

Acute-angled triangle: *ExtP;* alternative, *II.6*
Obtuse-angled triangle: *ExtP*
Acute-angled trapezium: Alg, with a mistaken cross-reference
Obtuse-angled triangle: *ExtP*

Leonardo Fibonacci, *Pratica geometrie*

Acute-angled triangle: *ExtP;* alternatives, *Alg* and *II.6*
Obtuse-angled triangle: *ExtP;* cross-references to *Alg* and *II.6*
Acute-angled trapezium: *Alg*
Obtuse-angled trapezium: *ExtP*

Heron, *Metrica*

Acute-angled triangles: *ExtP*
Obtuse-angled triangles; *ExtP*
Trapezia: Only a cross-reference

***Geometrica*/AC**

Acute-angled triangle: *ExtP*
Obtuse-angled triangle: *ExtP,* alternative akin to *II.6*
Acute-angled trapezium: corrupt or erroneous
Obtuse-angled trapezium: cross-reference

Geometrica/S

Scalene triangles and trapezia are not treated

Heights are never computed in the Old Babylonian tablets, and the few Late Babylonian texts that have been found only calculate those of isosceles triangles and trapezia. It would seem a natural guess that the determination of heights in scalene figures be a Greek invention. The material at hand, however, contradicts this assumption.

Heron, as we see, builds on Euclid, and *Geometrica*/AC does so, presenting the "algebraic" method in II.6 version as an alternative possibility. In the Arabic treatises, however, the situation is different – but only for internal heights. The determination of external heights follows *Elements* II.12; when finding internal heights, as a rule, the texts first state the "algebraic" solution, and then sometimes add the Euclidean method as an alternative. Savasorda, who tries to be primarily Euclidean when treating triangles, slips when he comes to the trapezium. So does Fibonacci.

The conclusion appears to be that the practical tradition knew the principle in "algebraic" form already before the Greeks; but that it had only applied it to internal heights, in agreement with al-Khwārizmī's statement that an obtuse-angled triangle possesses only one height. The Greeks generalized the concept of a height to the perpendicular on a side *or its extension,* and both theorems went into *Elements* II in a formulation that brought them into connection with the Pythagorean theorem and proved them from II.4 and II.7. One may even guess that the reason that II.12 (external heights) precedes II.13 (internal) is that it was new and hence more interesting. The practical tradition adopted the innovation and handed it down as faithfully as always, but kept the old way where it served, pointing however at times to the Euclidean method as an alternative way.

On this point we notice that Heron as well as *Geometrica*/AC follow the Euclidean way; but *Geometrica*/AC also shows that it knows the "algebraic" trick. *Geometrica*/S, in agreement with its use of the archaic surveyors' formula, does not care about such sophisticated questions.

Circle computations may seem slippery ground – after all, everything that is involved are squarings, square roots and numerical factors. None the less, they turn out to be informative (diameter d, radius r, perimeter p, area A):

Abū Bakr, *Liber mensurationum*

$A = d^2 - {}^1/_7 d^2 - {}^1/_2 \cdot {}^1/_7 d^2$; or $A = ({}^1/_2 d)\cdot({}^1/_2 p)$

$p = 3\,{}^1/_7\, d$; or $p = \sqrt{(A + {}^3/_{11} A)}$

Al-Khwārizmī, *Algebra*

$p = 3\,{}^1/_7\, d$, "a convention among people without mathematical proof")

(two Indian alternatives are mentioned, $p = \sqrt{\left(10d^2\right)}$ and $p = {}^{62832}/_{20000}\, d$)

$A = ({}^1/_2 d)\cdot({}^1/_2 p)$

Al-Karajī, ***Kāfī fī'l-ḥisāb***

$A = (^1/_2 d) \cdot (^1/_2 p);$ or $A = (^1/_4 d) \cdot p;$ or $A = d \cdot (^1/_4 p);$ or $A = d^2 - {}^1/_7 d^2 - {}^1/_2 \cdot {}^1/_7 d^2;$ or $A = p^2 \div 12\,^4/_7$

$p = 3\,^1/_7 \cdot d$

$d = p \div 3\,^1/_7$

Ibn Thabāt, ***Ghunyah al-Ḥussāb***

$A = (^1/_2 d) \cdot (^1/_2 p);$ or $A = (^1/_4 d) \cdot p;$ or $A = d \cdot (^1/_4 p);$ or $A = d^2 - {}^1/_7 d^2 - {}^1/_2 \cdot {}^1/_7 d^2;$ or $A = p^2 \div 12\,^4/_7$ or $^1/_4 p^2 \div 3\,^1/_7;$ or $A = {}^1/_4 \cdot (d \cdot p)$

$p = 3\,^1/_7 \cdot d$

$d = p \div 3\,^1/_7$

Savasorda, ***Liber embadorum***

$p = 3\,^1/_7 \cdot d$

$A = (^1/_2\, d) \cdot (^1/_2 p),$ alternative $A = d^2 - {}^1/_7\, d^2 - {}^1/_2 \cdot {}^1/_7\, d^2$

(followed by a correction of the π-value, $\pi = {}^{377}/_{120}$)

Old Babylonian texts

$p = 3d;\ A = p^2 \cdot 5'$ (normal method)

$A = {}^1/_4\, (d{\cdot}p)$[35]

Heron, ***Metrica***

$A = {}^1/_{14} \cdot 11 d^2$

$p = {}^1/_7 \cdot (22d)$

$d = {}^1/_{22} \cdot (7p)$

$2A = r \cdot p$

***Geometrica*/AC**[36]

$A = {}^1/_4 (d \cdot p);$ or $A = (^1/_2 d) \cdot (^1/_2 p);$ or $A = {}^1/_{88} \cdot (7 \cdot p^2);$ or $A = {}^1/_{14} \cdot (11 \cdot d^2);$ or $A = d^2 - {}^1/_7 d^2 - {}^1/_{14} d^2$ ("In Euclid"!)

$d = {}^1/_{22}(7p);$ or $d = 7 \cdot (^1/_{22} p)$

$p = 3d + {}^1/_7 d$

35 Strictly speaking, this formula is only used for semicircles in the Old Babylonian texts – in the problem text BM 85210, rev. I 18, and in various coefficient lists.

36 This scheme covers only chapter 17. Chapter 21, which tells to be an insertion "taken from another book by Heron", returns to the topic of circles, in ms C as follows: $p = {}^1/_7 \cdot (22d)$; $d = {}^1/_{22}(7p)$; $2A = p \cdot r$; "circulation" of a given area; separation of $d+p+A$; the corners between a circle and the circumscribed square. The first three formulae follow the *Metrica* rather closely, using even the same verbs though not the same grammatical forms; throughout, as in the *Metrica* but not in chapter 17, magnitudes are told to be *n* "units"/μονάδες; elsewhere, mss mostly AC reckons explicitly or implicitly in "ropes" (σχοινία) or "fathoms" (ὀργυιαῖ), whereas mss S and S.24 have the "foot" as their all-pervasive standard unit. Ms A represents a rewritten version of the same material, which expands both the explanations and the number of formulae (adding, e.g., the non-Heronic $A = {}^1/_4 \cdot pd = d \cdot (^1/_4\ p)$), but retains enough of Heron's words to make us sure of the connection.

p and d from $p + d$; $p = 3d + {}^1/_7 d$; $d = {}^1/_3 \cdot (p - {}^1/_{22} p)$; or $d = {}^1/_{22} \cdot (7p)$; $A = {}^1/_{88} \cdot (7 \cdot p^2)$; or $A = d^2 - {}^1/_7 d^2 - {}^1/_{14} d^2$; or $A = {}^1/_{14} \cdot (11 \cdot d^2)$; or $A = 3 \cdot ({}^1/_2 d)^2 + {}^1/_7 \cdot ({}^1/_2 d)^2$; or $A = (d^2 - {}^1/_4 \cdot d^2) + {}^1/_{21}(d^2 - {}^1/_4 \cdot d^2)$; or $4A = p \cdot d$; or $A = ({}^1/_2 d) \cdot ({}^1/_2 p)$; or $A = ({}^1/_2 \cdot p) \cdot d$; etc. (more complex numerical examples follow)

***Geometrica*/S**

$A = (11d^2) \div 14$
$p = (22d) \div 7$
$d = (7p) \div 22$
$p = 3d + {}^1/_7 d$;
$d = (p \div 22) \cdot 7$;
$A = {}^1/_{14} \cdot (11d^2)$; or $A = {}^1/_4 \cdot (p \cdot d)$; or $A = {}^1/_{88} \cdot (7p^2)$; or [corrupt or deliberately misleading]

Columella, *De re rustica*

$A = {}^1/_{14} \cdot (11d^2)$

Here we should distinguish as carefully as our texts between $A = {}^1/_4 \cdot (d \cdot p)$ and $A = {}^d/_2 \cdot {}^p/_2$, between taking ${}^1/_n$ and dividing by n (μερίζω), and between ${}^1/_7 \cdot (22d)$, $(22d) \div 7$ and $3d + {}^1/_7 d$. The "orthodox" Archimedean formulae are those found in the *Metrica.* "Those who made more precise investigations", as we remember, took p to be $3d + {}^1/_7 d$ – and in all cases where this formula is used,[37] the formulation tells very clearly that the diameter is taken thrice, and calculated explicitly, after which a supplementary seventh is added. Everywhere, the expressions τρισσάκις and τριπλάσιον are used even when neighbouring multiplications are ἐπὶ n. This is quite striking. The Old Babylonian texts, indeed, find the perimeter as the diameter "repeated until three" or as "thrice" the diameter, not by the normal multiplication used, e.g., when A is found as $5'$ times p^2; and the same idea is still found in a design booklet from c. 1488, written by the master builder Mathes Roriczer – in a constructive formulation that betrays the probable reason for the survival of the particular wording[38]. It seems beyond doubt that the anonymous practitioners who "made more precise investigations" belonged within the tradition which is already reflected in the Old Babylonian tablets, although Archimedes's calculation caused them to add a supplementary seventh;[39] in the Arabic writings the characteristic formulation

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[37] Mss AC, 17.8, 17.10, 17.29; S:22.16; and S:24.45; cf. ms A, 17.16, where the area is found as $3({}^1/_2 d)^2 + {}^1/_7({}^1/_2 d)^2$.

[38] "If anyone wishes to make a circular line straight, so that the straight line and the circular are the same length, then make three circles next to one another, and divide the first circle into seven equal parts", one of which is marked out in continuation of the three circles – *Geometria deutsch,* 9, trans. [Shelby 1977: 121]. ⟦Cf. article I.11.⟧

[39] The formula $A = (d^2 - {}^1/_4 \cdot d^2) + {}^1/_{21}(d^2 - {}^1/_4 \cdot d^2)$ looks like a similarly corrected version of $A = (d^2 - {}^1/_4 \cdot d^2)$, a formula that seems to be presupposed in N^{os} 32–33 of the Demotic P. Cairo J.E.89127–30,89137–43 (third century BCE). The proofs of the same problems use the formula $A =$

was discarded,[40] but it was conserved by the higher artisans of Christian Europe.

The determination of the area as $^1/_4 \cdot(p \cdot d)$ seems to be inherited, too; whether the variant $A = (^1/_2 d)\cdot(^1/_2 p)$ is also part of the Near Eastern legacy or derived from Archimedes is undecidable.[41] Equally undecidable is the questions whether the idea of determining the area from the perimeter was inherited from the Old Babylonian orbit – however much the idea seems unfamiliar to us, the practical problem of finding the volume of a standing log or column would easily call for independent "invention". So much is sure, however, that none of these formulae in *Geometrica*/AC and *Geometrica*/S are borrowed from Heron's *Metrica*, and that the treatment of the circle is strongly eclectic in either *Geometrica;*[42] among other sources they may *also* have used the *Metrica;* but material from the *Metrica*, if at all there, is by no means privileged.[43] As we see, mss AC – in other respects more "Greek" than ms S – even favour the primacy of the perimeter. Particularly intriguing is the supposedly "Euclidean" formula from AC 17.5. It corresponds so closely to the Arabic formula $A = d^2 - {}^1/_7 d^2 - {}^1/_2 \cdot {}^1/_7 d^2$ that we may presume it to be a translation from a language using ascending continued fractions as a standard idiom – either the Arabic or the Aramaic/Syriac.[44] Since the same formula is also used

$(^1/_4 \cdot p)\ d$, where p is found as $3d$ – see [Parker 1972: 40*f*]. Another corrected version of the Demotic rule seems to be found in S:24.39 (the independent treatise from ms S), $A = {}^1/_2\ d^2 + {}^1/_4\ d^2 + {}^1/_{28}\ d^2$.

40 Al-Khwārizmī thus multiplies (*ḍaraba*) the diameter directly with the number $3^1/_7$ (*šalāšah wasubu*').

41 Al-Khwārizmī gives the sketched proof that in every regular polygon "you find the area by multiplying half of the perimeter with half of the diameter of the largest inscribed circle" [trans. Gandz 1932: 70]. This is evidently not a direct reference to the Archimedean proof; but it may equally well be an argument produced by al-Khwārizmī himself for an inherited formula and an argument inherited together with the formula and sharpened by the mathematician al-Khwārizmī.

42 This eclecticism is abundantly confirmed both by the random organization of the text, by the shift between ἐπὶ … ποιέω- and πολυπλασιάζω-multiplication even within the same chapter (e.g., chapter 17), and the use of division instead of part-taking, in some passages correlated with πολυπλασιάζω-multiplication (e.g., 17.32–36), in others not (e.g., 17.18–22).

43 Even isolated borrowings into ms S and mss AC, chapter 17, are highly unlikely (AC 21.1–3, on the other hand, are clearly derived from Heron but also presented as an extraneous insertion – see note 36): in all cases where formulae are shared with the *Metrica,* either the choice of numerical example, the details of the terminology, or both, are different. Since neither *Geometrica* shows traces of stylistic normalization, such differences are significant. Ms A chapter 21, moreover, shows to which extent even expanding rewriting would conserve the original vocabulary.

44 Alternatively, one might think of an adoption of the Archimedean result in Demotic Egypt. On one hand, however, better candidates for a Demotic-Archimedean formula are at hand – see note 39; on the other, the Arabic formula, which would then be likely to derive from the same source, is found in treatises that contain no hints of such a connection.

Ascending continued fractions are not uncommon in *Geometrica*/AC, it is true, but only as the outcome of multiplications and where reduction would yield an unhandy result – e.g., when

in the separate treatise S:24, in a context that does not suggest a late Byzantine borrowing of Arabic material, it seems that the Archimedean π had already reached the Syrian practitioners well before the Hegira.[45]

Semicircles offer the same picture – there is no need to go into details. The very fact that they are not dealt with on their own in the *Metrica* but are so in both *Geometrica* (and in the brief treatment of mensuration in *De re rustica*) shows that Heron is not the main reference of these works.

The treatment of segments is informative in a different way. As we know from Heron, the determination of their area by means of approximate formulae was old in his world – whatever this world was.[46] One was pre-, another post-Archimedean. The mathematical quality of the formulae is not impressing. If Heron is responsible for the Archimedean approximation to small segments, his work was messy (see text before note 32); if, against my expectations, the insertion is later, he was also a not very critical compiler. Both *Geometrica* versions, in any case, are such compilations, and not with the *Metrica* as their main source; both also offer approximate formulae for the length of the arc.

The treatment of segments in Arabic treatises is totally different. They find the area from height, chord and arc, using thus a procedure that is both correct and exact, but evidently overdetermined.[47] This contrast is important: since practitioners' mathematics

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$7^1/_2\ ^1/_{10}\ ^1/_{15}\ ^1/_{75}$ is reduced no further than $7+(3+^2/_5)/5$ (12.48) or $^{24}/_{49}$ expressed as $(3+^3/_7)/7$ (12.54). But all these cases conserve the genuine form of the ascending continued fraction, while the expression $^1/_7+^1/_{14}$ reduces it to a mere allusion.

45 In this connection is should also be noted that the circular area is similarly determined as $A = d^2 - {}^1/_7\, d^2 - {}^1/_2 \cdot {}^1/_7\, d^2$ in the Hebrew *Mišnat ha-middot* [ed. Gandz 1932: 48]. The date of this work is disputed – c. 150 CE according to Gandz, early Islamic period if we believe Gad Sarfatti [1968]. As far as I can judge from Sarfatti's English abstract, he overlooks the Aramaic/Syriac influence on the formation on both Hebrew and Arabic terminologies, and forgets that shared metaphors as close at hand as the "arrow" (for the height of a circular segment understood as a "bow-shape") are likely to be common heritage. All in all, Gandz's dating seems the better one. The subject-matter points in the same direction: *Mišnat ha-middot* gives the post-Archimedean approximation for the circular segment, which is absent from all the Arabic treatises – in particular from that of al-Khwārizmī, which one way or the other is closely related to the *Mišnat.* Cf. also note 48.

46 Segment computations are indeed found in a couple of Old Babylonian texts from a text group which also gives the area of the semicircle as $^1/_4(d \cdot p)$; unfortunately one is corrupt and the other badly broken, but the former seems to involve arc, chord and height, while the latter has a vague similarity with the formula of "the ancients" but apparently no more. One text, finally, demonstrates that the Babylonians knew how to calculate the diameter of the circle from the chord and the height of a segment – essential if a major segment is to be found from the formula for the minor segment, or vice versa.

47 The reason for this is not ignorance but practical wisdom; al-Karajī teaches indeed how to find the arcs for 60° and 120° and how to interpolate from these values; but he also tells that direct

is invisible, we have almost no other evidence that the practitioners of the Hellenistic world (or some of them) had developed canons and procedures of their own – procedures which went no further in the Near Eastern tradition than *Mišnat ha-middot,* in contrast to *Elements* II.12 and "Heron's formula".[48] The problem of Heron's historical position cannot be reduced to a simple dilemma between a Euclidean-Archimedean and a Near Eastern practitioners' tradition.

Numerical values

Numbers are treacherous – apparently striking coincidences may result from the compulsion of mathematical relations. That the sides of the standard scalene triangle are 13–14–15 may look as evidence for connections, but proves nothing in environments that know the Pythagorean theorem and want to work in integer numbers. Whether the special status of 7 is a number-psychological universal in cultures that count as far as 7 or is the outcome of cultural diffusion (and, in the latter case, diffusion at which level) is so far undecided. If we want to show that a particular number is chosen because it belongs with a particular problem type and not because the author likes it in general or because it is more convenient than other choices we need to demonstrate that it is *only* chosen in this particular situation and not elsewhere.

The use of 10 as the preferred side length for regular polygons *can* be argued in this way to belong with the situation. We have encountered it as the standard side in the problem on the four sides and the area, but the evidence is much more impressing:

Abū Bakr, *Liber mensurationum*

Square, equilateral triangle

Al-Khwārizmī, *Algebra*, and al-Karajī, *Kāfī*

Equilateral triangle (and the square side corresponding to the paradigmatic irrational root, $\sqrt{200}$)

Ibn Thabāt, *Ghunyah al-Ḥussāb*

Square, equilateral triangle, regular hexagon (exemplar for regular polygons)

Savasorda, *Liber embadorum*

Square, equilateral triangle

measurement is much better [trans. Hochheim 1878: II, 25*f*].

[48] The only analogous case which I know of is the survival of the "thrice the diameter plus an extra seventh" in Europe, a formula that is not found in Arabic treatises (see note 38). Even in this case, *Mišnat ha-middot* contains the information that "the people of the world [or, "the landmeasurers"] say that the circumference of a circle contains three times and a seventh of the thread" (V.4, trans. [Gandz 1932: 49]).

Heron, *Metrica*

First square; regular n-gons, $2<n<13$; diameter of first circle

***Geometrica*/AC**

First equilateral triangle; regular n-gons, $6<n<13$; second example for $n = 5$.

That 10 is not chosen with this high frequency just because it is a round number, in Greek as well as Hebrew and Arabic, follows if we look at other situations where parameters are chosen freely – e.g. the width of the rectangle that is squeezed into the 13-14-15-triangle in order to produce a convenient trapezium. This width is *never* 10. As far as the Old Babylonian problem is concerned one may notice that this problem is the *only* problem in the whole Old Babylonian record about a single square where the side is 10. The Old Babylonian school norm – evidently connected to the characteristics of the place value system – is 30, mostly in the order of magnitude of "minutes". Even in the *Metrica* and in *Geometrica*/AC, 10 possesses no generally favoured status. That it plays a central role precisely as the standard side of the regular polygons is thus an indubitable and very direct reference to the Near Eastern tradition. How it was mediated through local traditions we cannot say.

Phraseology

As to phraseology, I shall limit myself to two observations with different but not contradictory implications.[49]

Above, the phrase καθολικῶς/"in general" and its correspondence with the use of *semper* in Abū Bakr's *Liber mensurationum* was mentioned. In *Geometrica*/AC 21.9, καθολικῶς and ἀεί/"always" occur in the riddle about the circular diameter+perimeter+ area, ἀεί without καθολικῶς in a few other circle and segment formulae and in some formulae dealing with regular polygons – and as far as I have noticed nowhere else in AC;[50] both words tell that a certain number used in the procedure does not depend on the particular free parameter of the problem – in the quadratic problems thus the sum (the coefficients are *not* free, as it follows, they have to be the "natural" parameters of the problem). In the context of mss AC it is thus evident that the problem $d+p+A = \Sigma$ – found for the first time in an Old Babylonian text from a group which gives the semicircular area as ${}^{1}/_{4}(p{\cdot}d)$ – is brought into the main text from outside (cf. also note 36 on the status of chapter 21).

49 But see also note 42 on the conclusions that can be drawn from the distribution of the terms for multiplication.

50 Of course, formulae for circles and their parts and for regular polygons are precisely those where fixed numerical parameters occur; but only a small minority of the corresponding problems contain the ἀεί.

S:24 uses καθολικῶς in the square area+perimeter- as well as the diameter+perimeter+area-problem (S.24.3 and S.24.46–47, respectively), alternating in the latter with πάντοτε/"at all times"; S:24.46 and 47, fully parallel apart from different values of the sum Σ, use both words but in changing places, treating them thus as synonyms. πάντοτε is also used in other problems dealing with circles (N^{os} 43–45, the first of which asks for the separation of $p+d$), while a sequence of problems asking for the separation of area and perimeter of a Pythagorean triangle (N^{os} 10–13) use παντρός/"always" in the same function (once πάντοτε). πάντοτε is also used in S:22.4, the formulation in general terms of the $^1/_2 \cdot$ base $\cdot$ height-rule for the triangular area, and together with καθολικῶς in *Geometrica*/S 17.5–6, 19.6 (circle and segment formulae).

Most of the occurrences of the explicitation of general validity are thus within problems that ask explicitly for the "separation" of a sum,[51] a familiar phrase from the Near Easter tradition which is already reflected in the Old Babylonian school texts. Even though these texts contain no similar term for generality, ⟦[52]⟧ there is little doubt that the usage was adopted together with the area+perimeter and diameter+perimeter+area problems, indubitable legacy of the Near Eastern tradition.

As we see, the word chosen to express general validity varied from one text to the other; so does the term for separation. *Geometrica*/AC 21.9 has διαστέλλω (with καθολικῶς and ἀεί); S:24.3 (square area+perimeter) uses διαχωρίζω (with καθολικῶς); S:24.10–13,43–47 (triangular area+perimeter, circle problems including $d+p$ and $d+p+A$) employ ἀποδιαστέλλω (with παντός, πάντοτε and, in the $d+p+A$-problem, καθολικῶς). The correlations seem to reflect separate developments that were accidentally brought together again, first by Byzantine compilators and next by Heiberg.

This inference is confirmed by other terminological considerations. Many of the παντός/πάντοτε-problems of S:24 (but not S:24-problems in general) share the characteristic phrase φανερόν, "obviously". The two mixed second-degree problems of S:24 (square area+perimeter, circular diameter+perimeter+area), on the other hand, use quite different terminologies for multiplication in spite of their shared ultimate origin – the former thus the geometrically suggestive ποιέω ἐπὶ/"I put on top of" for squaring, the latter πολυπλασιάζω/"I make multiple", with its purely arithmetical (and even integer-arithmetical) connotations. If we think of Moritz Cantor's old metaphor,[53] according to which the development of mathematics is to be likened to a river landscape, the river

51 And all separation problems in the material except *Geometrica*/AC 17.9 (circular diameter+perimeter) use καθολικῶς, ἀεί, πάντοτε or παντός. The coupling is certainly strong.

52 ⟦This turns out not to be true. Two mathematical texts from late Old Babylonian Susa (TMS XII, XIV) use the term *kajamānum,* "constantly, always, customarily"⟧ in precisely the same function – see [Bruins & Rutten 1961: 79, 84], cf. interpretations in [Muroi 2001] and [Robson 1999: 119–121].⟧

53 [Cantor 1875: 2].

that had sprung from Near Eastern geometrical practice had dissolved itself in later antiquity into a delta, in a multitude of independent streams now running together, now splitting apart. Heron knew some of them and used them – at times literally – in the *Metrica; Geometrica*/AC collected others, *Geometrica*/S and S:24 still others. Further studies of terminology and style may help us sort out more details; given the complexity of the situation and the paucity of sources for precisely the practitioners' level of mathematical activity, however, we are not likely to get very far.

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Chapter 11 (Article I.10)
Which Kind of Mathematics Was Known and Referred to by Those Who Wanted to Integrate Mathematics in Wisdom – Neopythagoreans and Others?

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

J. Høyrup, *Selected Essays on Pre- and Early Modern Mathematical Practice*,
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Abstract

Plato, so the story goes, held mathematics in high esteem, and those philosopher-kings that ought to rule his republic should have a thorough foundation in mathematics. This may well be true – but an observation made by Aristotle suggests that the mathematics which Plato intends is not the one based on theorems and proofs which we normally identify with "Greek mathematics".

Most other ancient writers who speak of mathematics as a road toward Wisdom also appear to be blissfully ignorant of the mathematics of Euclid, Archimedes, Apollonios, etc. – though not necessarily of their *names*. The aim of the paper is to identify the kinds of mathematics which were available as external sources for this current (on the whole leaving out of consideration Liberal-Arts mathematics as not properly external). A number of borrowings can be traced to various practitioners' traditions – but always as bits borrowed out of context.

Contents

SONJA BRENTJES gewidnet
und
in Erinnerung an GERLINDE WUßING

Remarks about Plato

In Aristotle's *Metaphysics* N, $1090^{b}14–1091^{a}5$ there is a short polemical passage dealing with the "ideal numbers" and the supposedly Platonic "mathematical" numbers intermediate between ideal and sensible number. About the former it is said that "not even is any theorem true of them, unless we want to change mathematics and invent doctrines of our own" [trans. Barnes 1984: 1723], and about the latter that they are either "the same" as ideal number or an absurdity.

We must presume Aristotle to have known Plato's and other contemporary doctrines better than we do. This would not necessarily have prevented him from distorting such doctrines for polemical purposes; but we must also assume that his *audience* knew these doctrines, and this must have kept Aristotle from making too gross distortions if he wanted to convince.

Aristotle's formulation implies that the current mathematics about numbers which he refers to contains theorems; we should hence describe it as "theoretical arithmetic", as different from practical computation.

This might raise some doubts about that passage in *Republic* VII (525A) where Socrates/Plato distinguishes two branches of knowledge about number, logistics and arithmetic, normally taken to correspond to the two approaches to number of which he speaks in the following (525B-526C): the vulgar approach of retailers, and the noble approach which suits the guardians, the approach serving war and contemplation – from which the approach dealing with the contemplation of merely intelligible number is singled out as particularly worthy.

Actually, there is nothing in the text which suggests that this contemplation should deal with theorems, demonstration or anything of the kind. It may be true that the *arithmetic* spoken of in 525A belongs to the same family as *Elements* VII–IX, but in that case there is nothing which identifies it specifically with the discipline about numbers accessible only to thought which should be taught to the future guardians. Alternatively, *arithmetic* might really be intended to designate this latter discipline (less likely, since the word is used before Glaucon understands the distinction), but then there is no reason to believe that Plato thinks of the theoretical discipline we know from the *Elements*.

Things become even more blurred if we look at 587D, where Socrates shows the distance between the tyrant's imagined pleasure and real pleasure to be the "plane number" 3·3 = 9 when regarded as "number of the length (τοῦ μήκους ἀριθμός). He goes on to claim it to be "clear, in truth, how great a distance it is removed according to *dýnamis* and third increase" (κατὰ δύναμιν καὶ τρίτην αὔχην). Glaucon comments that it is

"clear at least to the logistician" (δῆλος τῷ γε λογιστικῷ). We may plausibly link this reference to a logistic art concerned with the second and third power of (seemingly pure) numbers to Diophantos's description of his own concern as "theoretical arithmetic", of which the *dýnamis* is an "element" (στοιχεῖον) [ed. Tannery 1893: I, 4] – which might imply that Plato's distinction between logistics and arithmetic was not the same as that between theory and non-theoretical practice, and that it did not coincide with the distinctions made in later time.[1] In any case it makes it even more obvious that nothing in Plato's text forces us to believe that the guardians should learn a theoretical arithmetic containing theorems.[2]

Plato himself had certainly encountered theoretical mathematics consisting of theorems. There are references to it in the dialogues, even though most of them do not prove intimate familiarity. Some, however, do prove direct familiarity at least with rather technical *results* – for instance, the references to mathematical harmonics and to the system of heavenly circles in *Timaeus* 35A–36D.[3] Moreover, Eudemos was so close in time and so close to Aristotle (that is, to somebody who was quite reserved as regards Plato's mathematics) that his narrative of mathematicians working together at the Academy must be considered reliable, at least *grosso modo*. But it might be time to revise the reading which has been current since the Renaissance, according to which this was the kind of mathematics that Plato saw as conducive to "wisdom".

I am quite aware that I am not the first to propose such a revised reading. I shall only mention Review Netz' delicious simile of "the book according to the film" [1999: 290]:

> we all know the fate of a book which suddenly becomes a bestseller after being turned into a film – in the version "according to the film". This process was originated in south

[1] Summarized in [Heath 1921: I, 14–16]. Both Geminos and a scholiast to Plato's *Charmides* take logistics to deal with concrete, not abstract number. Plato's elder contemporary Archytas, taken by Heath to think along similar lines, instead states that logistics is "far ahead of other arts in relation to wisdom or philosophy", and that it "seems to make the things of which it chooses to treat even clearer than geometry does; moreover, it often succeeds even where geometry fails". If anything, this supports the assumption that the meaning of the word changed between Plato and Geminos.

[2] Thus, however much some latter-day mathematicians would like the philosopher-kings to be mathematicians, they were *not* (in any sense in which the mathematicians would recognize themselves). Were they philosophers? My personal hunch, built on the strength of the description of *light* in the myth of the cave, and also in the *Seventh Letter* (independently of whether the latter text is really written by Plato or by a close disciple) is that their long preparation was meant to guide them to *mystical* experience and insight.

In an observation about Whitehead's dictum that European philosophy is a series of footnotes to Plato, Imre Toth once made the point [1998] (I do not have the book at hand for a precise reference) that the same holds for Plato himself: philosophy *is* footnotes, namely critique, commentary and second thoughts. Philosophy thus begins with Aristotle – Plato was a sage.

[3] Familiarity with mathematical *results* and *facts* is also abundant in Theon of Smyrna's *Expositio*. If chronology did not forbid it, Plato might probably have learned all his mathematics from Theon.

> Italy in the late fifth century BC, but it was Plato who turned "Mathematics: the Movie" into a compelling vision. This vision remained to haunt western culture …

and his summing up of the curriculum passage in *Republic* VII as "Do it, but only in a certain, limited way" (p. 303).

What was at hand?

I shall not go on with Plato but concentrate on less prestigious readers of the "book according to the film" – more precisely those "quasi-gnostic" writers[4] who claimed mathematics to be a road toward Wisdom. Which were the types of mathematics that were around for those who, for lack of competence or sympathy, would not read Eudoxos, Euclid, Archimedes, Apollonios, etc. – leaving aside that arithmology which the group itself and its tradition created.[5]

On a general level, an answer is offered by *Republic* VII, 525a–527c: the arithmetic of merchants, and the practical geometry used in warfare; inherent in the etymology of γεωμετρία is also the geometry of surveying, to which we might add that of city-planners and architects. But on that level of generality we find no information of relevance for our question.

We should therefore first ask what went *together* with the mathematics of merchants and accountants, and with the practical geometries. Indeed, the everyday routine of these groups was too trite to be paraded as kindred to "wisdom".

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4 Since some of these writers might be characterized as Neopythagoreans, others as Neoplatonists, others again as late Platonists, I introduce this ad-hoc neologism.

5 I shall also leave aside what we find in the handbooks serving or reflecting Liberal-Arts mathematics. Part of what they include derives from sources that somehow saw mathematics as a way toward *gnosis*, and many of those who belong to the quasi-gnostic tradition may have known the mathematical substance of their own tradition by way of its presence in this kind of teaching – which could imply that the very notion of "their own tradition" is problematic, this "tradition" possessing perhaps no inner continuity beyond the mere idea that "mathematics" or "number" were conducive to higher insight. It is true that the purpose of Liberal-Arts teaching was to impart culture rather than Wisdom; but the reason that the mathematical disciplines were at all accepted (at least by a minority – the actual curriculum seems not to have gone much beyond grammar and rhetoric) as a constituent of necessary culture was probably their supposed affinity with Wisdom. In consequence, Liberal-Arts mathematics cannot be distinguished from quasi-gnostic mathematics as a separate and external entity, which excludes it as an independent source for quasi-gnostic mathematics *per se*; at the same time, however, its different pretensions forbids an identification of the two.

With the partial exception of Proclos, I shall also not consider those Neoplatonists who did understand their Euclid, as well as the young Platonizing Augustine, whose *De musica* [PL 32, 1184] sees "sensible number" as a step toward understanding "immutable number" but who was competent enough to have read Euclid on his own (and still could not forget it when writing *De civitate Dei*).

Fortunately for the various quasi-gnostics, the same need for something beyond trite everyday turns up in all professions which use their particular knowledge as a means to demarcate themselves. In oral mathematical practitioners' cultures, the need was often fulfilled by "neck riddles" – riddles which one had to be able to answer in order to show oneself an authentic member of the group, and which, in order to serve this purpose, should look as if they had something to do with the particular practice of the group. Among the various kinds of mathematical practitioners, this gave rise to a phrase which, with variations, is often found in writings situated at the interface between oral practitioners' culture and literate mathematics, "tell me, if you are a diligent calculator,…", accompanying so-called "recreational" problems.[6] In the original context, "recreational" is thus a misnomer – it corresponds to the role of the problems within the new, literate context.

These problems often go together in clusters, depending (so we must presume) on clusters of social groups in professional interaction. As a matter of course we have no direct evidence from the non-literate groups which were their original carriers, but the problems may turn up in written sources after having been adopted by widely scattered literate traditions – often solved by means of techniques developed by these traditions. The obvious parallel is Apuleius's taking a fable "as old women tell them", inserting into it the names of Amor and Psyche and twisting it for his own (moral-religious) purposes.

The "Silk Road" cluster

The best known cluster – in the sense that it gathers very well-known recreational problems, not that it is normally thought of as a cluster[7] – may have been carried by the community of long-distance traders interacting along the Silk Road and/or the sea routes over the Chinese Sea and the Indian Ocean. Within this cluster we find:

– unity doubled 30 or 64 times (the "chessboard problem");
– pursuit problems;[8]
– problems of the type "a hundred fowls";[9]
– problems of the type "give and take";[10]

6 I have discussed this relationship in [1990a] and [1997] (and elsewhere). ⟦It should be taken note of that the characteristic culture of a social group may be of oral type even though many members may be partially literate; literacy needs both density and intensity before it determines predominant interaction modes, that is, cultural type.⟧

7 Most of these problems are listed in [Tropfke/Vogel et al 1980] together with a wide range of occurrences from China to Western Europe.

8 For instance, "one man starts 100 steps in front of another one; the first takes 60 steps while the second takes 100". Variants with increasing or decreasing speeds are also widespread.

9 For example, "I go to the market and buy 100 fowls for 100 dinars. A goose costs three dinars, a hen costs two, and chicks go three to a dinar".

10 For instance, "One man says to another, if you give me 30 dragmas of your money, I shall have

– problems of the type “purchase of a horse”.[11]

The “purchase of a horse” appears as a pure-number problem in Diophantos’s *Arithmetica* I.24 [ed. Tannery 1893: I, 56], from where I borrowed the numbers of the example. I.22 and I.23 contain a variant where each (of three respectively four) receives only a given fraction ($^1/_3$, $^1/_4$, $^1/_5$ and, in the four-number case, $^1/_6$) of the neighbouring one. With different dress the same bizarre mathematical structure turns up with three respectively six participants in the Chinese *Nine Chapters* VIII.12–13 [ed., trans. Vogel 1968: 87; ed., trans. Chemla & Guo 2004: 641, 643] from the first century CE.[12]

A strange passage in *Republic* I (333B–C) suggests that Plato knew about the problem, and supposed his readers to be in the same situation. At least he refers to the need to associate with an expert in horses when one is going to buy *in common* or sell a horse; since horses were not used for any purpose which made common possession meaningful, this is likely to be an oblique reference to the problem.[13]

A single case of even striking similarity proves nothing; but there are other similarities of the same kind.

Firstly, it is obvious that Zeno’s paradox of Achilles and the tortoise has the structure of the pursuit problem. Even though Diogenes refuted it by walking (if we are to believe Diogenes Laërtios – VI.39, trans. [Jürß 1998: 267]), the original intention may well have been not to refute the common sense of everybody but that of calculators – or at least to make a point with reference to a familiar mathematical problem, where the inexperienced calculator is likely to fall into the trap needed for the paradox.

Secondly, there is a problem which the Latin and Italian Middle Ages borrowed from the second-century Roman jurisprudent Salvianus Julianus [Cantor 1875: 146–149] – as found in Jacopo da Firenze’s *Tractatus algorismi* from 1307 [ed. trans. Høyrup 2007: 259]:

> A man is ill and wants to make testament. And he has a wife, who is pregnant. And this one devises that if his wife makes a male child, he leaves to him $^2/_3$ of everything of his, and to the wife he leaves $^1/_3$. And if the wife makes a female child, he leaves to the girl $^1/_3$. And to the wife $^2/_3$ of all his possession. Now it happened that the good man departed from this life, and in due time the wife gave birth and made a male child, and a female child.

twice what you have left. The other says, if you give me 50 of yours, I shall have thrice what you have left”.

11 For instance, “three men go to the market in order to buy a horse; the first man asks for $^1/_3$ of what the others have in order to be able to pay it, the second needs $^1/_4$ of the possession of the others, and the third only needs $^1/_5$ of what the first and the second have”.

12 My example of the pursuit was borrowed from problem VI.12 of the same Chinese treatise [ed., trans. Vogel 1968: 62; ed., trans. Chemla & Guo 2004: 519].

13 Below I shall present more evidence for the familiarity with the problem type in Plato’s times.

One may suspect the Roman jurisprudent to have taken over a mathematical recreational problem belonging to the same cluster and giving it a dress corresponding to his own field (juridically, the case would be impossible). Indeed, the earliest extant Chinese mathematical manuscript, the *Suàn shù shū* from no later than c. 186 BCE, contains a problem about a fox, a wild-cat and a dog going through a customs-post and sharing the tax according to the ratios between their skins, which again are pairwise 1:2 [trans. Cullen 2004: 45]. A similar story about animals (now eating in the same proportions) is found in the *Nine Chapters* [ed., trans. Vogel 1968: 28; ed., trans. Chemla & Guo 2004: 285–287].[14]

Taken alone, neither Plato's reference to the collective purchase of a horse, nor Zeno's paradox or the twin inheritance is more than a suggestion. Taken together, and seen in the light of the shared structure of *Arithmetica* I.22–23 and *Nine Chapters* VIII.12–13, they make it plausible that the cluster of problems to which they belong, and which reached from the Mediterranean to East Asia in the Middle Ages, was already known over most of the same area in Antiquity.[15] However, there is only one fairly certain set-off in the "book according to the film", namely Iamblichos's account of "Thymaridas's bloom",[16] a technique that can be used to solve problems belonging to the family of the "purchase of a horse". If we can trust Iamblichos's ascription to Thymarides (I have never seen any doubts raised), this shows that at least somebody in the Pythagorean environment of Plato's times was interested in number problems of a kind which was derived from the purchase-of-a-horse family, and which is also represented by Diophantos's *Arithmetic* I.22–25; since Iamblichos discusses the method in detail with examples, the interest must have remained alive in at least some Neopythagorean circles.

This, however, is the only trace I have found in wisdom-oriented writings type of mathematics pointing to the "Silk Road" problem cluster. Zeno, if he was really inspired by the pursuit problem, rather proved that the insights gained from mathematics are deceptive. In general, the sometimes elegant, sometimes convoluted tricks used to solve problems from this category may give the same impression, which of course might make them unsuited for the purpose. (Indeed, Plato's reference, if it is one, is to a situation where you should better not trust your own reason.)

14 There is no reason to conclude that all these problems *originated* in China; China just happens to be the only region outside the Mediterranean where documents of the kind from the epoch have survived.

15 Further supportive evidence could be found in the arithmetical epigrams of the *Greek Anthology*. The doubling of 1 "until 30 times", first found in a text from Mari in Iraq from the 18th century BCE, is also known from a Greco-Egyptian papyrus of CE-date, and again in the Carolingian *Propositiones ad acuendos iuvenes*, which in the main might consist of problems that had circulated in the Gallic region since Roman times – see [Høyrup 1990b: 23f].

16 Ed. [Pistelli 1975: 62–67], cf. [Heath 1921: I, 94–96]. Heath (p. 69) describes Thymarides as "an ancient Pythagorean, probably not later than Plato's time").

Surveyors' riddles

Another cluster consists of geometric proto-algebraic riddles about squares and rectangles:[17]

– given, for a square, the sum of or the difference between the area and either one or all four sides, to find the side;
– for at rectangle, given the area and either the sum of or the difference between the sides, to find the sides;
– still for a rectangle, given the diagonal and the area, to find the sides;
– to find a rectangle where the sum of length and width equals the area;
– for two squares, given the sum of or the difference between the areas together with the sum of or the difference between the sides, to find the sides;
– for a circle, to find the perimeter or diameter from the sum of perimeter, diameter and area;
– and a few more.

These riddles appear to have been invented in a Near Eastern lay surveyors' environment around the outgoing third millennium. Their first manifestation in written culture is in the "algebra" of the Old Babylonian scribe school, which expands their scope immensely. With the collapse of the Old Babylonian culture around 1600 BCE, this sophisticated discipline disappears, but the original riddles turn up in the late Babylonian period, perhaps in the fifth century BCE. In written sources from the Seleucid period (the third and second century BCE) some further riddles are added, for instance to find the sides of a rectangle from the area and the sum of length, width and diagonal. In these texts, the solution of rectangle problems goes via the sum and the difference between the sides – until then, their half-sum and half-difference (in other words, the average side and the deviations from the average) were used. At this stage and in this characteristic form, (some of) the riddles also turn up in Demotic papyri[18] The geometrical section of Māhāvīra's ninth-century *Gaṇita-sāra-saṅgraha* shows that both the original and the Seleucid-Demotic version of the riddles had reached India [Høyrup 2004] ⟦= article I.4⟧ – apparently in separate waves.

Both versions also had an impact in Mediterranean classical Antiquity. Most of Euclid's *Elements* II is a "critique" of the traditional solutions (in the Kantian sense – investigating *on which conditions* and *to which extent* they are true); Diophantos' *Arithmetica* I.27–30 are pure-number versions of traditional riddles; and chapter 24 of the pseudo-Heronic *Geometrica* contains the four-sides-and-area riddle about a square

[17] See, for instance, [Høyrup 2001] ⟦= article I.3⟧.

[18] Concerning the Seleucid-Demotic period, see for instance [Høyrup 2002].

[ed. Heiberg 1912: 418].[19] All of these build on the original riddles and methods; but certain problems in the Latin *Liber podismi* [ed. Bubnov 1899: 511*f*], which according to its title must be based on a Greek original (πόδισμός means "mensuration in feet"), and also those in the Papyrus graecus genevensis 259 [ed. Sesiano 1999] descend from the Seleucid-Demotic type [Høyrup 2002: 21*f*].

Geometrica 24 also contains pure-number problems (discussed by Jacques Sesiano [1998]) which betray some kind of inspiration from these geometric riddles; probably they are witnesses of that kind of "theoretical arithmetic" which we know best from Diophantos, and therefore constitute evidence that this discipline had some of its ultimate roots in the geometric riddles, though rather in the questions of the riddles than in the methods used to solve them.

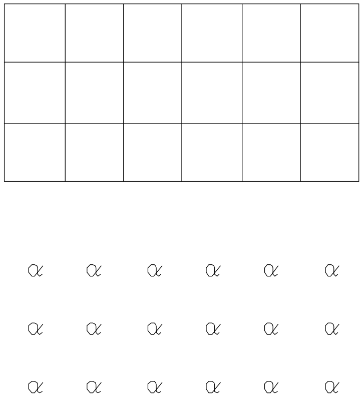

Figure 1. 6×3 represented as a surface and as a surface number.

Once again, I know of *one* piece of mathematical knowledge in the "book according to the film" which points to these geometrical riddles. However, it occurs several times.

Firstly, Plutarch has the following in *Isis et Osiris,* chapter 42[20],

> The Egyptians have a legend that the end of Osiris's life came on the seventeenth of the month, on which day it is quite evident to the eye that the period of the full moon is over. Because of this the Pythagoreans call this day "the Barrier", and utterly abominate this number. For the number seventeen, coming in between the square sixteen and the oblong rectangle eighteen, which, as it happens, are the only plane figures that have their perimeters equal to their areas, bars them off from each other and disjoins them, and breaks up the ratio of eight to eight and an eighth [$8 : 9 = 8 : 8\cdot\left(1+{}^1/_8\right)$, the whole tone/JH] by its division into unequal intervals.

The text speaks of surface numbers, not surfaces, which might make us believe that it refers to a representation of numbers by means of of ψῆφoi, calculi. If so, however, the counting of the total number of calculi and of those on the perimeter is certainly meaningful – but the statement is false, cf. Figure 1, bottom. It is only true if rectangular

[19] Chapter 24 is actually an independent treatise (or rather a conglomerate of several independent problem collections), which happens to be in the same composite codex as one of the main components of what Heiberg put together as *Geometrica* but is well separated from it. See [Høyrup 1997: 92*f*] ⟦= article I.9⟧.

[20] Ed., trans. [Babbitt 1936: 101–103]. This passage must be the one to which Thomas Heath [1921: I, 96] tells to have found a reference in a letter from Sluse to Huygens without being able to locate it.

areas and their sides are thought of. There is thus no doubt that both Plutarch and those Pythagoreans whom he refers to thought of the upper configuration and its square counterpart.

Secondly, there are two references to the equality of square perimeter and area in the *Theologumena arithmeticae* (II.11 and IV.29)[21] – once under the dyad and once under the tetrad. Under the dyad, the fact that in smaller squares the perimeter is larger than the area and in larger squares it is smaller explains why 16 is "a sort of mean between larger and lesser"; the second, taken over from the mid–third-century bishop and computist Anatolios of Alexandria, explains that 4 "is called 'justice', since the square which is based on it is equal to the perimeter"; both observations refer to themes that fit early as well as later Pythagorean currents – and, in general, fit the metaphorical use of mathematics in the service of (social) Wisdom. So does Plutarch's account of the matter. The observation might thus have been borrowed already in Plato's times or before (it does not ask for that level of mathematical competence which Thymaridas must have possessed, and which is evident in Archytas's discussion of the various means in Fragment 2[22]). But it may also have been borrowed much later.

Summations until 10

A third cluster, first known from Seleucid and Demotic sources, is constituted by summations of various series. In the tablet AO 6484[23] (a mixed anthology text from the early second century BCE), we find among other things two summations "from 1 to 10". Obv. 1–2 finds $1+2+\ldots+2^9$, while obv. 3–4 determines $1+4+\ldots+10^2$. The latter summation is solved as

$$Q_{10} = \sum_{i=1}^{10} i^2 = (1\cdot\tfrac{1}{3} + 10\cdot\tfrac{2}{3})\cdot 55,$$

a special case of the formula

$$Q_\mathrm{n} = \sum_{i=1}^{n} i^2 = (1\cdot\tfrac{1}{3} + n\cdot\tfrac{2}{3})\cdot T_n, \quad \text{where} \quad T_n = \sum_{i=1}^{n} i\ .$$

The determination of the factor $1\cdot\frac{1}{3}+n\cdot\frac{2}{3}$ is described in detail; unless we assume gross stylistic inhomogeneity, the unexplained number 55 must therefore have been found as T_{10} in an earlier problem of the original thematic text from which the anthology borrowed the two summations.

This is mathematically impressive, but totally isolated within the cuneiform tradition. The idea of taking precisely 10 members in both cases might therefore be a quirk of the author, or it might agree with a more general pattern.

21 Ed. [de Falco 1975: 11^{11-13}, 29^{6-10}], trans. [Waterfield 1988: 44, 63].

22 Ed. [Diels 1912: 333*f*].

23 Ed. Neugebauer in [MKT I, 96–99].

However, the Demotic P. British Museum 10520[24] (probably of early Roman date) is helpful. In direct translation it says that "1 is filled up twice to 10"; as the numbers show, this refers to the sums

$$T_{10} = \sum_{i=1}^{10} i \quad \text{and} \quad P_{10} = \sum_{i=1}^{10} T_i \ .$$

The answers given correspond to the (correct) formulae

$$T_n = \frac{n^2+n}{2} \quad \text{and} \quad P_n = (\frac{n+2}{3}) \cdot (\frac{n^2+n}{2}) \ .$$

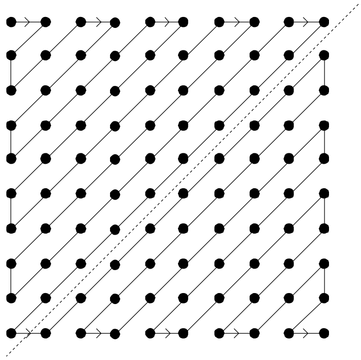

Figure 2. 10×10 arranged as a "race-course".

This does not overlap with the series dealt with in AO 6484, but the four summations are sufficiently close in style to be reckoned as members of a single cluster. Moreover, the formula for T_{10} is just what (as argued) must have been in the theme text on which the Seleucid anthology text is based, and the Seleucid formula for Q_n follows from the Demotic formula for P_n when combined with the observation that $i^2 = T_i + T_{i-1}$.

In the formulae for T_n, P_n and Q_n it is noteworthy that the latter two are expressed in terms of the former (represented by the number 55); also worth noticing is that T_n is *not* found as the product of mean value and number of terms, as normal in most mathematical cultures.

In modern symbolism, the formula is easily derived from the identity $n^2 = T_n + T_{n-1}$, from which follows

$$n^2 + n = T_n + T_{n-1} + n = T_n + T_n, \text{ and thus } T_n = \frac{1}{2} \cdot (n^2 + n) \ .$$

This was evidently not the way things were expressed in Antiquity, but the structure corresponds to an observation made by Iamblichos in his commentary to Nicomachos's *Introduction*[25] – that 10×10 laid out as a square and counted "in horse-race" as shown in Figure 2 shows that 10×10 = (1+2+…+9)+10+(9+…+2+1), whence 10×10+10 = $2T_{10}$.

Exceptional as the formula is in the general historical record, it is fairly certain that the Neopythagorean observation and the Seleucid-Demotic formulae are linked. Since both the Seleucid and the Demotic text postdate Euclid, they *could* prima facie have borrowed a result obtained by early Greek arithmeticians (perhaps Pythagoreans, perhaps

[24] Ed., trans. [Parker 1972].

[25] Ed. [Pistelli 1975: 75^{25-27}], cf. [Heath 1921: 113*f*].

The diagram described by Iamblichos is used also by several modern historians – thus by J. Dupuis in his edition of Theon of Smyrna's *Expositio* [1892: 69 n. 14] and by Ivor Bulmer-Thomas in a commentary to an excerpt from Nicomachos [Thomas 1939: 96 n. a].

not). However, the same texts contain nothing else which might remind of Greek theoretical mathematics, which speaks against a borrowing of just these summation formulae, in particular because this very selective adoption should have happened both in Egypt and in Mesopotamia.

There is a further reason to doubt a Greek invention. The determination of

$$Q_{10} = 1^2 + 2^2 + \ldots + 10^2 \quad \text{as} \quad \left(1 \cdot \frac{1}{3} + 10 \cdot \frac{2}{3}\right) \cdot \sum_{i=1}^{10} i$$

also turns up in the *Theologumena arithmeticae* (X.64, ed. [de Falco 1975: 86], trans. [Waterfield 1988: 115]), in another quotation from Anatolios (in a passage dealing with the many wonderful properties of the number 55). Anatolios, however, gives the sum in abbreviated form, as "sevenfold" T_{10}, that is, in a form from which the correct Seleucid formula cannot be derived; this in itself does not prove that earlier Greek arithmeticians did not know better; but at least it shows that the Seleucid-Demotic cluster cannot derive from the form in which the formula was known to Anatolios. In addition, the absence of the formula from any earlier Greek source derived from the theoretical or Pythagorean tradition (including Theon of Smyrna and Nicomachos) suggests that the learned Anatolios has picked it up elsewhere.

The shape of the summation formulae in combination with the reference to the race-course arrangement points with high certainty toward a derivation or proof based on ψῆφοι. If we base ourselves on the axiom that only Greek and Greek-inspired mathematics can have been based on (even heuristic) proofs and that everything else has been "empirical", then we may still conclude that the formulae *must* be of Greek origin, in spite of contrary evidence. Without this prejudice or axiom, the evidence instead suggests that (heuristic) proofs based on pebbles were no Pythagorean or otherwise Greek invention. Instead the technique will have been part of the heritage which the Greeks adopted from the Near East; most plausibly, the source was that practitioners' melting pot of which the various shared themes and formulae of Seleucid and Demotic mathematics bear witness. Since Epicharmos Fragment B 2[26] refers to the representation of an odd number ("or, for that matter, an even number") by a collection of ψῆφοι as something trivially familiar, the adoption must be placed no later than the early phase of Pythagoreanism – whence it may well have been pre-Pythagorean, all reliable evidence for Pythagorean *mathematics* being later. However, there is no doubt that at some moment the representation of numbers by ψῆφοι (and, as Iamblichos shows, heuristic proofs) were taken over by Pythagoreans and other quasi-gnostics.[27]

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26 Dated no later than c. 475 BCE. Ed. [Diels 1912: I, 118*f*].

27 It may be an accident, but the ever-recurrent summation until precisely ten suggest that even the sacred Pythagorean ten could have been a borrowing; whether even its sacredness was a borrowing is a matter of guessing (my own guess being that it was not). If an accident, the coincidence must have pleased the Pythagoreans.

Ψῆφος arithmetic is known to have been used for other purposes than the summation of series – the Epicharmos fragment refers to the "doctrine of odd and even", apart from which the figurate numbers (including the summations just discussed) constitute its most conspicuolus application. The Seleucid-Demotic material suggest that even the Near Eastern predecessors of the Greeks had used it to argue about triangular and square numbers and the corresponding pyramid numbers P_n and Q_n; since these turn up together (and always together with the sum $\sum_{i=1}^{n} i^3 = T_n^2$) in Indian sources and in al-Karajī's *Fakhrī,*[28] it is a fair assumption that these were all dealt with before the borrowing took place; the absence of higher polygonal numbers from all these sources (of which the Indian sources, Āryabhaṭa as well as Bhāskara II and Brahmagupta, are more systematic than can be expected from the random surviving papyri and fragments of clay tablets) indicates that these represent further Greek explorations of the tool – explorations that did not spread eastward.

Westward they did spread, or rather an unintended repercussion. The higher polygonal numbers were taken over by the Roman agrimensors, who mistook these inhomogeneous expressions for area determinations of regular polygons.[29] No Near Eastern surveyor would have made this mistake, nor would Hero or even the less able compilers of the *Geometrica*. There is thus little doubt that they came from mistaken Greek theory (maybe a reminiscence from the teaching of Liberal-Arts arithmetic, where these numbers played a conspicuous role). On the other hand, the side of the regular polygons in the treatise mentioned in note 29 is invariably 10, which seems to be a heritage from the Near Eastern tradition – see [Høyrup 1997: 91] ⟦= article I.9.⟧ .

The higher figurate numbers play a role in the handbooks for Liberal-Arts arithmetic, but I have not noticed them in quasi-gnostic contexts. Here, only the Near Eastern heritage (perhaps in watered-down form, witness Anatolios) turns up.

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28 See [Clark 1930: 37] (Āryabhata), [Colebrooke 1817: 290–294] (Brahmagupta), [Colebrooke 1817: 51–57] (Bhāskara II), and [Woepcke 1853: 61] (*Fakhrī*).

29 For instance Epaphroditus & Vitruvius Rufus, ed. [Bubnov 1899: 534–545]. But the nonsense survived into the late medieval abbacus tradition.

Fields to be measured would hardly ever by regular pentagons, hexagons etc. – and if they were, standard measurement would only reveal them to be equilateral, not equiangular, for which reason their area would anyhow have to be found by subdivision. We may therefore safely assume that the wrong formulae were never used in practice; though no riddles, these formulae, giving an impression of completeness, were supra-utilitarian adornments.

Side-and-diagonal numbers

The last topic I shall take up in some detail is a likely borrowing from architectural geometry, at some moment transferred into a number algorithm. I refer to the "side-and-diagonal–number algorithm", an algorithm for producing increasingly precise approximations to the ratio between the diagonal and the side of a square (in anachronistic terms, to $\sqrt{2}$). The basis for this algorithm is what I shall call the side-and-diagonal rule: if s and d are the side and diagonal of a square, then the same holds for $\Sigma = s+d$ and $\Delta = 2s+d$. Experience combined with common sense shows that iteration of the process from values s_1 and d_1 which do not fulfill the condition $d_1^2 = 2s_1^2$ leads to convergence of the ratio $d_n^2{:}s_n^2$ toward 2:1. In particular, if we start from $s_1 = d_1 = 1$, we get the successive pairs 1:1, 3:2, 7:5, 17:12, 41:29, 99:70, 239:169, …; this is the side-and-diagonal *algorithm*.[30]

The algorithm is not described by any of the "great" or "genuine" mathematicians, but it was known by both Theon of Smyrna (*Expositio* I.XXXI, ed. [Dupuis 1892: 70–74] and Proclos;[31] a detailed description is found, moreover, in Iamblichos's commentary to Nicomachos [ed. Pistelli 1894: 91*f*]. We may assume it to have circulated in quasi-gnostic circles, which was part of the shared background of these three authors (and Iamblichos's principal background).

In his edition of Proclos' commentary to the *Republic*, Kroll supposed that the rule was proved by means of *Elements* II.10,[32] which he further took to be of Pythagorean origin (as a matter of fact, it is the justification of one of the old two-square riddles, thus antedating Pythagoras by more than a millennium). Another (quasi-algebraic) proof can

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30 Asymptotically, each added step reduces the error of the ratio $d{:}s$ by a factor $\frac{1}{1+\sqrt{2}}$.

31 Proclos describes it in a commentary to a passage in *Republic* 546C ([ed. Kroll 1899: II, 24*f*]; cf. discussion in [Vitrac 1990: 351*f*]); there is also an oblique but unmistakeable reference in his commentary to *Elements* I ([ed. Friedlein 1873: 427$^{21-23}$], trans. [Morrow 1970: 339]), where it is spoken of as σύνεγγυζ, "proximate".

It has been assumed that Plato's reference to "a hundred numbers determined by the rational diameters of the pempad lacking one in each case" in *Republic* 546C, trans. [Shorey 1930: II, 247]) shows him to be familiar with the same algorithm. Actually, all it shows for certain is that he was familiar with the use of 7 as an (approximate) value for the diagonal in a square with side 5.

Heath [1926: I, 399] supposes that the "lacking one" refers to the fact that 7^2 is lacking 1 compared to the square on the true (irrational) diameter in the square with side 5, which is an essential feature of the sequence of approximations produced by the algorithm. As pointed out to me by Marinus Taisbak (personal communication), Plato's point is rather that the number 48 (the number which is required) is lacking one with regard to the "number on the rational diameter 7" (and 2 with regard to that on the irrational diameter *dynámei*, as Plato goes on). This is indeed also Proclos's explanation, cf. Hultsch in [Kroll 1899: II, 407].

32 Using the letters of Figure 4: If *CB* is bisected by *G*, and prolonged by *BE*, *then* $\square(CE)+\square(BE) = 2\cdot(\square(CG)+\square(GE))$.

be based on the diagram which Leonardo Fibonacci employs in the *Pratica geometrie* [ed. Boncompagni 1862: 62] when solving the problem $\Delta - \Sigma = 6$ (see Figure 3 ⟦observe that I+II = IX⟧); by simple counting, the same diagram can also be used to prove *Elements* II.10. The proof is of a type that is familiar from the Seleucid rectangle riddles, and strong arguments can be given that the similarity is based on an actual historical link.

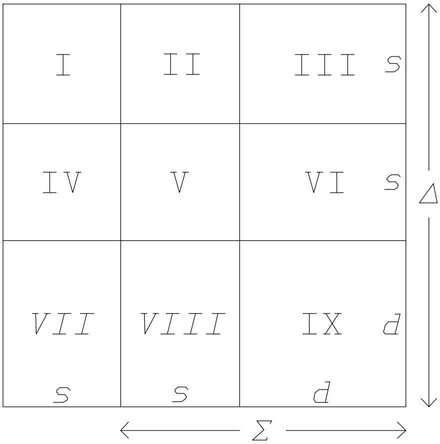

Figure 3. Fibonacci's implicit proof of the side-and-diagonal algorithm.

However, the rule can also be observed rather directly in the construction of a rectangular octagon by superposition of two identical squares (see Figure 4): if $CG = GB = DG = s$, then $CD = AC = AF = d$. Therefore $\Sigma = DJ = DG+GJ = s+d$, while the corresponding diagonal is $\Delta = FD = FC+CD$. But $FC = CB = 2s$, whence $\Delta = 2s+d$.

In the pseudo-Heronian *De mensuris* 52 [ed. Heiberg 1914: 206], a reduced version of Figure 4 is used for the octagon construction: the oblique square is omitted, but it is used that $AB = EC = AO$ (etc.). This follows from exactly the same arguments as lead to the side-and-diagonal-rule. It is difficult to believe this construction to have been invented directly, without the passage over the superimposed squares.

The reduced construction turns up again in Abū'l-Wafā' 's *Book on What is Necessary from Geometric Construction for the Artisan* VII.xxii [ed., Russian trans. Krasnova 1966: 93]; in the *Geometria incerti auctoris* no. 55 [ed. Bubnov 1899: 360*f*]; and in Mathes Roriczer's late 15th-century *Geometria deutsch* [ed. Shelby 1977: 119*f*]. Roriczer's *Wimpergbüchlein* [ed. Shelby 1977: 108*f*] makes use of the superimposed squares and shows (though this is not the topic) that Roriczer knew some of their relevant properties. The superimposed squares producing the regular octagon are found as an illustration to the

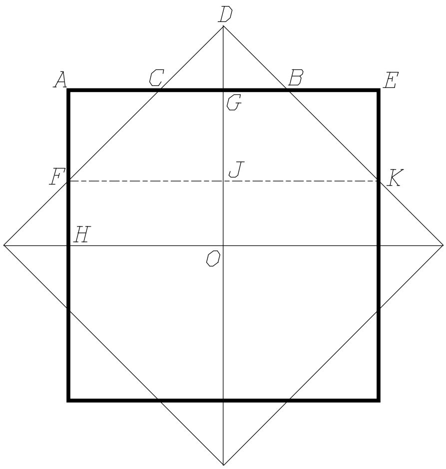

Figure 4. A regular octagon produced by superimposed squares.

determination of its area (via the octagonal number!) in Epaphroditus & Vitruvius Rufus [ed. Cantor 1875: 212, Fig. 40[33]]. As I have been told by Hermann Kienast (personal communication) they can also be seen to have been used in the ground plan of the Athenian "Tower of the Winds" from the first century BCE.[34] All in all there is thus no doubt that both constructions were known by practical geometers in the classical age; the places where we find references to the algorithm or material traces of the construction (all late and far removed from the theoretical tradition) make it unlikely that the idea *originated* among Greek theoretical mathematicians – including the Pythagorean *mathematikoi*.

The rule and even its transformation into an algorithm could be of much earlier date. Two Old Babylonian tablets (YBC 7289 and YBC 7243, in [MCT, 43, 136]) give the value 1;24,51,10 for the ratio between the square diagonal and side. In their commentary, Neugebauer and Sachs noticed that this value is the sixth step in an alternating iteration by arithmetic and harmonic means,[35] the fourth step of which is the value 1;25, which also turns up in cuneiform sources (as we now know, of Old Babylonian as well as Seleucid date). As shown by David Fowler and Eleanor Robson [1998], however, the calculations require repeated divisions by sexagesimally irregular numbers; approximation by regular divisors would lead to roundings which would either yield a result which was less or which was even more precise. This explanation can therefore be discarded; so can the iteration suggested in note 35, which suffers from the same defect.

This seems to leave us with the side-and-diagonal algorithm. Indeed, 1;24,51,10 is the rounded value of 239÷169; it can be found by a single division by an irregular number, which we know Mesopotamian scribes to have been trained to perform already before the mid-third millennium BCE. The approximation 1;25, also found in Babylonian sources as we remember, is nothing but 17÷12. This value *can* be found by the iteration of note 35 from above, starting from the value $1^1/_2$; but the plausible use of the side-and-diagonal for the better approximation speaks in favour of its use even here. *If* this is so, a possible link between the Old Babylonian (plausible) use of the rule and its certain presence in the classical world is at hand. In any case, a certain link connecting the Old Babylonian way to express the perimeter of the circle in terms of the diameter pops up again in Greek

33 The text is also in [Bubnov 1899: 539], but the diagram is omitted.

34 Vitruvius's description of how the ground plan was made (*De architectura* I.vi.4) is thus an *a posteriori* reconstruction – "rational", but wrong.

35 More likely than this alternation would be the equivalent iteration of the approximation $\sqrt{n^2+d} \approx n+\frac{d}{2n}$, which can be argued geometrically. This eliminates half of the steps from the Neugebauer-Sachs procedure, but leaves the relevant ones.

One Old Babylonian text uses this iteration from below, but none from above (which in general is much less common in the sources until the outgoing Middle Ages).

practical geometry, and finds its explanation in a construction described by Roriczer and in an Icelandic manuscript from the early 14th century (which allows to find the perimeter without calculation); it is likely to have been carried by the profession of master builders.[36] However, the reader counting the occurrences of words like "plausible", "seems" and "if' in the course of this argument will realize that it is far from compulsory – and definitely insufficient to decide with any certainty between a borrowing and independent (re)discovery, either of the construction or of the number algorithm.

We have no indication as to when the algorithm was adopted by the quasi-gnostic environment; it may have been in the age of Thymarides and the Pythagorean *mathematikói,* or much closer to Theon's late first century CE. But we cannot avoid noticing that all sources we possess for the algorithm link mathematics with Wisdom, while the evidence we have for the diagram behind it is an architectural real-life construction; if mathematicians with no esoteric affinity had once worked on the topic, they seem to have lost all interest in epochs from which sources survive.[37] None of our explicit sources – that is, neither Theon nor Proclos – show convincingly to know the "principles and causes" behind the algorithm.

A final note about fractions and ratios

I promised in note 5 to leave aside Liberal-Arts arithmetic together with its impact in the mathematics of Wisdom. I shall permit myself a slight breach of this promise, a mere reference to a publication which in my opinion by far has not received the attention it deserves: Kurt Vogel's habilitation thesis from [1936].

One of the points made by Vogel (p. 449) is that the Greek vocabulary for ratios is shaped after that for fractions. For reasons I shall not discuss here, the Euclidean (but not the Diophantine) brand of theoretical arithmetic as well as the arithmetic of Liberal-Arts handbooks avoided fractions.[38] Instead, as we know, theoretical Greek mathematics

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[36] See [Høyrup 2009: 368–370] ⟦= article II.11⟧.

[37] There is one just possible impact on the theoretical tradition: the proof of *Elements* II.10, the diagram of which is nothing but the section of Figure 4 designated by the letters *KEBGCDD* (but in the general case without the specific ratio between *GB* and *BE*). The proposition states that $\square CE+\square BE = 2(\square EG+\square BG)$, which is obviously fulfilled when $\square BE = 2\square BG$, $\square CE = 2EG$, as happens in the case of the superimposed squares. Whereas the proofs of *Elements* II.1–8 all correspond to the techniques by which the rectangle riddles were solved already in the Old Babylonian epoch, those of II.10 and the closely related II.9 are of a wholly different kind.

Isolated as that similarity is, the preceding observation can be nothing but a suggestion. Euclid and his predecessors were certainly able to devise their own diagrams as they needed. Only the association with other proofs borrowed from the tradition supports the suggestion.

[38] The avoidance may have to do, both with the fateful answer "a collection of units" once given to the question "what is a number", and (in Plato's case, according to the curriculum passage of *Republic* VII) with the use of fractions by petty traders. A supplementary stimulus for interest in

had recourse to ratios, and a large part of Liberal-Arts arithmetic is dedicated to the classification and naming of ratios – an interest which is also visible in some of the quasi-gnostic writings (first of all of course in Nicomachos's *Introduction*, if we count that, next in Iamblichos's commentary to this work). The whole apparatus built up around this classification was quite adequate for those who felt attracted to the easy "royal road" to mathematics – in particular when it was taught exclusively through numerical examples and without even paradigmatic proofs built on single cases. Ultimately, this is another case of mathematics coming from base practice and taken over as "wisdom". With the difference, however, that only the transposition to ratios called for the creation of the classification system – for fractions most of it would have been obviously superfluous.

Apart from this, however, that "royal road" to mathematical Wisdom whose existence Euclid denied (as Proclos's story goes) was in part paved with material borrowed from those who designed common roads or moved their merchandise along them – but borrowed piecemeal, mostly as bits without coherence and out of context. The internal coherence of quasi-gnostic mathematics, to the extent it can be seen to have possessed one, was probably provided by arithmology and by the interests it shared with Liberal-Arts mathematics.

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Chapter 12 (Article I.11)
The Rare Traces of Constructional Procedures in “Practical Geometries”

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧
Translations, if not otherwise identified, are mine

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Abstract

From a sociological point of view, pre-Modern "non-theoretical geometry" is not adequately described as merely "practical". The "practical geometry" we find in written treatises is mostly that of "scribal" environments, and aims at *calculating* lengths, areas or volumes from already performed measurements. As a rule it is not interested in geometrical construction, nor in the making of measurements – tasks taken care of, broadly speaking, by master builders/architects and surveyors.

The paper discusses two cases – one fairly well-established, another more conjectural – where nonetheless "scribal" practical geometry does reveal traces of (very simple) geometrical construction. Both of these concern the "long run", connecting Old Babylonian, classical ancient and late medieval material. A final instance of weak communication between "scribal" and "surveying" geometry is located in 13th-14th-century France.

Contents

“Practical” and constructing geometry

In a customary dichotomy, geometry (like many other fields, mathematical as well as non-mathematical) falls into “theoretical” and “practical”. In full agreement with this, Stephen K. Victor [1979: ix] writes about his Ph.D.-project that

> My first assumption, and that of most of the people I have spoken to about the topic, was that “practical geometry” must relate somehow to architecture, surveying and city planning, to those areas, in other words, where geometry plays a central role in the exercise of other professions. The study of medieval buildings, fields and towns from extant physical evidence was not a fruitful approach for me, and I have left it to those better trained in the methods of archaeology and art history. Since I was working as a historian of science, I chose to concentrate on the written tradition of treatises called “practical geometry”.

The treatises he chose to work on – the Latin late-12th–century *Artis cuiuslibet consummatio* and a vernacular (Picardian) *Pratike de geometrie* from the late 13th century which is largely a translation of the former work – led him to a different view,

> namely that practical geometry has its greatest importance as a popularization of mathematics. The treatises on practical geometry were a way of teaching some basic principles to those who would not remain in school or university long enough to become philosophers or theologians and would not necessarily exercise a mathematical profession, but who might want, or even need, some mathematics in their everyday lives. The sampling of arithmetic and astronomy in ACC and of commercial arithmetic and metrology in the Pratike argues for the generally pedagogic, rather than scholastic, purpose of the treatises. The development of a vernacular version of ACC is further evidence that the practical geometries sought their homes outside of the universities, perhaps in the bureaucratic and commercial milieus. Nonetheless, as the Introduction shows, the formalized structures of university education had an influence even on the non-scholastic tradition of practical geometry. As the tradition developed, the practical geometries acquired an increasingly theoretical underpinning, to the point where they are sometimes considered works on measurement rather than simply practical geometries.

Part of this conclusion depends critically on the choice of a Latin treatise and a vernacular treatise which in the main was derived from it. Other features, however, are shared not only with the Italian vernacular *Pratiche di geometria* and with Fibonacci’s *Pratica geometrie* but also with most Arabic,[1] Sanskrit, Chinese, Greek, Babylonian and ancient Egyptian writings on the subject-matter. They deal, not at all or not much with *measurement*, as Victor states euphemistically, but rather with *how to calculate* something on the basis of measurements that have already been performed or which are presupposed to have been performed, either on pre-existing objects or on configurations which are

[1] An Arabic exception to this rule is Abū’l-Wafa ’ ’s *Book on What is Necessary for Artisans in Geometrical Construction* [ed., Russian trans. Krasnova 1966].

supposed to have been already constructed.[2] In general terms, they belong within the "scribal" sphere, or in Victor's words, the "bureaucratic and commercial milieus".

Nonetheless, a few traces of constructional procedures hide within these texts. I shall present two instances, one fairly certain and the other not much more than suggestive.

Constructing the circular perimeter

In *Metrica* I.xxx [ed. Schöne 1903: 74] Heron explains that "the ancients" – οἱ ἀρχαῖοι – in their formula for the area of a circular segment seem to have "followed those who took the perimeter to encompass the triple (τριπλάσιος) of the diameter", whereas I.xxxi (*ibid.* p. 74) states that "those who made more precise investigations" must have followed the course according to which the perimeter is the triple diameter and in addition $^1/_7$ of the diameter.

Heron himself teaches (I.xxvi, *ibid. p.* 66) to multiply the perimeter by 22 (using the construction "22 ἐπὶ") and then to take the seventh, while the pseudo-Heronic *Geometrica* [ed. Heiberg 1912][3] – throughout using the "more precise" variant – invariably takes the diameter "thrice" or "tripled", and calculates this triple explicitly, after which a supplementary seventh is added. The terms for tripling are invariably τρισσάκις or τριπλάσιον even when neighbouring multiplications are ἐπὶ *n*.[4]

The same terminological distinction between tripling and multiplication is found already in Old Babylonian geometry (c. 1700 BCE). Here, the perimeter is always found as the diameter "repeated until three" (*ana* 3 *eṣēpum*), or it is "tripled" (*šalāšum*). It is not calculated by means of the normal multiplication (*našûm,* "to raise") used, e.g., when the area of the circle is found as 0;5 (= $^5/_{60}$ = $^1/_{12}$) times the square on the perimeter.

The explanation for this linguistic puzzle is found in two texts from the 14th and the 15th century (CE). One is Mathes Roriczer's *Geometria deutsch* from c. 1488 [Shelby 1977: 121]:

> If anyone wishes to make a circular line straight, so that the straight line and the circular are the same length, then make three circles next to one another, and divide the first circle into seven equal parts,

[2] Actually, the genre studied by Victor – Latin practical geometries such as *Geometria incerti auctoris* and Hugh of Saint Victor's *Practica* – deals to some extent with mensuration, namely the determination of (e.g.) inaccessible heights by means of equilateral right triangles. This also had a slight (very slight!) impact on Italian abbacus geometries.

[3] Definitely *not* Heronic, and actually a composite created by the modern editor from two rather incompatible manuscript groups, respectively A+C and S+V, as Heiberg [1914: xxi] points out.

[4] Thus mss AC, **17**.8, between **17**.7 and **17**.9, and ms. S, **17**.6, after **17**.6 [ed. Heiberg 1912: 336, 334].

one of which is marked out in continuation of the three circles – see Figure 1, ⟦from [Rorizcer 1497]⟧.[5]

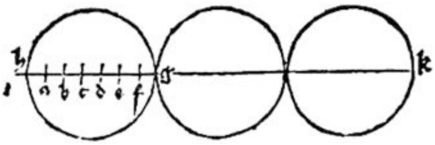

Figure 1. Roriczer's construction of the circular perimeter.

The other is the Old Icelandic manuscript A.M. 415 4to from the early 14th century, according to which (fol. 9v) "the measure around the circle is three times longer as its width, and a seventh of the fourth width",[6] obviously a reference to a similar construction.

Roriczer was a Gothic master builder; what he tells is the way to find by means of a drawing, *without calculation*, the perimeter corresponding to a given circular diameter. The Icelandic text confirms that the method was widespread; there seems to be little doubt that it offers the explanation why both the Greek and the Old Babylonian text refer to a tripling, a material repetition, and not to a mere numerical multiplication. This trick had thus been known for more than three thousand years in the late Middle Ages, first as a simple tripling, after the acceptance of the Archimedean improvement with an addition of an extra seventh – still a separate supplement, and still to be provided in physical space.

The regular octagon and the side-and-diagonal numbers

The other example is differently balanced, in the sense that the traces in the calculational material are fewer but those in other sources more copious.

One trace is constituted by the Old Babylonian approximations to the ratio between the diagonal and the side of a square. One, already quite good, is 1;25 = $^{17}/_{12}$; the other,

5 Shelby [1977: 182] observes "some resemblance between [Roriczer's procedure] and one of the theorems in a brief *Tractatus de quadratura circuli* – traditionally attributed to Campanus de Novara, but authorship and date uncertain". The passage in question [ed. Clagett 1964: 591] deals with how to "give a straight line equal to a circularly drawn line", and runs as follows:

> Using mathematical knowledge and physical truth, a circle is divided into 22 equal parts, and with one part subtracted, that is, the 22nd part, a third of the remainder, namely, 7, is the diameter of the circle. Therefore, let the diameter be tripled and let there be added a seventh of the diameter, and let these parts be ordered in a straight line. We shall have a straight line equal to a circular line, as is apparent in the figure.

This could well be an attempted "theoretical" explanation of Roriczer's construction, but since the diagram shows a circle divided into 22 parts (with a diameter prolonged indefinitely toward the right) it could at least as well be a justification of the *calculation* found in the *Geometrica* and writings of the same kind, like that 15th-century *De inquisicione capacitatis figurarum* to which Shelby [1977: 6–65] refers in his introduction.

6 "Ummæling hrings hvers Þrimr lutum lengri en bréidd hans ok sjaundungr of enni fiorðo breidd" [ed. Beckman & Kålund 1914: 231*f*]. I am grateful to Peter Springborg for localizing a passage which is quoted without reference by Menninger [1957: I, 91] and for providing me with a photocopy from the microfilm in the Arnamagnean collection, Copenhagen.

excellent, is 1;24,51,10. The former may have been found by iteration of a procedure also known from elsewhere in the Old Babylonian record, corresponding to the formula

$$\sqrt{n^2+d} = n + \frac{d}{2n}$$

– actually, the text VAT 6598 contains what may be a failed attempt at such iteration [Høyrup 2002: 271*f*]. The latter can be found *by us* by further iteration, but hardly by the Babylonians: as pointed out by David Fowler and Eleanor Robson [1998], the calculations have to pass through repeated divisions by very unpleasant sexagesimally irregular numbers; if we try to approximate by regular divisors (in agreement with what we know about Babylonian computational techniques), the reconstruction no longer yields the approximation it should but either one which is too rough or one which is even better.

Neugebauer and Sachs [1945: 43] proposed a different way to the same approximations, namely through alternating arithmetical and harmonic means. Algebraically, this gives the same results – and computationally it runs into the same problems.

A third possibility – also algebraically equivalent – is the use of the "side-and-diagonal-number algorithm",

$$s_1 = d_1 = 1, \quad s_{n+1} = s_n + d_n, \quad d_{n+1} = 2s_n + d_n.$$

The value of $2s^2 – d^2$ oscillates between –1 (for odd n) and +1 (for even n). Since s and d increase, the ratio $d:s$ therefore converges toward $\sqrt{2}$.

The procedure is first described by Theon of Smyrna (*Expositio* I.XXXI, [ed. trans. Dupuis 1892: 70–74]), but according to his own statement in agreement with Pythagorean traditions without any addition whatsoever (book II, the introduction). It is also habitually assumed that Plato's reference to "a hundred numbers determined by the rational diameters of the pempad lacking one in each case" (*Republic* 546C, [ed. trans. Shorey 1930: II, 247]) shows him to be familiar with the same algorithm. Actually, all it shows for certain is that he was familiar with the use of 7 as an (approximate) value for the diagonal in a square with side 5.[7] In any case, another discussion of the algorithm is found in Proclos's commentary to the passage in question from the *Republic*[8]. Finally, Proclos's

7 Heath [1926: I, 399] and others read the "lacking one" as a reference to the fact that 7^2 is lacking 1 compared to the square on the true (irrational) diameter in the square with side 5, which corresponds to an essential feature of the sequence of approximations produced by the algorithm. Actually, as pointed out to me by Marinus Taisbak (personal communication), Plato's point is rather that the number 48 (the number which is required) is lacking one with regard to the "number on the rational diameter 7" (and 2 with regard to that on the irrational diameter *dynamei,* as Plato goes on). This is indeed also Proclos's explanation, cf. Hultsch in [Kroll 1899: II, 407].

8 Ed. [Kroll 1899: II, 24*f*]; cf. discussion in [Vitrac 1990: 351*f*].

commentary to *Elements* I contains an oblique but unmistakeable reference to the topic[9] and speaks of it as σύνεγγυζ, "proximate". ⟦[10]⟧

Though moderately to quite competent in mathematics, both Theon and Proclos have affinities to the environment which took mathematics as a way to or a kind of *gnosis* – in very loose terms, the Neopythagorean-Platonizing ambience ⟦Iamblichos, of course, is a central figure⟧. As I have discussed elsewhere [Høyrup 2001], this ambience, being unable to follow mathematics at the Euclidean or Archimedean level, borrowed its mathematics from the practitioners' level. Since no word about the algorithm has reached us from the ancient Greek high-level mathematicians, it seems reasonable to look for the roots of the procedure in some practitioners' environment.

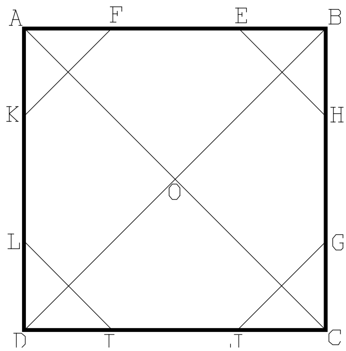

Figure 2. The construction of a regular octagon according to *De mensuris*.

The algorithm does not turn up as such in "mensuration" treatises, but the pseudo-Heronic *De mensuris* [ed. Heiberg 1914: 206] prescribes a construction of a regular octagon (under the misleading heading "mensuration of an octagon") which suggests the reasoning that may have led to its invention. In a square *ABCD,* the corners of the octagon *FEHGJILK* are found by making *AE* = *BF* = *BG* = *CH* = … = *AO* – see Figure 2.

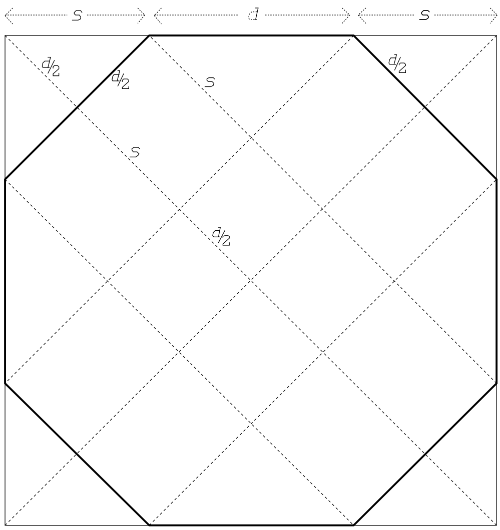

Figure 3. A diagram showing why the *De mensuris* construction works.

Figure 3 explains the correctness of the construction; the very same argument shows what we might call the "side-and-diagonal

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9 Ed. [Friedlein 1873: $427^{21–23}$], trans. [Morrow 1970: 339].

10 ⟦A very precise description of the algorithm can also be found in Iamblichos's *Introduction to Nicomachos's Arithmetic* (Προτρεπτικος) [ed. trans. Romano 2012: 752–755]⟧.

rule": namely that if s and d are the side and diagonal of a square, so will $s+d$ and $2s+d$ be.

The same construction is found in several other sources: in Abū'l-Wafā' 's *Book on What is Necessary from Geometric Construction for the Artisan* as problem VII.xxii [trans. Krasnova 1966: 93]; in the *Geometria incerti auctoris* no. 55 [ed. Bubnov 1899: 360*f*]; in Roriczer's 15th-century *Geometria deutsch* [ed. Shelby 1977: 119*f*]; and in Serlio's *Primo libro di geometria* [1584: C2^r]. However, it is difficult to believe that anyone would get the idea to draw this diagram if the construction was not known already; and indeed, a much more intuitive diagram can be drawn, of which Figure 2 is simply a reduced version – namely the one shown in Figure 4. For symmetry reasons it is intuitively obvious that the superposition of two identical squares of which one is tilted 45° produces a regular octagon; but if we look at the diagram we also observe that $FR = RE = RP = KV = UO;$ this length we may call $s;$ then the corresponding diagonal is $d = PF = AF = AK = RU$. Therefore, the semidiagonal PO is $s+d+s = 2s+d,$ thus equal to AE. Furthermore, since $KF = FE = 2s,$ $UP = s+d$ and $KP = 2s+d$ are, respectively, the side and diagonal of a square – that is, the argument that shows the correctness of the *De mensuris* construction from this diagram also leads to the side-and-diagonal rule.

This construction was employed in actual architecture at least in Classical Antiquity: according to Hermann Kienast (personal communication) it can be seen to have been used in the ground plan of the Athenian "Tower of the Winds" from the first century BCE (outside the octagon itself, the point P is marked).[11] The superimposed squares producing the regular octagon are also found as an illustration to the determination of its area in Epaphroditus & Vitruvius Rufus [ed. Cantor 1875: 212, Fig. 40[12]]. Since the area is found from the octagonal number, this (as well as any other) geometrical construction is irrelevant to the calculation; it can only be there because it was familiar. Finally, Roriczer's *Wimpergbüchlein* [ed. trans. Shelby 1977: 108*f*] makes use of the configuration.[13]

The conclusions to be drawn from this are somewhat shaky. It appears that the construction of the octagon, both by means of superimposed squares and via the simpler

11 The construction described by Vitruvius in *De architectura* I.vi.6–7 [ed. trans. Granger 1931: I, 58–61] is thus a (mistaken) reconstruction, explaining only how Vitruvius thought the construction *could* be made.

12 The text is also in Bubnov's edition [1899: 539], but the diagram is omitted.

13 Cantor [1907: 108] refers to the superimposed squares as common in Pharaonic wall painting, but this can hardly be considered as evidence, neither for use in actual architecture nor for mathematical reasoning based on it. But at least is shows the idea to be near at hand.

The several apparently regular octagons in Villard de Honnecourt's sketchbook [ed. Hahnloser 1935: Taf. 18, 63] are not accompanied by verbal or geometric indications as to how they were constructed. Only familiarity with Roriczer's description allows us to surmise that Villard's specimens were made in the same way; they cannot count as independent evidence.

diagram of Figure 2, was known in Classical Antiquity and by late medieval Gothic masterbuilders; it is near at hand to assume some kind of continuity. In the absence of better explanation it is also tempting to presume that the side-and-diagonal algorithm was inspired by one or the other of these constructions. Equally in the absence of better explanations, it is tempting to conjecture that the same algorithm was used by Old Babylonian calculators, and that even they had come to know it in this way (nothing neither excluding nor guaranteeing that the Classical knowledge of the algorithm was due to independent discovery).

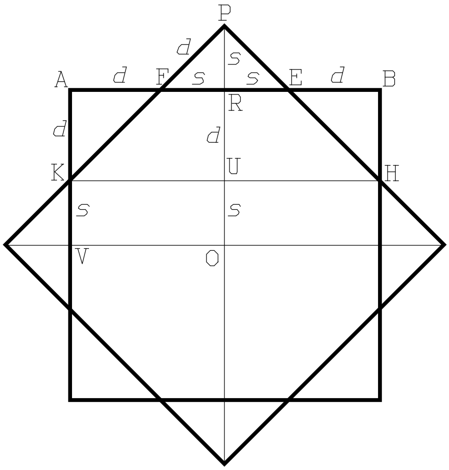

Figure 4. The completed version of Figure 2.

Concluding observations

Fairly broad reading of writings on practical “mensuration” from a variety of pre-Modern cultures have thus permitted me to locate one rather certain instance of inspiration from a (very simple) *construction*, and one more dubious case. Even in this field it is confirmed that “practitioners’ knowledge” was not unspecific “folk” but specialists’ knowledge, and that specialists belonged to distinct cultures with little mutual communication.

As an illustration of the rarity of such communication I shall mention one instance, albeit rather of communication between “scribes” and surveyors than between “scribes” and constructors. In the introductory remarks I mentioned that the vernacular *Pratike de geometrie* was largely a translation of the Latin *Artis cuiuslibet consummatio.* However, on one point it is not (in fact on several points, but only this one concerns us here). The *Artis cuiuslibet consummatio* I.15 [ed. trans. Victor 1979: 158–160] finds the area of an equilateral pentagon as the corresponding pentagonal number – in agreement with the agrimensorial tradition, and in spite of Gerbert’s explanation of the fallacy in the triangular case [ed. Bubnov 1899: 45], even though this same explanation is reported in chapter I.2 [ed. trans. Victor 1979: 130]. In contrast, the “*Pratike* (*ibid.* p. 489) suggests to multiply each side by half the height (which must be supposed to be measured, since no value is told) and to add the five partial areas afterwards.

A very similar procedure is proposed in the treatise *Geometrie due sunt partes principales* [ed. Hahn 1982: 155], whose earliest manuscript also dates from the 13th century. Here, for any regular polygon, it is proposed to construct the perpendicular

bisector of each side, to see where they meet, and measure the heights – etc. Finally, the *Trattato di tutta l'arte dell'abacho,* written in 1334 in Tuscan language but in Montpellier and under obvious Provençal influence,[14] gives an alternative "by geometry" to a corrupt version of the "arithmetical" computation by means of the pentagonal number. This alternative looks as a mixture of the two Latin prescriptions – which can only mean that all three texts share a common background where scribal "mensuration" had contact with real mensuration, probably in French vernacular culture.

I know of no evidence beyond these three passages for the character of this point of contact, and it is much of an accident that I noticed them. Other evidence for interaction between different geometrical cultures of the time may be hidden in odd corners of manuscripts and wait for detection. On the other hand, the very possibility of hiding shows that such contacts were exceptions: on the whole, the pre-Modern geometrical cultures of scribal administrators, surveyors and master builders were as isolated from each other as, say, the professional cultures of dentists, air traffic controllers and public relation experts nowadays.

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Chapter 13 (Article I.12) About the Italian Background to *Rechenmeister* Mathematics

Contribution to the colloquium
DIE RECHENMEISTER IN DER RENAISSANCE UND DER FRÜHEN NEUZEIT
STAND DER FORSCHUNG UND PERSPEKTIVEN
Munich, Deutsches Museum, 29 February 2008

Original title
"Über den italienischen Hintergrund der Rechenmeister-Mathematik"
Here translated from the German
A few (mostly explanatory) additions and cross-references in ⟦…⟧

J. Høyrup, *Selected Essays on Pre- and Early Modern Mathematical Practice*,
https://doi.org/10.1007/978-3-030-19258-7_13

Abstract

See “Introduction”.

Contents

OTTO NEUGEBAUER in memoriam

Introduction

Whoever has dealt with abbacus- or *Rechenmeister* mathematics will know this piece of conventional wisdom – or at least the rough outline of this (already roughly drawn) story:

- in 1202, Leonardo Fibonacci wrote his *Liber abbaci*, with a revised version from 1228; and in 1220 his *Pratica geometrie*.
- After c. 1260, the former, or both, gave rise to the emergence of the abbacus schools in Italian commercial towns.
- In the early 14th century, Italian abbacus teachers went to Provence, where they taught the new Italian techniques for computation.
- From here, a new Provençal school arose, whose traces we see in Nicolas Chuquet and Francès Pellos and in Francesc Santcliment's *Summa*.
- Starting in the mid-15th century, even mathematicians from the southern German area (university scholars, but not only) learned from the Italians. They also created an algebraic formalism.

As every piece of conventional wisdom, even this one carries some truth – but also, at closer inspection, half-truth and mistake. In what follows I shall try to distinguish these categories, with particular emphasis on my investigations of early abbacus mathematics, on the development of abbacus algebra in the 14th and 15th centuries, and on the Italian traces that can be discovered in early *Rechenmeister* algebra.

The *Liber abbaci*

Fibonacci declares in the preface to the *Liber abbaci* [ed. Boncompagni 1857: 1] that after the "some days" of "instruction in the abbacus" he had received in Bejaïa during his boyhood, he continued while on commercial travels[1] to "Egypt, Syria, Greece, Sicily and Provence" the study of the "nine figures of the Indians" – and, one may presume, the appurtenant applications.[2] Nobody, as far as I know, has asked the question what

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[1] That almost all manuscripts speak of "commercial travels" – and not only (as does the one used by Boncompagni) of travels to "locales of commerce" – has been shown by Richard Grimm [1976: 101*f*]. Substantially there is hardly any difference: Why should Fibonacci identify Egypt and Syria as "locales of commerce" if his reason to go there had not been to trade? At the time (time of crusades), both places were better known for quite different things.

[2] Here, as everywhere in the following, translations with no identified translator are mine. As usually, I try to translate precisely rather than beautifully. I leave it, for example, to the original and not to my own stylistic feeling whether a number standing as subject takes the

Fibonacci could learn about the topic in Provence. Fibonacci himself speaks about problems with which he has been confronted in Byzantium [ed. Boncompagni 1857: 188, 190, 249]; they all belong to types that can also be found in the Arabic Mediterranean, and which are encountered later on in the abbacus books.

Fibonacci says that his *regula recta*[3] is of Arabic origin, and that the name *regula elchatayn* (the rule of double false) is Arabic [ed. Boncompagni 1857: 191, 318]. The *regula recta* is certainly identical with the *regula* of the *Liber augmenti et diminutionis* [ed. Libri 1838: I, 304–371, *passim*]. Fibonacci's number schemes contained within a rectangular frame are already present in the *Liber mahameleth,* compiled or translated in Toledan environment around 1160 and apparently unknown to Fibonacci;⟦[4]⟧ so are the proofs by means of lettered line diagrams. Frames as well as line diagrams are thus likely to be borrowings from Arabic *mu'āmalāt*-writings. Finally we know that Fibonacci used the existing Latin translations from the Arabic – for example, in the *Liber abbaci,* Gerard of Cremona's translation of al-Khwārizmī's Algebra [Miura 1981] – without speaking much about it (cf. [Folkerts 2006: IX]).

Beyond that, Fibonacci's source for his algebra (which goes far beyond al-Khwārizmī) is obviously the Arabic discipline. The "Indian figures" he was first taught in Bejaïa; and at least his notation for "ascending continued fractions"[5] is also a borrowing from the recent Maghreb tradition, perhaps also other notations for composite fractions. About three problems he states explicitly that they had been given to him in Byzantium, and in consequence one may suspect that the others were found in the Arabic world or in Provence or Sicily – if not exactly in the form he gives,then at least as types.

It is striking, that in all problems where Byzantium is mentioned and money spoken of, the monetary unit is the *bizantius.* May we conclude that problems and rules referring to Provençal metrology are somehow of Provençal origin? And similarly concerning Sicily?

That is as far as we can come on the basis of the *Liber abbaci* itself.

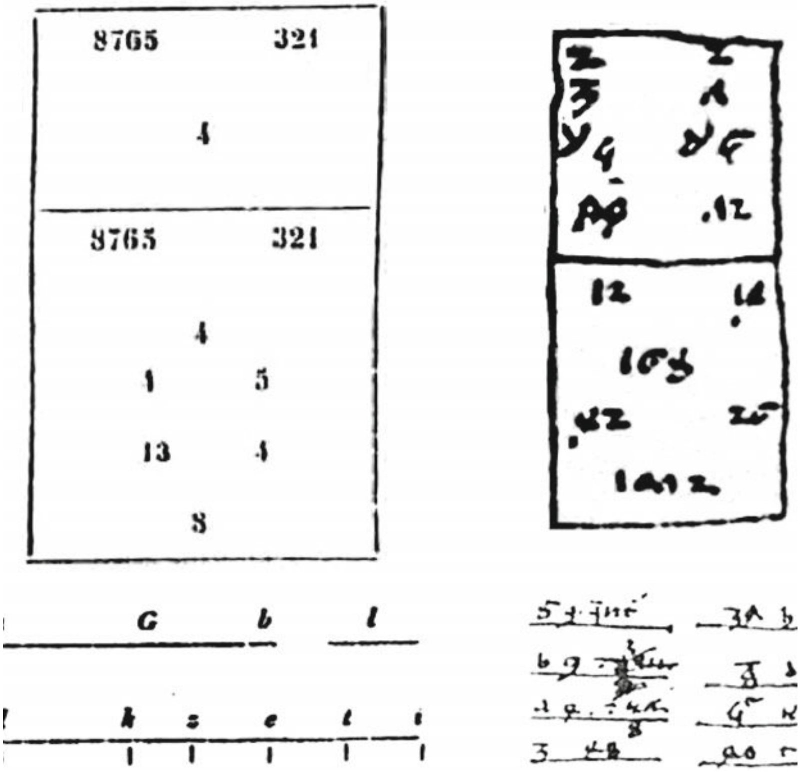
verb in the singular or the plural.

When quoting modern editions of sources I allow myself small tacit modifications at the same level as that of the editors – word separation, diacritics, etc.

3 Rhetorical equation algebra of the first degree with unknown *res* – however not spoken of as *algebra* by Fibonacci.

4 ⟦In recent years, two editions of this works have appeared, [Vlasschaert 2010] and [Sesiano 2014]. In the present article I have relied on a photocopy of Paris, Bibl. Nationale, Ms. Latin 7377A, which for its purposes was sufficient.⟧

5 In this notation, $\frac{1\ \ 5\ \ 7}{2\ \ 6\ \ 10}$ stands for $\frac{7}{10}$ plus $\frac{5}{6}$ of $\frac{1}{10}$ plus $\frac{1}{2}$ of $\frac{1}{6}$ of $\frac{1}{10}$.



Marginalia – left from the *Liber abbaci,* right (redrawn) from *Liber mahameleth*.

The earliest two abbacus books

Until recently, the *Livero de l'abbecho* [ed. Arrighi 1989] was considered the earliest surviving abbacus book. The beautiful vellum manuscript (Florence, Ricc., Ms. 2404, Fol. 1^r–136^v) was apparently written in Umbria. Loan contracts in the text, dated 1288–1290, were taken by Warren Van Egmond [1980: 156] as justification for the dating "c. 1290, *i*[*nternal evidence*]". Gino Arrighi [1989: 6] was more cautious, judging it to belong to the second half of the 13th century because of the general character ("stesura").

At first inspection, this manuscript seems to fit our conventional wisdom. It presents itself [ed. Arrighi 1989: 9] as "secondo la oppenione de maiestro Leonardo de la chasa degli figluogle Bonaçie da Pisa", "according to the opinion of master Leonardo Fibonacci of Pisa". Later abbacus writings have much in common with this manuscript, even though they say nothing about Fibonacci, and one is therefore easily led to the conclusion that even they descend (though perhaps less directly) from the *Liber abbaci.*

Similarities with later abbacus writings start already in the beginning of the *Livero*, namely with the presentation of the rule of three:

> If some computation was said to us in which three things are proposed, then we shall multiply the thing that we want to know with the one which is not of the same (kind), and divide in the other.[6]

With small variations, this formulation recurs in almost the whole of the abbacus corpus. Fibonacci, on the other hand, offers nothing even vaguely similar.

More in general, the whole chain of arguments is shattered as soon as one reads the *Livero* precisely and confronts it with the *Liber abbaci*. Roughly speaking (all details are found in [Høyrup 2005]), the *Livero* moves on two levels. On the basic level we find everything belonging to the syllabus of the abbacus school; nothing on this level comes from Fibonacci. But then there is a sophisticated level. Here, many problems are translated from the *Liber abbaci* – not rarely with errors showing that the compiler of the *Livero* did not understand what he copied.[7] But *that* which the compiler copies, he copies faithfully – often repeating Fibonacci's cross-references, even though they are no longer valid in the new context.

As well known, Fibonacci writes mixed numbers in the Arabic way – that is, not $69\frac{1}{3}$ but $\frac{1}{3}69$. In the problems he borrows from Fibonacci, the compiler does the same, only adding in many cases the unit, which Fibonacci leaves out.

Those problems that do not come from the *Liber abbaci* are different. The first pages follow the habits of the time and write, for example, "libra 1, soldo 1, denari 10, $\frac{6}{7}$ di denaro".[8] Then suddenly the compiler starts to write the fraction links, for example, "libre 2, soldi 15, denari $\frac{8}{13}$ 4 de denaro". This grammatically impossible structure shows that he has used an original writing "… denari 4, $\frac{8}{13}$ de denaro" and then (whether inspired by Fibonacci or not we cannot know, cf. below) shifted this according to Arabic habits. This interpretation is confirmed by a few slips: occasionally the shift is forgotten. The

6 Se ce fosse dicta alchuna ragione ella quale se proponesse tre chose, sì devemo moltiplicare quilla chosa che noie volemo sapere con quella che non è de quilla medessma, a partire nell'altra.

7 Fibonacci's composite fractions are read as normal fractions, demonstrating that the compiler has not followed the calculations. Fibonacci's alternative solutions by means of *regula recta* are mostly skipped, in one case however only the beginning (where the unknown is posited to be a *res*); in the passage that is taken over, the compiler translates this *res* as a nonsensical non-algebraic "thing" (*cosa*). In one case, where Fibonacci [ed. Boncompagni 1857: 399] solves a problem of the second degree (repeated commercial travels with profit) by means of proportions in a lettered line diagram and *Elements* II.6, all letter-references disappear from the text, as does the line diagram itself and the reference to the Euclidean proposition.

8 Ed. [Arrighi 1989: 16]. Abbreviations resolved, punctuation (and in later quotations diacritics) modern.

material that does not come from Fibonacci must therefore come from an earlier abbacus text – perhaps together with other additions. That other, later abbacus texts turn out to be related to *this* part of the material of the *Livero* hence does not at all link them to Fibonacci.

The same distorted way of writing amounts of money involving mixed numbers is found in the loan contracts contained in the *Livero*. In consequence, these cannot originally have been composed by the compiler; as the rest of his text, they must have been copied from a model. The date 1288–1290 is therefore only *post quem. The complete ignorance of the compiler of even the most basic algebraic terminology* tells us, however, that he cannot have much later than c. 1310.[9]

The so-called *Columbia Algorism* (New York, Columbia University, Ms. X 511 A13) was edited by Kurt Vogel in [1977]. The manuscript, from which several leaves are missing, is a copy. Vogel, basing his judgment on a coin list in the treatise, dated the original to the second half of the 14th century. More recently, an Italian numismatist has identified the coins better, and shown that the list is with great certainty to be dated between 1279 and 1284 [Travaini 2003: 92]. That explains, on one hand, the presence of many coins that were no longer minted in the 14th century, and on the other the archaic shape of the numerals [Vogel 1977: 12]. It is obviously not possible to exclude that the compiler of the manuscript borrowed an older coin list, but on the whole it does not seem likely. The shape of the numerals also supports a date of the original around 1285 (the copy we have may have emulated an earlier style).

The *Columbia Algorism* is hence likely to be the earliest abbacus text which we possess. It is therefore already interesting that it shows no traces of influence from the *Liber abbaci* – but it is even more interesting that it appears to be connected to Iberian material in several ways.[10]

Indubitable evidence for this Iberian connection is the way the rule of three is dealt with. In all Ibero-Provençal of abbacus type I have inspected this rule is introduced through purely abstract or even counterfactual number problems. The Castilian *Libro de arismética*

9 The manuscript, apparently a *de luxe* copy on vellum, may obviously be later. That, however, is of no concern for the present discussion.

10 It is also linked to the *Livero* and thereby to the non-Fibonacci source(s) of that work. One problem in the *Livero* [ed. Arrighi 1989: 119*f*] deals with two kinds of dirty wool which shrink differently when washed. The same problem – with the same very characteristic numerical parameters (378, 217, 4089) and with certain characteristic explanatory remarks – can be found in the *Columbia Algorism* [ed. Vogel 1977: 83*f*]. The formulations – not least the different ways to treat the rule of three – exclude direct copying.

que es dicho alguarismo[11] – a 16th-century copy of a manuscript from 1393, in itself a copy of an original going back at least to the early 14th century – identifies the problem-type by means of the phrase "sy tanto faze tanto, ¿qué sería tanto?" ("if so much would make so much, how much would so much be"), and gives as its first example the question "sy 3 fuesen 4, ¿qué seria 5?" ("if 3 were 4, what would 5 be?"). The *Traicté de la praticque de algorisme*[12] speaks of the "regle de troyz", and gives the general rule "multiplie ce que veulz savoir par son contraire et puiz partiz par son semblant" ("multiply that which you want to know by its contrary and then divide by its similar") – rather similar to what we have encountered in the *Livero.* The first example, however, is stated about abstract numbers, "se 6 valent 18, que vauldront 9? ("if 6 are worth 18, what will 9 be worth?"). Barthélemy de Romans' *Compendy de la praticque des nombres* [ed. Spiesser 2003: 256*f*] does not speak about any "regle des troyz", but it contains the same general rule and the example "si 5 valent 7, que valent 13?" ("if 5 are worth 7, what are 13 worth"). Jacques Sesiano's excerpts [1984: 45] from the "Pamiers Algorism" (c. 1430) do not indicate how the rule was spoken about, but still show the first examples to be of the types "$4\frac{1}{2}$ valent $7\frac{2}{3}$, que valen $13\frac{3}{4}$? ("$4\frac{1}{2}$ are worth $7\frac{2}{3}$, what are $13\frac{3}{4}$ worth?)". Francesc Santcliment's Catalan *Summa de l'art d'aritmética* from 1482 [ed. Malet 1998: 163] and Francés Pellos' *Compendion de l'abaco* [ed. Lafont & Tournerie 1967: 101–103], written in Nice in 1492, speak of the rule of three as "regla de tres" and "regula de tres causas" and also of *semblant* and *dissemblant/contrari,* but both initial examples are of the type just mentioned. Santcliment explains that *en nostre vulgar,* "in our [Catalan] vernacular" the rule is referred to by the phrase "si tant val tant: que valra tant". Chuquet [ed. Marre 1880: 632], writing in Lyon around 1484 and also speaking of the "rigle de troyz", gives a more thorough theoretical explanation (after all, he studied the Arts as well as Medicine at university). Here he explains that the problem consists in finding the fourth proportional, and that the first and the third are "similar and of one condition" and the second and the fourth "similar and of one nature and dissimilar and contrary to the other two".[13] But his introductory example is of the same kind as those of his Provençal

[11] Ed. Caunedo del Potro, in [Caunedo del Potro & Córdoba de la Llave 2000: 147].

[12] Ed. [Lamassé 2007: 469]. This treatise is conserved in the same codex as the *Compendy de la praticque des nombres* written by Barthélemy de Romans and may also have come from his hand. In any case it comes from the same Franco-Provençal environment and from the early second half of the 15th century.

[13] "Et sont tousiours le premier et le tiers semblans et dune condicion et le second et le quart entre eulx deux sont semblans et dune nature et dissemblans et contraires aux aultres deux". That obviously does not agree with Euclidean proportions, where the first and the second number must be of the same kind – if not, they have no ratio – and the third and the fourth must also be of the same kind. Chuquet has combined his explanations in terms of proportion theory and his reference to the traditional formulation badly.

precursors: “se 8 valent 12, que vauldront 14?” (“if 8 are worth 12, what will 14 be worth?”), where there is *no* difference of kind between the numbers involved.

This conflict between rule and example, which we find in all Provençal writings and also in Santcliment’s *Summa,* must probably be understood as the outcome of a collision – namely between the structure which was quoted above from the *Livero* (where the initial examples either deal with two different kinds of coin or with commodity and price) and the one we find in the *Libro de arismética que es dicho alguarismo,* and which Santcliment ascribes to *nostre vulgar. Precisely this Iberian approach to the rule of three is also that of the Columbia Algorism.*

A formulation – supposed to be that of the reader – “so much for so much, how much for so much?” is also found in al-Qurašī (11th century, probably Damascus). [ed., trans. Rebstock 2001: 64] as the very first introduction to the rule of three. Then follows a more precise explanation in terms of the usual Arabic commodity-price formulation, and finally a reference to *Elements* VII (as usual in the works of Arabic erudite mathematicians like Abū Kāmil, al-Karajī and ibn al-Bannā’⟦[14]⟧). The first example is also in pure numbers, but linked to the Euclid reference and therefore not conclusive. None the less, the Iberian formulation suggests a link to a component of Arabic mathematical culture that had no influence in Italy, and which also left no discernable traces in erudite Arabic mathematics.

The other participant in the collision – that is, the reference to the similar and the dissimilar – may have arrived in Provence from Italy. Yet this is nothing but a possibility. The name “rule of three” was already used by several first-millennium Sanskrit mathematicians;[15] even though no Arabic writing I know of refers to it, it must be assumed to have been transmitted somehow – plausibly through Arabic merchants’ culture. The similar and non-similar is given by both Sanskrit and Arabic authors as a secondary explanation[16], that is, once again, as something already familiar – and plausibly, once again, from commercial practice. Whether the Provençals have learned from the Italians or directly from the same source as the latter is hardly decidable; as we shall see, on this point the Italians cannot have learned from the Provençals.

Beyond the way kinship between the *Columbia Algorism* and the *Libro de arismética* is reflected in their shared approach to the rule of three, a particular problem points in

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[14] ⟦The original version, seduced by translations, included al-Khwārizmī in this list. This was a mistake, cf. article I.5⟧

[15] Namely Āryabhata [ed., trans. Elfering 1975: 140], Brahmagupta [ed., trans. Colebrooke 1817: 283], and Mahāvīra [ed., trans. Raṅgācārya 1912: 86].⟦See also article I.5.⟧

[16] Brahmagupta [ed., trans.. Colebrooke 1817: 283]; Mahāvīra [ed., trans. Raṅgācārya 1912: 86]; ibn al-Bannā’ [ed., trans. Souissi 1969: 88]; al-Qalaṣādī [ed., trans. Souissi 1988: 67]; ibn Thabāt [ed., trans. Rebstock 1993: 45].

the same direction. In the former we read [ed. Vogel 1977: 122]

> Somebody has money in his purse and we do not know how much. The $\frac{1}{3}$ ⟨and the $\frac{1}{5}$⟩ were lost, and 10 *denari* remained for him. I ask, how many *denari* he had before the $\frac{1}{3}$ and the $\frac{1}{5}$ were lost for him. This is its right rule, that we shall say {in which we shall say}, in what $\frac{1}{3}$ and $\frac{1}{5}$ are found, and they are found in 3 times 5, that is, 15; and thus one shall say that he had 15 *denari* in the purse. Remove the $\frac{1}{3}$ and the $\frac{1}{5}$ of 15, 7 escape. Say thus; if 7 were 10, what would 15 be? Say, 10 times 15 make 150, to divide by 7, and from this comes $21\frac{3}{7}$, and so much did he have in the purse before the $\frac{1}{3}$ and the $\frac{1}{5}$ were lost for him.[17]

In the *Libro de arismética* (ed. Caunedo del Potro, in [Caunedo del Potro & Córdoba de la Llave 2000: 167]), only 5 *denari* remain, and the story is told in the first and second, not the third grammatical person; apart from that, we find a somewhat abridged but otherwise quite faithful repetition:

> The $\frac{1}{3}$ and the $\frac{1}{5}$ of my *dineros* were lost for me from my purse, and 5 *dineros* remained in it, I ask, how many *dineros* were there in it at first? This is its right rule and calculation, that you shall say, "in what are there $\frac{1}{3}$ and $\frac{1}{5}$?", which is in 15, let us then say that you had 15 *dineros* in your purse, $\frac{1}{3}$ and the $\frac{1}{5}$ were lost, 7 remained, say, "if 7 were 5, what would 15 be?" Say, 5 times 15 are 76, divide by 7, and from that come $10\frac{5}{7}$, and so many *dineros* were there at first in the purse.[18]

The *Columbia Algorism* was not widely diffused – we know only this single copy – and no other Italian abbacus book identifies the rule of three by means of the counterfactual structure.[19] An Italian source for the *Libro de arismética* is therefore out of the question,

17 Uno avia denari in borscia e'nno sapemo quanti. Cadessi lo $\frac{1}{3}$ ⟨e lo $\frac{1}{5}$⟩ e romaseli 10 denari. Adomando, quanti denari avea in prima innatti che'lli chadessaro lo $\frac{1}{3}$ e lo $\frac{1}{5}$? Questa la sua ricta reghola, che noi dovemo dire, {in che noi dovemo dire}, in che' ssi trova $\frac{1}{3}$ e $\frac{1}{5}$, che'ssi trova in 3 via 5, cioiè 15; e chossì si deve dire che lo avesse in borscia 15 denari. Leva lo $\frac{1}{3}$ e lo $\frac{1}{5}$ di 15, chanparia 7. Di' chossì: se 7 fosse 10, che serria 15? Di', 10 via 15 fanno 150, a'ppartire per ⟨7⟩, e che ne viene $21\frac{3}{7}$, e chotanto denari avea in borscia enanti che'lli chadesse lo $\frac{1}{3}$ e lo $\frac{1}{5}$.

18 El $\frac{1}{3}$ y $\frac{1}{5}$ de los mis dineros se me cayeron de la limosnera e fincaron en ella 5 dineros, demando, ¿quántos dineros avía en ella primeramente?, esta es la sua derecha regla e cuenta, que tú debes dezir ¿en qué se fallan 3 e 5?, que es en 15, digamos agora qué tú ovieses 15 dineros en tu bolsa, cayósete el $\frac{1}{5}$ y el $\frac{1}{3}$, fincáronte 7, di sy 7 fuesen 5 ¿qué serian15?, di 5 vezes 15 son 75, parte por 7 e viénente $10\frac{5}{7}$ e tantos dineros avía primerament en la limosnera.

19 Counterfactual problems are certainly not uncommon in the Italian abbacus texts – even counterfactual *calculations* like "if 5 times 5 were 26, say me, how much would 7 times 7 be at the same rate?" (Jacopo da Firenze, ed. [Høyrup 2007a: 238]). However, these are either found separately from the rule of three, or they are offered as secondary examples – see

and Iberian inspiration for the *Columbia Algorism* is much more credible.

As regards the *Columbia Algorism* one may add that it makes occasional use of the above-mentioned notation for ascending continued fractions – once to be read from right to left, in agreement with Fibonacci's custom, once from left to right [Vogel 1977: 13]. As we shall also see it confirmed later, Fibonacci was thus not the only source for the Maghreb-inspired notation. Whether the compiler of the *Livero* got the idea for his bizarre notation for concrete mixed numbers from his reading of Fibonacci (or a vernacular translation of the *Liber abbaci*) or took inspiration from elsewhere thus cannot be decided.

"We say"

Both the *Columbia Algorism* and the *Libro de arismética* make use of a single false position in the problem that was just quoted. Practically all abbacus books employ this method. Thereby they differ from Fibonacci, who still does not like falsity in mathematics. Instead he does as follows [ed. Boncompagni 1857: 173]:

> There is a tree, of which $\frac{1}{3}\,\frac{1}{4}$ is underground. And they are 21 palms. It is asked how much is the length of this tree. Since $\frac{1}{3}\,\frac{1}{4}$ can be found in 12, understand this tree to be divided into 12 equal parts. […][20]

He then finds that $\frac{1}{3}+\frac{1}{4}$ of these 12 parts, that is, 7 parts, equal 21 palms, etc. He goes on:

> There is another methods which we use, namely, that for the unknown thing you posit a freely chosen number, that can be divided in whole numbers in fractions […][21]

Fibonacci does not use the authorial plural. His "we" either includes the reader together with Fibonacci or (as here, where the reader is learning and therefore cannot be supposed to know) some other group in which Fibonacci includes himself.

[Høyrup 2007a: 64–67]. In no other Italian abbacus book is the rule of three identified by means of this counterfactual structure.

Already the *Liber abbaci* [ed. Boncompagni 1857: 170] offers a counterfactual problem as well as a counterfactual calculation. Yet Fibonacci does not like them and takes care to present a properly mathematical explanation. We may hence safely exclude that he should be the inventor, and already in his times such problems must therefore have been in circulation.

20 Est arbor, cuius $\frac{1}{4}\,\frac{1}{3}$ later sub terra; et sunt palmi 21: queritur quanto sit arboris illius longitudo: quia $\frac{1}{4}\,\frac{1}{3}$ reperiuntur in 12, intellige ipsam arborem esse in partes 12 equales divisam. […].

21 Est enim alius modus quo utimur, videlicet ut ponas pro re ignota aliquem numerum notum ad libitum, qui integraliter dividatur per fractiones. […].

We find something similar in alloying problems. In many abbacus books, alloying problems often begin in the style "I have silver that contains *n* ounces per pound"[22]. So does the *livero,* but not in the problems that are taken over from Fibonacci. Nor is it used by Fibonacci in the alloying *problems* of the *Liber abbaci.* However, an introductory general explanation [ed. Boncompagni 1857: 143], explains that "when we say, 'I have bullion at no matter how many ounces', let us say at 2, then we understand that its pound of bullion contains 2 ounces of silver".[23] Whether the reader is included this time is perhaps not clear (cf. imminently), but since no preceding argument leads to the statement, some wider group must at the very least *also* be thought of. Once again, as we see, Fibonacci quotes a way of speaking that was already diffused – *how* diffused we see a little bit later [ed. Boncompagni 1857: 160]: Here, Fibonacci reduces a problem of the type "lazy worker" to an alloying problem, and introduces the latter by the words "I have bullion at 26 and at 37 […]"/"habeo monetam ad 26 et ad 37 […]". Although he does not use the structure himself, he thus expects the reader to recognize through this phrase the use of the alligation model.

The same structure is found in a Castilian merchants' manual,[24] and also in a Byzantine arithmetic book from the early 14th (Ψηφηφορικὰ ζητήματα καὶ προβλήματα, "Calculation Questions and Problems") [ed. Vogel 1968: 21–27], which (to judge from the metrology it makes use of) cannot be much influenced by the young Italian tradition. Here, the first person singular is also used for other problem types – mostly but not exclusively such as have to do with payment in gold coin. Also Paolo Gherardi, Italian but writing in Montpellier in 1328, often uses the first person singular in many problems that have to do with gold but not always with alloying. One may guess but probably cannot prove that the habit goes back to Byzantine money changers. In any case, Fibonacci already knew it in the early 13th century, and a century later it was diffused in the whole Christian Mediterranean area.

Finally, certain abbacus books designate the approximation $\sqrt{(n^2+a)} \approx n+\frac{a}{2n}$ as "the closest" root (*la più pressa*). The same designation is used by Fibonacci in the *Pratica geometrie* – neither in general nor the first or second time it is used but once in a hidden corner, apparently as a reference to something familiar [ed. Boncompagni 1862: 27].

22 Several examples are listed in [Høyrup 2007a: 125]. Beyond abbacus books, the structure is found in Pegolotti's early-14th-century *Pratica di mercatura* [ed. Evans 1936: 342–357].

23 Et cum dicimus; habeo monetam ad uncias quantaslibet, ut dicamus ad 2, intelligimus quod in libra ipsius monete habeantur uncie 2 argenti.

24 "E si te dixeren, yo tengo de tres suertes plata, la una suerte es de 1 marco, 7 onças $\frac{1}{3}$ de onça de plata fina […]". Real Academia Española, Ms. 155, *De arismetica* fol. 151r [Caunedo del Potro 2004: 45].

The second generation: Italians in Provence

Abbacus schools existed no later than 1265 [Ulivi 2002: 224]. At least at that time, abbacus authors must also have existed – but since the writings of these early writers have all been lost, we may consider them a "generation zero". The "first generation" will then be represented by the *Columbia Algorism* and the *Livero*. As "second generation" we may consider the texts that were written between 1305 and 1340, together with their authors.

A particular text from this period is a *Liber habaci* (Florence, Magl. XI, 88, fol. 1^r–40^v, ed. [Arrighi 1987b: 109–166]).[25] Beyond mathematics, the text contains calendarial and historical material and a coin list, which together allow a dating around 1310 [Van Egmond 1980: 115].

The text appears to have been written in Provence [Arrighi 1987b: 10]: Firstly, it contains a number of Provençal loan words; secondly, the calendar registers days dedicated to saints that were particularly honoured in Provence.

In spite of the date, the text seems to be a witness of the "generation zero". All integer numbers (even those appearing in the brief explanation of the place value system) are written with Roman numerals, and all fractions as full words. The beginning of the rule of three (the very beginning) runs like this:

> Se ci fosse detta alchuna ragione nella quale si proponesse tre chose, sì dobbiamo multipricare quella chosa che'nnoi volglamo sapere contr'a quella che'nnonn'è de quella medesima e partire nell'altra

– *exactly* as in the *Livero*, apart from orthographic differences and a multiplication *contra* instead of *con*. That should remind us that there is no *necessary* connection between the use of Hindu-Arabic numerals and the abbacus school teaching of problem solutions and metrological conversion rules. It is quite possible that the earliest abbacus masters used Roman numerals, and that the Hindu-Arabic numerals were only adopted by the "first generation".[26]

Three more abbacus texts belonging to the second generation were with full certainly written in Provence: Jacopo da Firenze's *Tractatus Algorismi* (Montpellier, 1307),[27]

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25 Arrighi ascribes it to Paolo Gherardi, who was active in Montpellier in 1328, but gives no pertinent reasons for this assumption.

26 Quite possible, but obviously nothing but a possibility. The *Liber habaci* may also have been a mere curiosity, product of its own time.

27 In total, three manuscripts represent Jacopo's *Tractatus:* (a) Vatican, Vat. Lat. 4826; (b) Milan, Trivulziana Ms. 90; and (c) Florence, Ricc. Ms. 2236. (a) can be dated by watermarks to c. 1450; (b) in the same way to c. 1410; (c), on vellum, is undated – Van Egmond [1980: 148] only repeats the date 1307 from the colophon, shared by all three manuscripts

Paolo Gherardi's *Libro di ragioni* (Montpellier, 1328)[28] and the anonymous *Trattato di tutta l'arte dell'abbacho* (Avignon, around 1334).[29]

Both Jacopo and Gherardi deal with algebra, and some algebraic problems are found scattered on the last folios of the Florence manuscript of the *Trattato di tutta l'arte.* This innovation is certainly the most striking difference between the first and the second generation.

It must obviously be argued that this is really an innovation of the early 14th century. I have treated this question elsewhere [Høyrup 2006], for which reason I shall only present a rough outline of the argument here.

and meaningless as a manuscript dating. Since (c) is closely related to (b) but of lower quality, it was probably also written during the first half of the 15th century; this date, however, is neither quite certain nor important.

Closer analysis of the three manuscripts [Høyrup 2007a: 5–25 and *passim*] shows that (b) and (c) represent a revision of the original text, probably an adaption to the usual curriculum of the abbacus school, where the algebra and a final collection of mixed problems have been eliminated and a few problems (for instance teaching the "welsche Praktik", that is, close to market practice) have been added. (a), on the other hand, is a faithful copy of a faithful copy, and this faithfulness appears to go through the whole chain until the beginning. With fair confidence we may consider (a) as a good copy of the original.

An edition of (a) with English translation and a critical edition of (b)+(c) can be found in [Høyrup 2007a]. A preliminary edition of (a) is accessible on the web, [Høyrup 1999] = http://www.ruc.dk/~jensh/Publications/1999%7Bc%7D_Jacopo-Tractatus_transcription.pdf. Also on the web is a preprint of the critical edition of (b)+(c), [Høyrup 2007b] = http://rossy.ruc.dk/ojs/index.php/fil3/issue/view/530.

28 Ed. [Arrighi 1987b: 15–107]. As observed by Van Egmond [1978: 162], the manuscript is a far from perfect copy of a lost original. None the less, the colophon (giving the date and the identity of the author) appears to come from the original; similarly, Jacopo's colophon is found unchanged in all three manuscripts.

29 For this dating, see [Cassinet 2001]. I have used the manuscripts Florence, Bibl. Naz. Centr., fond. princ. II,IX.57, and Rome, Acc. Naz. dei Lincei, Cors. 1875. The former appears to be the author's draft version, the latter was written in c. 1340. Van Egmond [1977: 19; 1980: 140, 179] ascribes the treatise to Paolo dell'Abbaco but does not tell his reasons. The *possible* reasons I can find have no weight, cf. [Høyrup 2007a: 54 n. 144]. In ms. Florence, Ricc. 2511 1340–50, an anonymous version of the *Trattato di tutta l'arte* is followed by the "Regoluzze di maestro Pagholo astrolagho" [Van Egmond 1980: 158] – fairly good evidence that the compiler of the only 14th-century manuscript that contains both works was convinced that the *Trattato* was *not* written by Paolo. In the *Regoluzze,* moreover, Paolo [ed. Arrighi 1966: 32] derives the circular perimeter from the diameter, which does not fit the *Trattato di tutta l'arte* at all – see below, text before note 43.

It seems that a sheet is missing in the beginning of Jacopo's algebra (which is only contained in the Vatican manuscript, cf. above, note 27) – it begins without title, introduction, sign rules and rules for the multiplication powers, jumping directly to the rules for the solution of the algebraic "cases" (equation types). Apart from that, it is quite tidy. It presents the six first- and second-degree cases in an order that was conserved by almost all other abbacus algebras until Luca Pacioli [1494: 145^{v}–146^{r}] returned to Fibonacci's order.[30] For each of these cases Jacopo offers an abstract prescription as well as one or more examples – for the case with a double solution one where subtraction leads to an acceptable solution, one where addition serves, and one where addition as well as subtraction are shown to give valid solutions. All cases are defined in their non-normalized form (in contrast to all preceding Latin algebras, translations as well as Fibonacci's), and therefore their first step consists in a normalization. That was to remain the habit of later abbacus algebra. Five of the ten examples deal with abstract numbers,[31] the other five are dressed as (pseudo-) commercial problems.[32] Not a single one is formulated (as was the norm in the first examples of Latin algebra) in terms of *censo* ⟦[33]⟧ and *cosa* (radice/"root" is not used at all by Jacopo as name for the first power of the unknown, and it is also quite rare in subsequent abbacus algebra). After the six traditional rules follow 14 more rules for reducible third and fourth degree cases,

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30 ⟦Jacopo's order is as follows:

(1) *cosa* = number	(4) *censo* + *cose* = number
(2) *censi* = number	(5) *censi* + number = *cose*
(3) *censi* = *cose*	(6) *censi* = *cose* + number

Al-Khwārizmī as well as Abū Kāmil present the fundamental cases in the order 3-2-1-4-5-6; Fibonacci has 3-2-1-4-6-5.⟧

Before Pacioli, I have noticed three abbacus writers that do not agree with Jacopo's order. One is Gilio da Siena (1384, ed. [Franci 1983: 22–25]. For the first three ("simple") cases he follows the habitual abbacus order, for the three "composite" cases that of Fibonacci. The second is Benedetto da Firenze [ed. Salomone 1982], who follows Fibonacci. The third is Florence, Bibl. Naz., Palatino 575 ([ed. Simi 1992], c. 1460), who first copies al-Khwārizmī (with *radice* instead of *cosa;* even al-Khwārizmī's geometric proofs are reproduced). Then follow the same rules in normal abbacus order, now with *cosa* as the name of the first power of the unknown.

31 In three of them, a number 10 is divided into two parts, two ask for two numbers in given ratio.

32 In order: Partnership calculation, compound interest, "give-and-take" (with a square root of supposedly real money), profit in commercial travel, money exchange.

33 ⟦*Census,* Italian *censo,* was the standard 12th-century Toledan translation of Arabic *māl,* "possession" or "amount of money", and came to serve as the second power of the unknown, while *radice*/"[square] root" or *res/cosa/*"thing" became its first power.⟧

all correct, and none of them provided with examples[34] – two biquadratics are missing in the end, all other cases that can be reduced to first- or second-degree problems or solved by means of root extractions are present. Algebraic core concepts (*cosa, censo, più, meno*[35]) are never abbreviated, as if Jacopo were conscious to present something new, where the reader would not be able to guess the full words from abbreviations (he says explicitly that his book is also meant for self-tuition [ed. Høyrup 2007a: 196]. Formal calculations – for example fractions where the numerator or the denominator are algebraic expressions – are absent. No single problem comes from Fibonacci or from the Latin translations of Arabic algebra.

Gherardi repeats Jacopo's first six cases and most of those of the third degree – he does not deal with the fourth degree. To these he adds a number of irreducible third-degree cases, which he provides with false rules – for instance, he solves the case "cube equal things and number" by means of the rule for "*censi* equal things and number"; finally he presents the case "cubes equal root of number". ⟦[36]⟧ All his cases are provided with an example (never more than one). Most of his examples for the first six cases agree with those of Jacopo.[37] The examples for the third-degree cases are all of the following type [ed. Arrighi 1987b: 106]:

> Find three numbers for me which are in position [mistake for "proportion" meaning "ratio"] together as 2 to 3 and as 3 to 4, and if the first is multiplied by itself and then by the [same first] number, it makes as much as the second multiplied by itself, and added to it the third number, and added then to it 12.[38]

[34] In an Arabic perspective, there was nothing new in this – at least since al-Karajī everything presented by Jacopo was familiar stuff, see for example the paraphrase of the *Fakhrī* in [Woepcke 1853: 71*f*]. Among the erudite Arabic algebraists al-Karajī is indeed the one who mostly comes to mind when one looks at abbacus algebra. Direct abbacus use of al-Karajī's writings can none the less be excluded. ⟦Cf. article I.14.⟧

[35] All the more striking since Jacopo uses the abbreviation m̃ for *meno* in the coin list. Later algebraic authors, for example those working when manuscript (a) was written, would indeed make algebraic use of the same abbreviation.

[36] ⟦The introduction of false rules and their function within abbacus practice are discussed in article I.12.⟧

[37] For reasons which it would it would lead too far to present here (but which have to do with small details in the formulations) we may be fairly sure that these examples not only agree with those of Jacopo but also represent indirect borrowings from him; see [Høyrup 2006] or [Høyrup 2007a: 159–169].

[38] Truovami tre numeri che sieno in positione insieme come 2 di 3 e 3 di 4 e multipricato lo primo per se medesimo e poi per lo numero faccia tanto quanto el secondo multiplicato per se medesimo et postovi suso lo terzo numero et poi postovi suso 12.

⟦"Positione" is a rather obvious mistake for "proportione"/"ratio". The mistake could be due to

Jacopo has two problems of this kind, and they are thus not Gherardi's invention. Dardi (see below) has many of them, too. Since the *zetetic* ("putting into equation") always consist (in the present example) of positing the first number to be 2 things, the second 3 things, and the fourth 4 things, the trick clearly serves to procure an exemplifying polynomial equation that looks more complicated than the equation itself without being really so.

Like that of Jacopo, Gherardi's text contains no formal calculations, but at one point it refers to a formal scheme for cross-multiplication that is also known from later texts:[39]

$$\begin{array}{lcl} 100 & & 1\ \texttt{cosa} \\ & \times & \\ 100 & & 1\ \texttt{cosa piu 5} \end{array}$$

As said above, the *Trattato di tutta l'arte* contains no systematic exposition of algebra but only isolated algebraic problems; Jean Cassinet [2001: 124–127] offers an almost complete list.[40] Even here there are no formal calculations; in a single place instead [Florence-manuscript fol. 159^{r}] we find a notation that for long would be in the way of the unfolding of formal calculations: $\frac{10}{cose}$, standing for "10 *cose*". The background is a conceptualization of the denominator of a normal fraction as a kind of metrological unit, reflected both in the term *denominatio* itself and in the abundant use, for instance, of "il $\frac{1}{3}$" for "the third" [that is, number three of a list], and which also finds more direct expression in two problems from the *Columbia Algorism* [ed. Vogel 1977: 64–66].[41]

Less conspicuous that algebra but still significant is the way the geometry of the circle is dealt with. Three of the four texts written by Italians in Provence take first the perimeter and not the diameter as fundamental parameter (Gherardi has no orderly presentation of geometry, only mixed problems[42]); first the diameter is determined from the perimeter

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a misinterpretation of an abbreviation, but could also come from an attempt of Gherardi or an intermediary to repair Jacopo's equally mistaken "propositione".⟧

39 See [Van Egmond 1978: 169].

40 After this list he also mentions a rudimentary beginning of a systematic presentation, which indeed is bound in the same volume and written on paper with watermarks from the same years, but which is written in a different hand [Van Egmond 1980: 140*f*]. One of the problems found in Gherardi's treatise but not in that of Jacopo is contained here. A related problem, also solved by means of *cosa* and *censo,* is dealt with by Gherardi much earlier in his *Libro* [ed. Arrighi 1987b: 21].

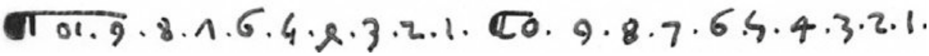
41 Here we find "simple fractions", such as $\frac{1}{grana}$ standing for "1 *grana*", as well as "ascending continued fractions written from left to right, such as $\frac{1}{grana}\frac{1}{2}$, "1 *grana* and $\frac{1}{2}$ of 1 *grana*".

42 They go under two different headings (each problem has its own): either *misura* or *giometria* (a few exceptions indicate the topic, for example *chadrare*). *Giometria* designates such problems

(via division by $3\frac{1}{7}$), and then the area as $\frac{1}{4}\times$(perimeter$\times$diameter). I know this preference from only two other Italian texts, both linked to Jacopo's geometry also in other ways, and both from the 15th century.[43]

Even more than the systematic use of the Provençal *carco* of 300 pounds by Jacopo and in the *Trattato di tutta l'arte* and not of the usual homonymous Italian unit of 400 pounds, this geometrical peculiarity shows that the Italian abbacus masters had come to Provence not in order to teach Italian ways but as learners.

Number schemes to be read from right to left turn up in Jacopo's *Tractatus*, in (a) as well as (b+c), in the latter as an addition, and also in the *Trattato di tutta l'arte.* Fibonacci, on the other hand, though writing his mixed numbers with the integer part to the right, has schemes that are to be read from left to right. Since such schemes are absent from the *Livero* and the *Columbia Algorism* (and are, as far as I have noticed, also not found in later Italian material), it seems plausible that even these schemes were learned in Provence.[44]



The two ways to write the Hindu-Arabic numerals according to Jacopo (Vatican-Manuscript).

In a presentation of the Hindu-Arabic numerals it is close at hand to present these numerals as a sequence. Both Jacopo's *Tractatus* and the *Trattato di tutta l'arte* indeed do so, but in a remarkable manner. Both show two variants of the sequence, one "old", the other "new", and both moving from the right toward the left. The only important differences between the two sequences concerns the numerals 4 and 7. The "old" shape seems to be of Iberian origin. The idea to show two variants of the sequence goes back (at least) to ibn al-Yāsamīn (active in Marrakesh und Seville, fl. c. 1200) [Burnett 2002: 269 Pl. 1; Kunitzsch 2005: 17]. Ibn al-Yāsamīn (and various Latin manuscripts that emulate him) confronts the East- and West-Arabic shapes. Instead, the two Italians writing in

as come or could come from the Latin ("Boethius-", agrimensor- and post-agrimensor-) tradition, while *misura* problems seem to stem from Arabic *misaha* geometry. As far as I know, this distinction is not made anywhere else; it is therefore difficult to derive anything from it.

43 The *Praticha di geometria e tutte le misure di terra* written by the nobleman and dilettante Tommaso della Gazzaia [ed. Arrighi & Nanni 1982], and the *Praticha di gieometria* of the military engineer Francesco di Giorgio Martini [ed. Arrighi 1970: 30]. Even though both are related to the abbacus tradition, they are not abbacus books in the proper sense.

44 Abundant errors in the copied manuscripts also suggest that the copyists were not accustomed to text pages organized in like this.

Provence show the original and a later adaptation of the West-Arabic numerals. Since no other abbacus author does anything similar, we may legitimately assume that even this reflect a Provençal habit.

Something, however, was brought from home by the Italians who wrote in Provence. Gherardi gives the rule of three in almost the same words as the *Liber habaci* (whence also as the *Livero*). Jacopo (a-version) replaces "che non è di quella medesima" with "che non è simigliante" and expands a bit in the end, adding "nel'altra cosa, cioè, nell'altra che remane"/"in the other thing, that is, the other that remains"; apart from that, his rule coincides with that of the others. The *Trattato di tutta l'arte* and the (b+c)-version of Jacopo expand "nell'altra, cioè nella terza cosa"/"in the other, that is, in the third thing". Gherardi's first example treats of silver against monetary value, the other three begin with coin against coin of different kind, as usual in the abbacus books (in Arabic treatises, a confrontation of commodity and price is the standard to such an extent that it determines the terminology). Here, we see, the Italians have not learned from the Provençal environment. It other domains the direction of inspiration probably cannot be decided – the Ibero-Provençal material is simply too sparse and too late for that.

More from the second generation

From around 1330 and from the neighbourhood of Lucca comes a compilation made by several hands, the *Libro di molte ragioni* [ed. Arrighi 1973]. In many respects it seems to be independent of the works that were just discussed and thus to present evidence of what had happened within the confines of Italy. It is not fully independent, however. It contains no less than two sections dealing with algebra, one with the heading *Regola della chosa,* one simply called *Algebra.* Both give the six fundamental rules with examples[45] and after that a selection of rules for reducible cases of the third and fourth degree. They mostly agree with each other, and their shared source stands between Jacopo's and Gherardi's texts (which is what enables us to conclude that Gherardi's text depends on Jacopo only indirectly, cf. note 37). That no less than two of the compilers present algebra is evidence of the importance which this new technique soon acquired among the abbacus teachers – an importance that was no matter of course, given that algebra had no space within the two-year curriculum of the abacus school.[46]

45 Actually, *Algebra* gives only five, but afterwards it refers to the "6 reghole"; one will simply have been forgotten.

46 We possess two sources for this curriculum, a description proper from Pisa, written in 1428–29 [ed. Arrighi 1967b], and a contract with an assistant from Florence from 1519 [ed. Goldthwaite 1972: 421–425]. Beyond that we have scattered remarks in various abbacus books.

An illustrative example of the kind of integration and transformation of the Provençal inspiration that is likely to have taken place in the Lucca compilation is offered by a geometrical problem [ed. Arrighi 1973: 121]. Both Jacopo [ed. Høyrup 2007a: 276*f*, 436] and the *Trattato di tutta l'arte* (Florence, fol. 132^r) present a problem type about paving (*lastricare*) a rectangular hall or square.[47] The type appears to derive from the post-agrimensor tradition[48], and it is repeated in "Provençal" version by Tommaso di Gazzaia [ed. Arrighi & Nanni 1982: 19]. The Lucca compilation instead deals with an obliquely oriented roof with given height and given eaving over the wall – but it still speaks of "paving" (*lastricare*), which in this architectonic situation is rather bizarre and thereby betrays the origin of the problem.

Late members of the "second generation" known by name are Biagio "il vecchio", Paolo dell'Abbaco and Giovanni di Davizzo. However, we only know Biagio and Giovanni from testimonies and excerpts from the 15th century; of Paolo's properly mathematical works we possess the *Regoluzze,* a brief collection of very concise rules [ed. Arrighi 1966]; and a single sheet about the measurement of casks [ed. Boncompagni 1854: 383*f*]; beyond that, two problem collections from the 15th century claim to consist of extracts from his writings.[49]

Alchune ragione, a manuscript written in 1424 (Vatican, Vat. lat. 10488[50]), contains six pages with the heading *Algebra* added in a later hand. These are stated to come from a book written 15 September 1339 by Giovanni di Davizzo de l'Abacho from Florence. The dating is likely to be taken from the colophon of the book and can therefore be considered reliable.[51]

Giovanni's algebra-fragment consists of two parts. The second (fols 30^r–31^v) contains 19 rules for the solution of reduced equations. 18 of these are already dealt with by Jacopo

47 Gherardi [ed. Arrighi 1987b: 62*f*] also gives it, with the only difference that the floor is covered by bricks (*mactoni*).

48 It is found in the *Propositiones ad acuendos iuvenes* [ed. Folkerts 1978: 62] and in the *Geometria incerti auctoris* [ed. Bubnov 1899: 355].

49 Van Egmond [1977: 20] expresses strong doubts, but for the reason that these two treatises differ rather much from the *Trattato di tutta l'arte,* which he ascribes to Paolo, as mentioned above. Since this ascription itself is highly dubious, Van Egmond's arguments can hardly be considered pertinent. However, comparison of the details of the published collection *Istratto di ragioni* [ed. Arrighi 1964] with Paolo's indubitable *Regoluzze,* for instance the way multiplication of mixed numbers is dealt with ([Arrighi 1964: 28] respectively [Arrighi 1966: 33]), speaks against close relationship.

50 My references are to the original foliation.

51 Giovanni (fl. 1339–1344) belonged to a Florentine family of abbacus masters, whose activity spanned most of the 14th century – see [Ulivi 2002: 39, 197, 200].

(2 of Jacopo's equation types are missing), but in words that are sufficiently different to exclude Jacopo as a direct source. One is certainly false but almost unreadable;[52] so much is certain, however, that it is not one of the extra cases treated by Gherardi. It thus seems that the source that had once inspired Jacopo was also drawn upon after his time.

The first part (fols 28^v–29^v, ed. [Høyrup 2007c: 479–481]) could correspond to the sheet that seems to have disappeared from the beginning of Jacopo's algebra: it contains the sign rules ("more times more makes more"/"più via più fa più",[53] "more times less makes less"/"più via meno fa meno" etc.) and rules for the multiplication of algebraic powers and examples for the calculation with roots.[54] But it goes further and postulates a whole sequence of rules for the division of algebraic powers, in which the negative powers are represented by "roots" (and the first negative power by "number".[55] The same system turns up again in treatises by Piero della Francesca and Giovanni Guiducci and as late as 1555 in Bento Fernandes [Silva 2006: 14] – always in formulations so close to those of Giovanni that he can be reasonably regarded as the sole source for the system (and hence its likely inventor). Gherardi's false solutions are thus not the only evidence that already the second generation experimented with algebra.

More mathematical competence is displayed by Biagio, as we know him through Benedetto da Firenze's excerpt. According to Benedetto [ed. Pieraccini 1983: 1], Biagio died in "1340, o circha", and he is thus a member of our "second generation".

In book XIV of his *Trattato di praticha d'Arismetricha* from 1463, Benedetto collects a number of algebraic cases

> from master Biagio's *Trattato di Pratica,* not because the others who write about it do not say rather much, but because he, according to master Gratia de' Chastellani, was the first to reduce the said *Trattato* to a good *pratica*, since he died in 1340 or so, and there had been nobody before him who had dealt with this *pratica,* if not for a long time, until

52 Somebody has discovered the error and glued a slip of paper over it. Subsequently, this slip has disappeared, but the manuscript paper has become as dark as the ink.

53 In Giovanni di Davizzo, and also in the *Trattato dell'alcibra amuchabile* (see presently), these translations are the adequate ones. When the notion of additive and subtractive contributions had gradually developed into a concept of positive and negative numbers, we should rather translate "plus times plus makes plus", "plus times minus makes minus", etc.

54 Even these rules may make us think of al-Karajī's *Fakhrī,* cf. note 49.

55 Close analysis would lead away from our topic, but see [Høyrup 2007c: 478–484] and [Høyrup 2009a: 55–59] ⟦= article I.12⟧. ⟦Higher "roots" are produced by multiplicative composition – the fifth root thus as "[square] root of cube root".⟧

> that where Leonardo Pisano flourished. And the said master Biagio was the teacher and companion of the great master Paolo [dell'abbaco].[56]

Benedetto was closer than most abbacus masters to the Humanists of his time; for instance, he goes explicitly back to the time before Fibonacci and copies also al-Khwārizmī's geometrical demonstrations "because they are older" [ed. Salomone 1982: 20].[57] Among earlier abbacus authors, however, he only cites those who belong to his own school group (as masters, students or friends) – a parochialism which (regarding similar manuscripts, cf. note 76) was already noticed by Arrighi [2004/1967: 163; 2004/1968: 209], and which also does not differ much from the mores of the Humanists. Whether, between Fibonacci and Biagio, he *does not know* or *refuses to know* about Jacopo and Gherardi, cannot be decided. In any case he respects Biagio highly.

Benedetto only brings problems from Biagio, no rules – neither sign rules nor rules for the multiplication of powers nor the solutions of standard cases. Benedetto gives such rules himself already in book XIII [ed. Salomone 1982], and their absence from Book XIV therefore does not mean that Biagio had nothing corresponding to Jacopo or Giovanni di Davizzo. Be that as it may, from Biagio's hand we do not possess any kind of orderly introduction, only selected problems. Benedetto links these to his own numbering of cases, and also the terminology is so similar to that of Book XIII that we may suspect Biagio's words to have been somehow normalized.[58]

In spite of these doubts concerning Biagio's words, it is obvious that he does not build on Fibonacci. Nor does he build on Jacopo (or Gherardi, etc.), but he has so much in common with Jacopo and the algebraic problems of the *Trattato di tutta l'arte* that he must have taken his inspiration from the same environment.

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56 Ed. [Pieraccini 1983: 1]. Since Benedetto's text is repeatedly ungrammatical (also at the conditions of his time) and the translation therefore sometimes not certain, I render his own words:

> Voglio porre e chasi che scrive maestro Biaggio nel suo trattato di praticha, non perchè gli altri che ànno scritto non dichino assai chopiosamente, ma perchè lui fu, sechondo che scrive maestro Gratia de' Chastellani, el primo che a una buona praticha ridusse el detto trattato, inperochè, nel 1340, o circha, morì che, inanzi a'llui non c'era stato chi avesse, se non per un lunghо modo, trattato di questa praticha e inn'el suo tempo L. Pisano fiorì. E fu el detto maestro Biaggio maestro e chompagnio del gran maestro Pagholo.

57 Problems from Fibonacci's *Liber abbaci* are presented in Tuscan translation in the third chapter of Benedetto's Book XV [ed. Salomone 1984].

58 One example: Both books speak rarely about numbers being given "in ratio"; they say instead (e.g.) that the first is such part of the second as 2 of 3, and the third such part of the fourth as 3 of 4. This way of speaking is not too common ⟦and differs from what we known from Jacopo and Gherardi⟧.

The problems are dressed in ways we know from the Italians who were active in Provence (all of them, but not from Fibonacci's algebra): divided 10; numbers in given ratio; a number with $\frac{1}{3}$ and $\frac{1}{4}$ removed or added; together with commercial or traditional recreational dresses. Even the details are repeatedly similar to those of Jacopo.

Very similar is, for example, a problem about commercial travels. In one of Jacopo's problems [ed. Høyrup 2007a: 314*f*], somebody undertakes two commercial travels. In the first, he has a profit of 12; after the second, earning at the same rate, his total possession is 54. Biagio's [ed. Pieraccini 1983: 62*f*] corresponding numbers are 6 and 27 (that is, halved, and similarly without indication of the monetary unit); both give the double solution, and both explain that both solutions are valid.

The problems about compound interest, on the other hand, are different though related. Jacopo [ed. Høyrup 2007a: 210*f*] speaks about a capital that in two years grows from 100 £ to 150 £, and takes the rate of interest as his unknown. Biagio [ed. Pieraccini 1983: 69–72, 84–85] lets a capital of 100 £ grow over three years to 172 £ 16 ß or in four years from 10,000 £[59] to 14,641 £, and posits the value of 100 £ after one year as the *cosa* (whereby the problem becomes homogeneous). Contrary to Jacopo, Biagio makes use of the rule of three in order to find the value of the capital after two, three and four years.

Biagio avoids false rules like the ones we know from Gherardi. Once, however [ed. Pieraccini 1983: 25*f*], he is trapped, as also observed by Benedetto ("here our master gets lost"). For an equation that we would express $x^2+\sqrt{x}=18$, (x being "a number"), Biagio posits $\sqrt{x}$ as *cosa* (that is, we might say, he forms an equation $c^4+c=18$, which may be generalized as $c^4+\alpha c=\beta$), and states the rule

$$c^2=\sqrt{((\frac{\alpha}{2})^2)^2+\beta}-(\frac{\alpha}{2})^2,$$

which is indeed valid in this specific case but not in general.[60]

In Benedetto's version, Biagio makes use of the abbreviation ρ for *cosa*.[61] Since Benedetto does not use it himself (although he explains it for the benefit of readers of other treatises [ed. Salomone 1982: 30, cf. p. iv]), we may presume that it was already in Biagio's text – but it *could* have been inserted by an intermediate copyist. So much is certain, at least, that Biagio once [ed. Pieraccini 1983: 133] uses a formal fraction,

59 Biagio/Benedetto writes 1000 but calculates with 10000.

60 May we suppose that Biagio has played around with the solution to the equation $t^2+\alpha t=\beta$ until he found something fitting the actual case?

61 ρ emulates the reproduction in [Pieraccini 1983: xiii]. The same abbreviation is used in the *Tratato sopra l'arte della arismetricha* [ed. Franci & Pancanti 1988b], whose affinity with Biagio's *Trattato* is discussed below, in note 95. We find it in the variant  in the manuscript Vatican, Ottobon. lat. 3367. This manuscript is linked to that of Benedetto, cf. below, note 76. ⟦Regiomontanus, as we shall see, uses a variant.⟧

A variant of ρ is also employed in [Buteo 1559: 123 and *passim*].

namely $\frac{1\rho}{10\,m\,1\,\rho}$ (perhaps without abbreviation, $\frac{1 cosa}{10\,m\,1\,cosa}$). It arises as the outcome of a division and is afterwards dealt with as a usual fraction.

As we have just seen, Paolo dell'Abbaco is praised by Benedetto as "great". The compiler of the *Istratto* [ed. Arrighi 1964: 9] speaks of him as "venerabile". Even if we ascribe to him the *Istratto* (dubious, cf. note 49), and in particular if we deprive him of the authorship of the *Trattato di tutta l'arte,* extant sources do not justify such honours. Even though he ascribes to Paolo the *Trattato di tutta l'arte,* Van Egmond [1977: 16] presumed his fame to be due to his friendship with the *prominenti* of his time (no unknown phenomenon in the history of learning), even though his (unoriginal) astrological activity may have played a role. Without this ascription, Van Egmond's judgment seems to be even better founded. In any case, with or without the respectable but not outstanding *Istratto* he teaches us nothing fundamentally new about how abbacus culture developed during the second generation.

Further traces

We learn more from two writings from the "third generation": Dardi's *Aliabraa argibra* from 1344 and an anonymous *Trattato dell'alcibra amuchabile* from around 1365. In spite of these datings, it is adequate to begin with the latter.

It is divided into three parts. The first [ed. Simi 1994: 17–22] contains the sign rules and rules for the multiplication of monomials and binomials consisting of number and roots. These are shown in schemes for cross-multiplication, for example

```
5 e  piu  ℞ di 20
via
5 e  meno ℞ di 20
```

The second part consists of rules for the solution of algebraic cases provided with examples. It follows Jacopo's algebra so closely as far as this goes that it would be meaningful (and quite easy) to make a critical edition of the two texts. Yet, where Gherardi has additional examples, with one exception these are repeated by the *Trattato dell'alcibra amuchabile,* and so are most of Gherardi's false rules for irreducible cases. However, the agreement with Gherardi is *not* verbatim, and his only case with four terms is lacking. It appears that the compiler has used Jacopo's text or a faithful direct descendant but to share a source with Gherardi[62] – which leads us to the conclusion that Gherardi cannot be

62 Van Egmond [1978: 162] interprets the failing agreement as one of several indications that the Gherardi-manuscript must be a copy. However, many of the differences are of a kind that cannot be explained in this way. ⟦That the Jacopo algebra is the source for the *Trattato* and not a secondary insertion in the Vatican manuscript borrowed from the *Trattato* follows from a passage where Jacopo [ed. Høyrup 2007a: 312] postpones the transformation of $4\sqrt{54}$ into $\sqrt{864}$, leaving spaces instead. In the *Trattato* [ed. Simi 1994: 25], the calculation is completed.⟧

regarded as the inventor of the false rules. They must have originated after 1307 and before 1328 – or at least have reached the Italians during this period if their origin is to be looked for outside their environment.

The third part consists of 40 algebraic problems, dressed as "divided 10"; compound interest and money exchange; distribution of a given amount of money first among *N*, next among for instance *N*+5 persons – and once "give and take", not involving a square root of money as with Jacopo, but its square. It is exactly in a problem about such distribution of money that Gherardi introduces his hinted schematization of algebraic computation (see above, text after note 39). The *Trattato dell'alcibra amuchabile* goes further in the same direction, using formal fractions, for instance [ed. Simi 1994: 42]

$$\frac{100}{per\ una\ cosa}\ \frac{100}{per\ una\ cosa\ e\ 5}$$

explaining the meaning with the parallel $\frac{24}{4}+\frac{24}{6}$. The scheme for multiplication of binomials is used again here, now for the multiplication of algebraic binomials. All this confirms tendencies that are already visible in Gherardi's and Biagio's texts, as if they knew the ideas but had not yet appropriated them to the full.

The multiplicative composition of roots as we met it in Giovanni di Davizzo (above, note 55) also returns – but now these are real roots, not stand-ins for negative powers. A problem about compound interest asks for the taking of a fifth root, which is spoken of [ed. Simi 1994: 48] as "root of cube root" (while a sixth root is referred to as "root of root ⟨of root⟩). A shared origin of the two ideas is possible but far from convincing – multiplicative compositions of algebraic *powers,* where *cubo di censi/censo di cubi* designates the fifth and *cubo di cubi/censo di censo di censo* the sixth degree, might easily have produced parallel but mutually independent generalizations.

The *Aliabraa argibra* was, according to Mordechai Finzi's Hebrew translation, written in 1344 by master Dardi of Pisa (otherwise unidentified, just as Jacopo, Gherardi and Biagio) [Van Egmond 1983: 419].[63] While being much more extensive than the *Trattato*

63 I have used the following versions:

- Vatican, Chigi M.VIII.170 (~1395);
- Raffaella Franci's edition [2001] of Siena, I.VII.17 (~1470);
- Warren Van Egmond's personal transcription of the Arizona manuscript, written in Mantua in 1429, for which I thank him heartily.

The Vatican manuscript is not only earliest but also on the whole the best (even though the three manuscripts complement each other), cf. ⟦Høyrup 2007a: 170 n. 331]. ⟦It is written with Venetian orthography; since the abbreviation *ç* (see below) for *censo* corresponds to the northern writing *çenso* (in the 15th century changing into *zenso*), we may presume Dardi to have written in Venice or its region (in Pisa he would obviously not be identified as "from Pisa").⟧

dell'alcibra amuchabile, even this work falls in three parts, all of them remarkable. First of all comes a preface,[64] in which Dardi claims the meaning of the Arabic word *aliabraa* to be *quistione sottile,* "subtle question". I do not remember having seen this explanation in any source preceding Dardi. The preface also gives a geometric interpretation of the terms *cosa, censo* and *cubo.*

The first part teaches the multiplication of monomials and of binomials consisting of number and root – and as Giovanni di Davizzo also division and other operation, though in greater breadth. For multiplications, Dardi uses a diagram that is somewhat more elaborate than that of the *Trattato* – for example (Chigi ms. fol. 6^r), for $(3-\surd 5)\cdot(3-\surd 5)$

```
3 m̃ ℞ de 5
              14 m̃ ℞ de 180
3 m̃ ℞ de 5
```

In order to explain the division of 8 by $3+\surd 4$ (fol. 12^{v}), Dardi makes use of the rule of three: Since $(3+\surd 4)\cdot(3-\surd 4)=5$, $5/(3+\surd 4)=3-\surd 4$; $8/(3+\surd 4)$ must therefore be $(8\cdot[3-\surd 4])/5$.

Jacopo [ed. Høyrup 2007a: 307] and Biagio [ed. Pieraccini 1983: 84*f*] also make use of the rule of three as a tool for algebra. In contrast, neither the Latin algebras nor Arabic algebras known to me do so.

The appearance of $\surd 4$ in the example is no accident. Dardi makes repeated use of rational roots "as if they were irrationals"/"como s'elle fosse indiscrete" (Chigi manuscript fol. 3^{v}) – but he rarely takes advantage of the possibility offered by this choice to control the result.[65] Even this habit he has in common with one of his predecessors – namely Giovanni di Davizzo.

Dardi does not, like the *Trattato dell'alcibra amuchabile* and Giovanni di Davizzo, give the sign rules in summary form; ⟦[66]⟧ instead he offers (Chigi Ms. 4^{v}) "by means of number"/"per numero" (namely through the example $[10-2]\cdot[10-2]$) an intuitive proof that "less times less makes more"/"men via men fa più". In the end follows a diagram:

```
10 m̃ 2
        64
10 m̃ 2
```

64 The preface ist lost in the Vatican-manuscript and replaced by a new one in the Arizona- manuscript. But it is conserved in the Siena-manuscript.

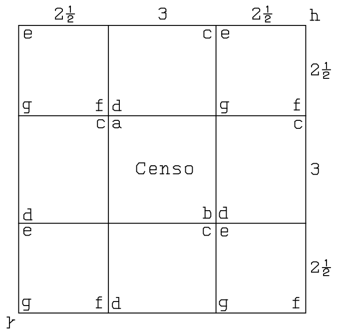
65 ⟦At an early point (Chigi ms. fol. 4^{v}), the multiplication of root by root is exemplified by the transformation $\surd 4\cdot\surd 9=\surd(4\cdot 9)=\surd 36$, and as "prova manifesta" it is explained that $\surd 4=2$, $\surd 9=3$, and $2\cdot 3=6=\surd 36$.

Surprising is a passage (Chigi ms. fol. 6^{r-v}) where $(3+\surd 4)\cdot(5-\surd 9)$ is first found "by mode of roots" to be $15+\surd 36-\surd 100-\surd 81$, and next, replacing $\surd 4$ by 2 and $\surd 9$ by 3, to be 10 – here, the two results are not pointed out to be identical!⟧

66 ⟦They *are* found in the Chigi manuscript on the otherwise blank fol. 153^{v}, at the very end of the first part, but in a different hand and with different abbreviations.⟧

As is well known, this proof (whether invented by Dardi or not[67]) is still repeated by Pacioli in the *Summa* [1494: 113^{r}].[68]

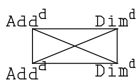
In the second part we find the definitions and the rules for solving the six basic algebraic first- and second-degree cases in the same order and the same non-normalized form as with Jacopo and other abbacus precursors. Different from all (Arabic, Latin and abbacus-) precursors, however, Dardi does not speak of the coefficient of (for example) the *censi* as *li censi* but as *la quantità de' censi* (the Siena manuscript often forgets and returns to the habitual terminology). A couple of times, as trace of his inspiration, Dardi refers to the number term as *dragma* – explaining it when it first occurs.



The diagram used in Dardi's first geometric proof

Dardi also differs from abbacus predecessors by giving geometrical proofs. They build on those of al-Khwārizmī, but their details diverge from everything one can find in Latin algebras, as well as those Arabic algebras I have inspected. This is illustrated already by the lettering of the diagram used for the first proof (for further discussion, see [Høyrup 2007a: 171*f*]): it has clearly been transmitted through an environment that was neither familiar with Euclidean conventions, nor with al-Khwārizmī's slightly different practice.[69] None the less, Dardi must have had some indirect connec-

67 The *Liber mahameleth* (Paris, Bibl. Nationale, Ms. Latin 7377A, fol. 109^{r}) offers in its discussion of the sign rules a somewhat similar diagram, though without numbers:



That suggests that Dardi may have borrowed his idea from somewhere but proves nothing.

68 And still in 1526, with the example $(14-4)\cdot(14-4)$, given by Feliciano da Lazesio [1526: K3^{r}]. An analysis of the proof with its variants can be found in [Høyrup 2007c: 471–474].

69 The Arizona manuscript starts its second part (fol. 23^{r}) with a reference to a *Tractado de algeble amughabala,* and slightly later (fol. 23^{v}) it speaks about what the reader further on will see in the "said book"/*dito libro;* one might believe this to be a reference to Dardi's source. The other two manuscripts, however, only say that we now begin the *trattato dell'arzebra* (Chigi orthography), and the cross-reference is to what the reader will see "in this book" (the one he holds in his hands). The inconsistent future tense in the Arizona manuscript shows the Chigi-Siena formulation to be original, and that Dardi does not refer to a treatise that is already at hand.

tion to the scholarly world: the preface explains in best high-scholastic style the "four causes" (*respetti*) of the book [ed. Franci 2001: 37].

Toward the end of the second part Dardi gives advice about zetetic (how to formulate a problem as an equation); finally he teaches how to multiply algebraic binomials and how *cosa, censo, cubo* and *censo di censo* are multiplied, divided and reduced (the latter operation is called *schizzare,* which also designates the reduction of fractions).

Already here Dardi uses the abbreviations *c* for *cosa* and *ç* for *censo* (in the interest of readability I shall use *Ç* in the following). He also makes use of the pseudo-fraction notation which we have encountered in the *Trattato di tutta l'arte,* and writes $\frac{10}{c}$ for "10 *c*". Both notations return in the third part, the "fraction" notation also expanded as ascending continued "fractions" (written left to right), in which $\frac{2}{c}\frac{1}{2}$ means $\frac{2}{c}$ plus $\frac{1}{2}$ of $\frac{1}{c}$ (that is, $2\,{}^1\!/_2c$). *Radice* is abbreviated ℞.

The commentary to the double solution of the fifth case contains phrases that come close to what Jacopo says about his example with valid double solution – see [Høyrup 2007a: 172]). Somehow, the two texts must draw on a shared source or tradition on this point.

The third part contains valid prescriptions[70] for the solution of 194 "regular" cases (that is, cases that can be reduced to homogeneous equations or equations of the first or second degree), and beyond that for 4 "irregular" cases, in which the rules are only valid for certain coefficients.

The reason Dardi can reach 194 regular cases is that he allows radicals as equation terms – not only, as Gherardi does once, roots of numbers but also of the powers of the unknown. As illustrative examples we may mention these (in mixed stenographic writing):

$$\alpha c+\beta\sqrt{K} = \gamma Ç \qquad\qquad \alpha c+\beta\sqrt{Ç} = \gamma Ç$$
$$\alpha ÇÇ = n+\sqrt[3]{m} \qquad\qquad \alpha ÇÇ+n+\sqrt[3]{m} = \beta Ç$$

K here stands for *cubo,* which Dardi does not abbreviate, *n* and *m* for *numero,* and Greek letters for coefficients (whose presence Dardi as other abbacus writers indicates by using the plural). Many of them may seem trivially reducible for us, but that is a delusion brought about by our modern algebraic symbolism. If he has not copied everything (a hypothesis without the least foundation), Dardi must have been a very skilled algebraist.

All regular cases are provided with one or two illustrative examples – those which possess a double solution however with three, as in Jacopo. All are pure number problems – almost half asking for two or three numbers in given ratio, more than a fourth for a number which fulfils the conditions of the equation, around 15% are of the type "divided 10".

70 Two mistakes are explained convincingly by Van Egmond [1983: 417] – see also below.

Then, as said, there are four irregular cases:

$$\alpha K+\beta Ç+\gamma c = n \qquad \alpha c+\beta Ç+\gamma ÇÇ = n+\delta K$$

$$\alpha ÇÇ+\beta K+\gamma Ç+\delta c = n \qquad \alpha c+\gamma ÇÇ = n+\beta Ç+\delta K$$

As Dardi says (Chigi manuscript, fol. 102r), these rules "fit only their question [*rason*] and the properties with which they are constructed [*ordenate*]". In spite of this deficiency they are included because they may turn up in certain questions. Each is provided with a single example.

The examples for the last two irregular cases are of type "divided 10", and might for that matter have been constructed by Dardi. Those for the former two are compound-interest problems as we know them from Jacopo and Biagio. Dardi first lets 100 £ grow to 150 £ over three years, then to 160 £ over four years. We remember that Jacopo also lets 100 £ grow to 150 £. Like Jacopo, the *Aliabraa argibra* posits the interest rate (in *denari* pro *soldo* and month) as *cosa* – but like Biagio, Dardi makes use of the rule of three in order to find the value of the capital after two, three and four years.

Since Dardi himself always constructs his examples as pure-number problems, at least the two former irregular rules and their appurtenant examples do not come from his hand – and then we may reasonably assume that the last two were also borrowed.

It is not difficult to reconstruct *how* the rules were constructed. In the first case, the procedure can be sketched like this: Let us take the interest rate to be *x soldi* per month and £ (when $\frac{12}{20}x$ £ per year and £); then we have that $100 \cdot (1+\frac{12}{20}x)^3 = 150$, whence

$$x = \sqrt[3]{(\frac{20}{12})^3 \cdot \frac{150}{100}} - \frac{20}{12} \quad .$$

But developing the equation we find

$$x^3+3\cdot\frac{20}{12}x^2+3\cdot(\frac{20}{12})^2x=(\frac{20}{12})^3\cdot\frac{150}{100}-(\frac{20}{12})^3 \quad .$$

We observe that $\frac{20}{12}$ can be found as the quotient between the coefficients of x and x^2, and that $(\frac{20}{12})^3 \cdot \frac{150}{100}$ arises as the sum of the number term and $(\frac{20}{12})^3$; and further, that x results if from this sum we extract the cube root and then subtract $\frac{20}{12}$ – which is exactly Dardi's rule. In order to boost his apparent ability and taking perhaps as his starting point a precursor for Biagio's problem (which may have combined Jacopo's numbers with the use of the rule of three), some abbacus master may in this way have transformed a homogeneous problem into an inhomogeneous equation with known solution. The other irregular cases run similarly.

The compound-interest problems (sometimes also the other irregular rules) turn up in not a few later abbacus books. I know of the following instances:

- Florence, Bibl. Naz., Fond. princ. II.III.198 (see [Franci 2002: 96*f*]);
- Parma, Bibl. Palatina, Ms. Pal. 312 [ed. Gregori & Grugnetti 1998: 24*f*];
- Palermo, Biblioteca Comunale, Ms. 2Qq E13; contains also the third irregular case (see [Franci 2002: 97*f*]);

– Vatican, Vat. lat. 4825 (Tomaso de Jachomo Lione), fol. 80^{r}–81^{r}; contains also the third case, but in strangely distorted form[71] (fol. 81^{v}–82^{r}), and an additional case *ziensi e chubi e ziensi di ziensi sono iguali a radize di numeri;*[72]

– probably also in Florence, Ricc. 2252, which according to [Franci 2002: 98] should be "quite similar" to the Palermo manuscript;

– Florence, Bibl. Naz., Palatino 567 (Raffaello Canacci, *Ragionamenti d'algebra* [ed. Procissi 1954: 441]); contains precisely the same four rules as given by Tomaso di Jachomo di Lione, yet without examples;

– Vatican, Vat. lat. 10488, fol. 93^{v}, brings Dardi's first irregular Rule without example; on fol. 94^{v} we also find the rules for "*cose* is equal to root of number "/"chose è ingual a radice di numero", "the number is equal to roots of the cose"/"i numero è ingual ale radice dele chose", "the *censi* equals root of number"/"le zensi è ingual a radice di numero" and "the number equals root of *censo*"/"li numero s'è ingual a radice di zenso";[73]

– Florence, Bibl. Naz., Palatino 575 [ed. Simi 1992: 53], rules alone; it also contains two of the extra rules of Vat. lat. 10488 and others of the same kind which are not in Dardi (for example "radici di censi di censi sono iguali al numero"), which supports the conclusion of note 73.

71 The text itself is unclear – one would believe that *radice di censi e di cose e di censi di censi* should mean,

$$\sqrt{\beta Ç}+\sqrt{\gamma c}+\sqrt{\alpha ÇÇ}$$

and then everything is just meaningless; but it is also possible that

$$\sqrt{\beta Ç+\gamma c+\alpha ÇÇ}$$

is intended, and then everything is just artificially distorted and the explanations that are given apparently irrelevant. The related belief that

$$\sqrt{a+\sqrt{b}} = \sqrt{a}+\sqrt{\sqrt{b}}$$

is found in the just-mentioned Parma manuscript [ed. Gregori & Grugnetti 1998: 115*f*].

It ought to be checked whether the same confusion also characterizes the Palermo manuscript.

72 As example (and certainly as basis for the rule for solving the case) another problem about compound interest serves: 50 £ grow in 2 years to $(50+\sqrt{484})$ £. The example contains writing or copying errors as well as erroneous calculations, but the underlying idea is as good or as bad as in Dardi's irregular cases.

It is a fair guess that the rule and the appurtenant example are also in the Palermo manuscript.

73 These four rules are also stated by Dardi (as no. 18, 17, 19 and 20), but they are too simple and too closely related to Gherardi's case $\alpha K = \sqrt{n}$ to suggest dependence on the *Aliabraa argibra*. A shared source or (rather) inspiration is much more likely.

– Finally, the first three rules with the usual examples turn up in Bento Fernandes's *Tratado da Arte de Arismética* from 1555 [Silva 2006], together with Tomaso de Jachomo Lione's extra case.

If really these manuscripts (and the printed book) should have drawn their inspirations from Dardi,[74] then it would be more than strange that they took over only these rules of limited validity without exhibiting genuine traces of Dardi's 194 regular cases [75]– all of them, moreover, without mentioning the restricted validity and without taking over Dardi's innovative terminology for coefficients. They must share a source with Dardi, which is thus earlier than 1344 and to be located in the "second generation". Dardi may himself have discovered their limited validity but in spite of this have included them in his book because of their occasional utility – namely in the competition with professional rivals, who might present *ragioni* depending on them in public contests.

Antonio de' Mazzinghi

According to Franci [1988a: 240], three encyclopedic treatises from Florence in abbacus tradition, all from around 1460,[76] consider Antonio de' Mazzinghi[77] as "the best algebraist of the 14th and 15th centuries"; on the next page it is repeated (as if it were the same statement) that they claim him to be the "best Florentine mathematician of all times". To judge from the corresponding excerpts from the encyclopediae in [Arrighi 1967a: 8], [Arrighi 2004/1968: 221] and [Arrighi 2004/1967: 194], Franci's interpretation of the three texts may be somewhat excessive, not least in view of the praise they give to other precursors; but they do admire Antonio highly – and for good reasons; that is confirmed by his *Fioretti*, which Arrighi published in [1967a] on the basis of Benedetto's *Trattato*.

All three encyclopediae quote problems not only from Antonio's hand but also material borrowed from Fibonacci and other abbacus authors – and Benedetto and the compiler of Palatino 573 al-Khwārizmī's algebra in Gerard of Cremona's translation (further translated into Tuscan). Already Antonio speaks with veneration about Fibonacci.[78]

74 That is claimed by Raffaella Franci [2002: 96–98].

75 Modern mathematicians might perhaps assume that these rules were the most interesting since they dealt with irregular cases. That would certainly overestimate the compilers of these manuscripts, who also repeat the completely wrong rules.⟦It also overlooks the presence of simple problems like those mentioned in note 73.⟧

76 Benedetto's *Trattato,* already mentioned; Vatican, Ottobon. lat. 3307; and Florence, Bibl. Naz., Palatino 573.

77 Probably born around 1355 and deceased c. 1385/86 – see [Ulivi 1996: 109–115].

78 Quoted in Ottobon. lat. 3307, ed. [Arrighi 2004/1968: 221].

It therefore suggest itself to ask whether he should somehow stand in a Fibonacci-tradition.[79]

First of all we must take into account that Benedetto paraphrases (as he had done with Biagio). We can therefore not be quite sure about Antonio's words; the numbering of cases, in particular, is the same as Benedetto's own numbering in the *Trattato*.⟦[80]⟧ As far as the substance is concerned, however, we can probably trust Benedetto.

Most of the 44 problems are pure-number problems, but compound interest, exchange and other commercial dresses are not missing. Among the latter we may mention a five–year version of interest with unknown rate [Arrighi 1967a: 38*f*], corresponding to what we have encountered in a two-year version in Jacopo and in three- and four-year versions in Biagio and Dardi. Antonio, however, finds the fifth roots of the factor of increase (here $2\frac{1}{2}$), as a modern calculator would do. He also makes use of the concept of a continuous proportion and does not mention the rule of three; in this problem his obvious starting point is thus the abbacus tradition as we know it, but he brings in tools he knows from elsewhere, as well as his own mathematical ability. Just before he has presented two related problems, about which it is said that "many say that they cannot be made" [Arrighi 1967a: 35*f*] – what must mean that these problems circulated as challenges. The former deals with the conversion of composite interest made up every nine months to the equivalent rate if made up yearly, the latter with a conversion from yearly making up of accounts to making up every eight months.[81]

The number problems also present us with familiar matter serving new sophistication. Dardi's example for the third irregular case ⟦[82]⟧ is a divided 10, where the product of the parts divided by their difference equals $\sqrt{18}$. Dardi posits one of the parts as *cosa*. Eliminating the root he then arrives at a fourth-degree equation. In Antonio [ed. Arrighi 1967a: 64] the quotient is more abstruse, namely $\sqrt{11}+\sqrt{12}$. He posits the two parts to be

[79] That was the thesis of Franci and Toti Rigatelli in [1985: 45], where even Biagio is seen as a representative of this tradition. The thesis is not repeated in [Franci 2002], and Biagio is here (p. 100) connected with the "Tradition of 22 Rules" (which is also taken to include Jacopo).

[80] ⟦Within the text Benedetto also speaks occasionally in the third person about Antonio [Arrighi 1967a: 28, 38, 47, 72]. In the last of these passages, Benedetto says that he expands Antonio's very concise explanation.⟧

[81] In the latter example Antonio sees that interest is added three times in two years, and that therefore two mean proportionals between initial and final value of the capital must be found. These sophisticated problems agree well with the claim that Antonio was the first to calculate interest tables (Florence, Bibl. Naz., Palatino 573, fol. 258^r, ed. [Arrighi 2004/1967: 183]).

[82] ⟦Arguably, as we have seen, borrowed from a predecessor belonging to the "second generation".⟧

5+*cosa* and 5–*cosa,* and (in agreement with normal abbacus aesthetics) reduces the problem to the somewhat unpleasant equation

$$25 = censo + \sqrt{44\,censi} + \sqrt{48\,censi}\ .$$

Antonio knows, however, that $\sqrt{44\,censi} + \sqrt{48\,censi}$ belongs to the genus *cose,* and indeed in number $\surd 44 + \surd 48$, and then everything is simple – ⟦*if only* we allow ourselves to consider $\surd 44 + \surd 48$ as a number⟧. Once again Antonio shows himself to be rooted in the abbacus tradition, and once again he is creative and not repetitive. Noteworthy is that Antonio (or Benedetto?) uses the term *raguagliare* as an equivalent of Arabic *muqābalah* (respectively Latin *opponere*), but in the unmistakeable sense of producing an equation reduced to its basic form (Jacopo [ed. Høyrup 2007a: 316] uses *raoguaglamento*). *Ragugliare* und *aguagliamento* appear similarly in Biagio [ed. Pieraccini 1983: 4, 21, 22].

Most of Antonio's problems I do not know from elsewhere, or they are to be found everywhere since al-Khwārizmī and are therefore useless for a discussion of origin.[83] One problem type is missing in the *Fioretti* which is otherwise copiously represented in all earlier and many later abbacus algebras: questions about numbers in given ratio. One may guess that this cheap trick to create apparent complexity did not please the eminent mathematician Antonio.[84]

A whole sequence of problems deal with continued proportions. That might make one think of the first part of Chapter XV of the *Liber abbaci*. The approaches of Fibonacci and Antonio are wholly different, however;⟦[85]⟧ nor have I noticed any shared problems. Beyond that we should remember that continued proportions also turn up occasionally in the earlier abbacus tradition; Jacopo thus has four correctly solved problems, dressed as problems about a geometrically increasing salary, one of which represents an irreducible cubic problem.[86] Since Antonio knew the *Liber abbaci,* it

83 That several of them can be found in Gilio da Siena's *Questioni d'algebra* from 1384 [ed. Franci 1983] is not informative, since Gilio may have learned from Antonio.

84 There is no reason to assume that Benedetto has left such problems out in his selection. In his own algebra (Book XIII of the *Trattato* [ed. Salomone 1982]) they abound.

85 ⟦An analysis of Fibonacci's approach is found in [Høyrup 2009b: 62–65] ⟦Article I.15⟧ and, in more detail, [Høyrup 2011: 89–92.⟧

86 If *a, b, c* and (in four-year problems) *d* represent the yearly wages, the problems are [ed. Høyrup 2007a: 324–331]

$$\begin{aligned} a+c &= 20, \quad b = 8 \\ a &= 15, \quad d = 60 \\ a+d &= 90, \quad b+c = 60 \\ a+c &= 20, \quad b+d = 30 \end{aligned}$$

At least in the third case, Jacopo does not seem to understand why his solution works; but Cardano [1539: II ii^{v}] seems to fare no better when he offers exactly the same formula.

is not to be excluded that his interest in continued proportions could be stimulated by Fibonacci; but he deals with the questions with methods which he knows from the *abbaco*-tradition (and occasionally *Elements* II).

He is certainly no carrier of a "Fibonacci tradition".

Varia from the 15th century

Analysis of the three encyclopediae that were listed in note 76 would lead to similar results.[87] To a large extent they build on the same sources – works from which they borrow excerpts, as well as sources for historical information, such as Gratia de' Chastellani (mentioned above, quotation before note 56). Beyond this, the two compilers of Ottobon. lat. 3307 and Palatino 573 are both pupils of a certain Domenico d'Agostino *vaiaio* (that is, tanner or leather-dealer – whether Domenico himself or an ancestor plied this trade remains a guess). It is thus not amazing that they are similar in many ways – and for the same reasons, it is impossible to consider them independent witnesses.

Instead of pursuing this issue, we may approach the developments of the 15th century thematically. Among such themes we shall take up the following (leaving out others that might also have been interesting):

- the terminology for and the dealing with powers;
- attempts, *really* to solve higher-degree equations;
- schemes and rudimentary symbolism.

Terminology for and dealing with powers

Already this heading is properly speaking misguided, since the idea of a continuing sequence of powers only took root gradually during the epoch we are looking at. Much better, since neutral, would be the concept of "cossic numbers" which we find in 16th-century writings.

In Jacopo and Dardi as in many others we find translations of the Arabic names: *numero*[88] – *cosa* – *censo* – *cubo* – *censo di censo;* in Giovanni di Davizzo further *censo*

⟦Jacopo does not speak of continued proportions but of wages increasing "at the same rate"⟧.

[87] In Florence, Bibl. Naz., Palatino 573 we find for instance (in the introduction to partnership calculations [ed. Arrighi 2004/1967: 180]) that Fibonacci in his Chapter 10 has treated many questions belonging to this domain, "but many more cases are written about and shown by master Gratia [de' Chastellani/JH]; and therefore we shall follow him in this part". When he gets to algebra, this author begins with al-Khwārizmī, as does Benedetto. Then follow [Arrighi 2004/1967: 191–194] rules for the arithmetic of powers, said to follow Antonio, and various examples taken over from the compiler's teacher ("il vaiaio"), from Fibonacci, from Luca di Matteo, from Giovanni di Bartolo and from Antonio.

[88] Dardi also uses *dramma* twice – obviously taken over together with the geometric proofs,

di cubo and its alias *cubo di censo* – *cubo di cubo* – and then, after leaps, *censo di censo di censo di censo* (8th power) and *cubo di cubo di cubo di cubo* (12th power). Those among these authors who understood the reducibility of cases like $\alpha ÇÇ+\beta K = \gamma Ç$ and did not copy mindlessly must somehow have understood the sequence as a geometric series. In the early years, however, nobody explains that.

As we see, the system is multiplicative, which has the advantage that all powers can be expressed easily (though not unequivocally). But it has three disadvantages:

- Firstly, the names for higher powers soon become rather confusing;
- secondly, and worse, the names of the powers cannot be connected to the names of the corresponding roots –if not by mistaken imitation as in Giovanni di Davizzo (see text before note 55), who employs the cube root of the cube root as sixth instead of ninth root (however in a connection where "roots" stand for negative powers).[89] Dardi's two mistakes (see note 70) are consequences of this problem: Dardi has no names for the fifth and the seventh roots, and he therefore speaks of them as *radice cuba* and *radice della radice,* even though in general he has no difficulty with the embedding of roots;⟦[90]⟧
- thirdly, a contradiction inheres in the multiplicative interpretation. That is seen as soon as we speak of *censo* or *cubo of something* (and thereby, in modern terms, treat them as functions); *cubo* of 2 must then be 8, and *cubo* of *cubo* of 2 therefore 8^3, 512 and not 64: the Latin and Italian genitive construction allows no escape if taken to the letter.[91]

since theword is used within these.

89 Similarly, we remember, the *Trattato dell'alcibra amuchabile* speaks of the (true) fifth root as "root of cube root" (above, paragraph before note 63).

⟦We might try to explain the multiplicative composition of roots by saying that just as powers are understood as independent entities and not as functions (see imminently), even roots are considered entities that can be given whatever name one pleases. We would be mistaken: roots were taken of numbers in all algebra texts, and thus *were* functions; taking the cube root of 512 would give 8, and taking again the cube root one would get 1, the ninth root of 512. Even *within* the horizon of the time, multiplicative composition of roots was nonsense.⟧

90 ⟦On the notion of "embedding", see Article II.4 and II.4.⟧

91 Neither Diophantos's κυβόκυβος nor Arabic *ka'b ka'b* (see al-Karajī's list of the Arabic terms in [Woepcke 1853: 48]) lead to this problem. The Greek composition is no more multiplicative than the composite term "Anglo-American". *Ka'b ka'b* is a genitive construction, but the function of the Arabic genitive is so much broader than that of Indo-European languages that it does not contradict the multiplicative interpretation (Ulrich Rebstock, personal communication).

According to the textbooks of dialectic, contradiction engenders development – but, often forgotten by the dear old books, neither smooth nor predictable development. Thus also here. We see that in Antonio de' Mazzinghi. In the manuscript Pal. 573 [ed. Arrighi 2004/1967: 191] the compiler quotes Antonio for saying "in the beginning of his treatise"/"nel principio del suo trattate":

> *Cosa* is in this part [of the treatise] a hidden quantity; *censo* is the square of the said *cosa; cubo* is the multiplication of the *cosa* in the *censo; censo di censo* is the square of the *censo,* or the multiplication of the *cosa* in the *cubo.* And observe that the terms of algebra are all in continued proportion; such as: *cosa, censo, cubo, censo di censo, cubo relato, cubo di cubo,* etc.

In the problem about compounding of interest over five years, Antonio speaks of the fifth root as *radice relata.* He has thus solved the problem about the fifth root – the term to be used, as well as the relation to the corresponding power. He has not overcome its cause, as we see in the name *cubo di cubo* for the sixth power – but he has neutralized it by making the continued proportion explicit.

I am aware of no source that might inform us about Antonio's way to deal with negative powers (or about whether he deals with them at all). So much is certain, however, that he would not repeat Giovanni di Davizzo's nonsense – not only because "he could not have done so as a good mathematician" (always a dubious argument when applied *a posteriori*), but also because his treatment of the fifth power and the fifth root does not fit it.

Two of the encyclopediae from which we know Antonio take over his names for powers and roots (Benedetto and Palatino 573). Ottobon. lat. 3307 speaks instead of the fifth power as *censo di cubo* alternating with *cubo di censo* (fol. 304^{r}).

For the multiplication and division of powers, both Benedetto [ed. Salomone 1982: 20–25] and Ottobon. lat. 3307 [fol. 304^{r}–305^{r}] offer something new. For example, in Benedetto[92]

[…]

92 […]

e a moltiplichare cose per chubi fanno censi di censi, chome dicendo moltiplicha 6 chose via 8 chubi, fanno 48 censi di censo

[…]

partendo chose per censi ne viene rotto nominato da chose, chome partendo 48 chose per 8 censi ne viene $\frac{6}{1\ \textit{chosa}}$

[…]

partendo cubi per censi di censo ne viene rotto nominato da chosa, chome dicendo parti 48 chubi per 8 censi ⟨di censo⟩ diremo questo rotto, cioè $\frac{6}{1\ \textit{chosa}}$

[…]

and multiplying *cose* by *cubi* make *censi di censi,* as saying, multiply 6 *cose* by 8 *cubi,* they make 48 *censi di censo*

[…]

dividing *cose* by *censi,* from this comes a fraction denominated by *cose,* as, dividing *cose* by *censi*, from this comes $\frac{6}{1\ \textit{chosa}}$

[…]

dividing *cubi* by *censi di censo*, from this comes a fraction denominated by *cosa*, as saying divide 48 *cubi* by 8 *censi* ⟨*di censo*⟩, we shall say this fraction, that is, $\frac{6}{1\ \textit{chosa}}$

[…].

In Ottobon. lat. 3307 we find the parallel[93]

[…]

multiplying *cose* times *cubi* make *censi di censo,* as saying 6 *cose* times 8 *cubi* make 48 *censi di censo*

[…]

dividing *cose* by *censi*, from that comes a fraction denominated by *censo*, that is $\frac{8\ \textit{chosa}}{1\ \textit{censo}}$

[…]

dividing *cubi* by *censi di censo,* a fraction comes denominated by *censo di censi,* that is $\frac{8\ \textit{chubi}}{1\ \textit{censi di censo}}$, which reduce are $\frac{8\ \ \textit{dramme}}{1\ \ \ \ \textit{chosa}}$

[…].

There can be no doubt that the two texts draw on a shared source. That this source cannot be neither Antonio nor Benedetto follows among other things from the use of *chubo di censo* for the fifth power in Ottobon. lat. 3307 (but also from the necessity to find, for example, $\frac{8\ \textit{dramme}}{1\ \textit{chosa}}$ via $\frac{8\ \textit{Chubi}}{1\ \textit{censi di censo}}$, and from the use of the archaic phrase *multiplicare via*. Since Benedetto's text appears to be the older of the two (1463 against "c. 1465" [Van Egmond 1980: 190, 213]), Ottobon. lat. 3307 can probably also be excluded as a source for Benedetto.[94] I have not been able to find any positive evidence for the

93 […]

multiplichando chose via chubi fanno censi di censo chome a dire 6 chose via 8 chubi fanno 48 censi di censo

[…]

partendo chose per censi ne viene rotto nominato da censo, chome partendo 48 chose per 6 censi ne viene questo rotto, cioè $\frac{8\ \ \textit{chosa}}{1\ \ \textit{censo}}$

[…]

partendo chubi per censi di censo vienne rotto nominato da censo di censi, chomo a partire 48 chubi per 6 censi di censo vienne questo rotto, cioè, $\frac{8\ \ \textit{chubi}}{1\ \ \textit{censi di censo}}$, che schizzato sono $\frac{8\ \ \textit{dramme}}{1\ \ \textit{Chosa}}$

[…]

94 A final argument for this can be found in the introductions to algebra of the two manuscripts. Ottobon. lat. 3307 (fol. 303^{v}) contains a shortened and distorted version of the quotation

identity of the source.

Outside the line connecting Antonio and the three encyclopediae we find the *Tratato sopra l'arte della arismetricha,* probably written in Florence around 1390 [ed. Franci & Pancanti 1988b].[95] As we shall see, the author is mathematically competent (he seems indeed to be more than a mere compiler). All the more significant are the difficulties he encounters in the naming of powers [ed. Franci & Pancanti 1988b: 3–6].

Like Antonio, but in more detail, the present author explains the meanings of *cosa, censo,* and so forth. He starts with the explanation that the *cosa* is nothing but a "positing" (*posizione*), which is made in many problems; that this positing can be a quantity of number, of time, of cubits, and so on; and that "in itself it has not the name of a distinct

from Guglielmo de Lunis; Benedetto instead has it in full and without distortion, see below, note 114.

⟦The dating argument, on the other hand, is shaky. The dating of Ottobon. lat. 3307 is based on watermarks, which allows a rather wide margin.⟧

95 Instead, the manuscript belongs without doubt to the same broad tradition as Jacopo, Dardi, Giovanni di Davizzo, Biagio and the *Trattato dell'alcibra amuchabile*. It contains the same 20 algebraic cases as Jacopo, and in addition the two missing biquadratic cases (and where Giovanni inverts right and left in one of Jacopo's equations, the present manuscript does the same); it gives the same diagram for the multiplication of binomials as Dardi; like Biagio it uses the expression "tal parte sia il primo del sechondo …" to state that two numbers are in given ratio. Finally, where Dardi and Giovanni deal with rational roots "as if they were irrational" without taking serious advantage of this, the present author explains the reason.

It is not excluded that this author has had direct access to Biagio's *Trattato*. Like Biagio he lets 10000 £ grow to 14641 £ in four years; like Biagio he posits the value of 100 £ after one year as *cosa – and like Biagio he writes 1000 £ instead of 10000 £ by mistake* (cf. note 59).

However, he must also *share* a source with Biagio. In the algebraic solution of a problem about a geometrically increasing salary, Biagio [ed. Pieraccini 1983: 89*f*] uses *cosa* about an unknown amount of money, the present manuscript [ed. Franci & Pancanti 1988b: 80–82] instead *censo.* The author does not know that *censo* (as *māl*) originally stood for an a amount of money – he finds the *cosa,* and in consequence needs to square in order to get the *censo.* He must therefore have drawn on a source which is closer to the Arabic tradition than Biagio; the agreement of formulations is, on the other hand, so great, that Biagio must have used either the same or a closely related source. Almost the same problem is found in Jacopo, only with all parameters halved; Jacopo also solves it without recourse to *cosa*-algebra.

⟦Second thoughts: it is not to be excluded that Biagio's original text also had *censo*, and that Benedetto corrected him silently. In that case, the detour *censo-cosa-censo* may also have been in Biagio's text, or it may have been introduced by the author of the Florence manuscript.⟧

quantity, but it certainly produces a distinct quantity".[96] Apparently original – and fully transparent. Further:

> A *cosa* multiplied in itself makes a root, which is called a *censo,* so that it is the same to say a *censo,* as a quantity that has a root, about a number multiplied by itself, so that it would be to say that when the *cosa* brings forth 4 in number, then the *censo* shall produce the square of that number; that is, that which 4 makes multiplied by itself, that is, 16, will be the value of the *censo,* so it is seen that 4 is the root of 16. So it comes that the *cosa* is said to be the root of the *censo,* so that it is the same to say *censo* as root of number.[97]

It seems strange that the *censo* should *be* a root because it *has* the *cosa* as its root. However, that this formulation is no mistake but intended can be seen in what follows, where similar formulations are repeated in the explanations of the higher powers.[98]

Several causes may interact. First perhaps badly understood influence from the Arabic tradition – an influence that always makes itself felt when geometric proofs are given. In these, the *cosa* is identified with the root; that may possibly have inspired the generalization that all powers are designated "roots". Second, interaction with the terminology that is used for the conversions (often found in abbacus algebra) $a = \sqrt{a^2}$ and $a = \sqrt[3]{a^3}$: to bring a to root respectively cube root, "recare a radice/radice cubica" (where the outcome is stated merely as a^2 respectively a^3). 6 "brought to cube root" is thus 216. Finally, some interaction with Giovanni di Davizzo's "roots" is not to be completely

96 Probably an echo of Sacrobosco's words about the zero (which "signifies nothing, but gives signification to others"), regularly quoted in abbacus books.

97 "una chosa in se medesimo multipricata'ffa una radice, la quale si chiama uno censo, sichè tanto vol dire uno censo quanto dire una quantità ch'à radice nata d'uno numero in sé multipricato, sichome sarebe a dire se'lla chosa producerà 4 per numero, il censo de' producere il quadrato della chosa; cioè quello che farà il 4 in sé multipricato, coè le 16 sarà la valuta del censo, sichè veduto è che'14 ene la radice del 16, chosì intervienne che'lla chosa si dice essere radice del censo, sichè tant'è dire censo quanto radice di numero."

98 For instance, "When the *cosa* is worth 5 […], will the *censo di censo* be worth 625; so that, taking the root of the root, which will be 5 and be equal to the *cosa.* So that it is the same to say *censo di censo* as to say root of root"/"se la cosa varà 5 […], il censo del censo varà 625; sichome chi pigliasse la radice della radice di 625, che sarebe 5 ed aguagliasi alla chosa. Adunque tant'è a dire censi di censo quant'è a dire radice di radice"; and later, "if the *cosa* should be worth 6, that the *cubo di censo* will be worth 7776, and there are some who call this root *radice* relata"/"se'lla chosa valesse 6, che'l chubo del censo varà 7776, e sono alquanti che questa chosì fatta radice chiamano radice relata".

excluded.

As we see, the powers are at first named in the usual multiplicative way, as *cosa – censo – cubo – censo di censo – cubo di censo,* and the stepwise ascent by means of multiplication is explained. By means of proportions it is (for example) shown that *censo* times *censo* equals *cubo* times *cosa.*

In the inherited multiplicative terminology (as we find it for example in Giovanni di Davizzo), *cubo di censo* and *censo di cubo* are identical. Not here. Surprisingly, *censo di cubo* is understood through embedding, that is, as the sixth power (namely, as explained, because the square root of 729 is 27, and the cube root of 27 is 3). Here we are at the limit of the author's comprehension; slightly later it is indeed explained that *cubo* times *cubo* - once again the sixth power – "makes *cubo di cubo,* that is, cube root of cube root"/"farà chubo di chubo coè radice chubicha di radice chubica".

In Luca Pacioli's *Summa* [1494: I, 67^{v}], in a presentation of the powers (*gradi*) as named *modernamente,* we find both systems further developed – both, because "as many regions, so many usages"/"tante terre, tante usanze", and "as many heads, so many opinions"/"tot capita: tot sensus". In the margin are listed the numbered "root" designations, abbreviations of the customary names and these names written in full:

℞ 1^{a}	n^{o}	*numero*
℞ 2^{a}	co	*cosa*
℞ 3^{a}	ce	*censo*
℞ 4^{a}	cu	*cubo*
℞ 5^{a}	ce.ce	*censo de censo*
℞ 6^{a}	p^{o} r^{o}	*primo relato*
℞ 7^{a}	ce.cu	*censo de cubo e anche cubo de censo*
℞ 8^{a}	2^{o}r^{o}	*secundo relato*
℞ 9^{a}	ce.ce.ce.	*censo de censo de censo*
[…]	[…]	*[…]*
℞ 29^{a}	ce.ce.2^{o}r^{o}	*censo de censo de secundo relato*
℞ 30^{a}	[9^{o}] r^{o}	*nono relato*

On fol. 143$^{r–v}$, root designations and abbreviations return in a tabulation of the products of powers, now combined with the powers of 2 used for identification (℞ 11^{a} thus identified by 1024).

In the right column above we see that all names are now produced by embedding[99] – and all those that cannot be composed like that appear as first/second/

99 That does not prevent (genuine) roots with the exception of the fifth root to be named according to the multiplicative principle. Here we find the following sequence:

℞ ($^{2}\surd$) – ℞.cuba ($^{3}\surd$) – ℞℞ ($^{4}\surd$) – ℞.relata ($^{5}\surd$) –℞.cuba de ℞.cuba ($^{6}\surd$) – ℞℞℞.cuba ($^{7}\surd$) – 6℞℞℞.cuba de ℞.cuba ($^{8}\surd$) –

third/…/ninth *relato.* That is not very practical, but even the arithmetization of the root names is inconvenient, among other things because the inventor has not dared like Chuquet[100] to pigeonhole number as "level 0". Therefore, multiplication of powers does not correspond to any addition of "exponents".

Attempts, *really* to solve higher-degree equations

The above-mentioned *Tratato sopra l'arte della arismetricha* contains neither false not "irregular" algebraic rules. Its author is none the less interested in the solution of irreducible higher-degree equations. After the presentation of the "22 rules" he explains [ed. Franci & Pancanti 1988b: 98] that certain problems can be solved in other ways; but for this purpose, however, one needs "other roots than those that are normally spoken about, that is, other roots than square roots and cube roots". Among these others he only mentions "cube root with an added number". For example, "the cube root of 44 with 5 added" equals 4, because $4^3 = 44 + 5\cdot 4$; in general, expressed in symbols, the cube root of n with added α is equal to c when, with our usual abbreviations,

$$K = n + \alpha c$$

(that is, when $c^3 = n+\alpha c$). That is one of the cases for which Gherardi gave a false rule, and in itself the cube root with an added number only serves to provide the unknown solution to this equation with a name; this is hardly to be considered a genuine step forward, and the author declares indeed that an [integer] value of this root often cannot be found, for which reason it is not very useful.

We should not forget, however, that abbacus algebra (in contrast to abbacus geometry) did not give approximate values for irrational square and cube roots; "the square root of n" was thus nothing but a name for "the solution to the equation $Ç = n$". Later mathematics present us with many parallels – as long as we prepare no tables, elliptic functions are no different.

If we leave aside the possibility of numerical approximation, square roots and elliptic functions are only mathematically fruitful because they allow us to establish networks of theoretical connections – and such connections are indeed established by our author. He does not use the cube root with addition to solve a single problem of type $K = n+\alpha c$; what he does is to prescribe how to transform equations of the types $K+\beta Ç = m$, $K = \beta Ç +m$ and $\beta Ç = K+m$ so as to obtain the form $K = n+\alpha c$, and only then does he apply the cube root with addition. He also shows [ed. Franci & Pancanti 1988b: 102] that solutions may exist when n is a debt, that is, when it is negative.[101]

℞.cuba de ℞.cuba de ℞.cuba ($\sqrt[9]{\ }$) – ℞℞℞.cuba de ℞.cuba ($\sqrt[10]{\ }$)

100 Ed. [Marre 1880: 737].

101 In the actual case he shows that the cube root of "debito 80" with addition of 108 equals

He does not explain the origin of his transformation prescriptions, but the details reveal it. For convenience we may write the equation K+β$Ç = m$ in symbols as

$$t^3 + 3at^2 = m$$

Completion gives

$$(t+a)^3 = m + a^3 + 3a^2 t = m + a^3 + 3a^2(t+a) - 3a^2 \cdot a$$

– exactly as the rule of the manuscript states, in this order and without any reduction of the expression $m + a^3 + 3a^2(t+a) - 3a^2 \cdot a$.

The author speaks, as quoted, about a plurality of particular roots, but specifies only this one. Other abbacus books refer to a *radice pronica,* which belongs to the same family. It is defined in two different ways. Pacioli [1494: I, 115^{v}] has this:

> By *radice pronica* one normally understands a number multiplied in itself, and above it set the root of the said number, of this sum that number is called *radice pronica.* As 9 multiplied in itself makes 81, and above it set the root of 9, which is 3, it makes 84. The *radice pronica* is called 9 by practitioners.[102]

That has little immediate connection with the notion of a "pronic number" – a number that can be written $n \cdot (n+1)$. To this, through generalization, the number 3 would seem more to the point, since $3 \cdot (3^3 + 1) = 84$. And one finds indeed, in Gilio da Siena [ed. Franci 1983: 18*f*] as well as in Pierpaolo Muscharello's *Algorismus* from 1478 [ed. Chiarini et al 1972: 163], that the pronic root of 84 equals 3.[103]

Benedetto [ed. Pieraccini 1983: 26] may provide us with the solution. At least he shows us that the pronic root served the solution of irreducible equations. In connection with the problem where Biagio "gets lost" (see page 313) he mentions the pronic root without explaining what it is. The problem (in our notation $x^2 + \sqrt{x} = 18$) has as solution Pacioli's pronic root of 18; if it is transformed into $y^4 + y = 18$, then the solution is Gilio's pronic root.

Pacioli may have heard about solutions by means of particular roots, but find that they are of no interest. After his observation [1494: I, 150^{r}] that so far only such equations could be solved according to general rules where the three powers are separated by equal intervals he observes in passing that other types can only be solved occasionally and *a tastoni* ("by groping"). He was right – in the sense that these particular solutions led to

10, since $108 \cdot 10 + (-80) = 10^3$.

102 "Per radice pronica communamente se intende numero multiplicato in se e sopra suo quadrato posto la radice de ditto numero de questa summa quel numero sia ditta radice pronica. Commo 9 multiplicato in se fa 81 e sopra 81 posto la radice de 9 che e 3 fa 84. La radice pronica de 84 sia ditta da pratici 9".

103 Pacioli's 9, on the other hand, is found in the manuscript Florence, Bibl. Naz., Palatino 575 [ed. Simi 1992: 20*f*]. Even there, the use of this particular root goes unexplained.

nothing. With hindsight he was also wrong – in the sense that the solution of single equation types by del Ferro, Tartaglia and Cardano were eventually to provide a *general* solution of the third-degree equation thanks to the *transformations* that went together with these particular solutions.

Schemes and rudimentary symbolism

Diagrams for the multiplication of binomials, abbreviations for *cosa* and *censo* and the pseudo-fraction notation for multiples of *cosa* and *censo* already appeared before 1350; formal fractions were also to appear soon. From the end of the 14th century, more suggestions of symbolic operation turn up.⟦[104]⟧

The *Tratato sopra l'arte della arismetricha* [ed. Franci & Pancanti 1988b] was mentioned several times above. In this text we find not only the abbreviations ℞ (*radice*), p (*più*), m (*meno*), ρ (*cosa*) and *c* (censo)[105], and the diagram for the multiplication of binomials which we know since Dardi. For the multiplication of longer polynomials (for example, *cose* + number + $\sqrt{}$ number) a multiplication *a chaselle* is taught [ed. Franci & Pancanti 1988b: 11], which emulates the multiplication of multi-digit numbers in vertical columns[106] Similar schemes are found not only in later abbacus writings (e.g., Vatican, Ottobon. lat. 3307, fol. 331^{v}, and in Raffaello Canacci, ed. [Procissi 1954: 316–322]), but also for example in Stifel's *Arithmetica integra* [1544: fol. 123–125 and *passim*], in Scheubel [1551: 3^{v}*ff*], in Peletier [1554: 15-22] and in Ramus [1560: A iii^{r}].

In Vatican, Ottobon. lat. 3307, fol. 331^{r} we find in the margin an interesting combination of operation with formal fractions and rudimentary schematization. It illustrates a problem

$$\frac{100}{1\rho}+\frac{100}{1\rho-7}=40$$

(these fractions, without "+" and "=", are also written within the main text). The solution goes via the transformation

$$\frac{100\rho-100\cdot(p-7)}{(1\rho)\cdot(1\rho-7)}=\frac{100\rho-(100\rho-700)}{1\sigma-7\rho}=40,$$

104 ⟦A much more thorough exposition of the slow development towards symbolism can be found in Article II.13.⟧

105 The abbreviations p, m, ρ und *c* are not provided with strokes or arcs, as they often are – ⟦neither in the edition nor, as far as I can see on a bad copy of a secondary copy of a microfilm, in the manuscript.⟧

106 Apparently the method which Giovanni de' Danti [ed. Arrighi 1987a: 16] speaks of as "sechondo l'arte nova"/"according to the new art". In contrast, the Maghreb style (according to the Jerba manuscript as reproduced in Abdeljaouad [2002: 47]) has the oblique columns which de' Danti [ed. Arrighi 1987a: 16] designates "a brichuocolo secondo l'arte vecchia"/"in crumbs according to the old art" or "a schacchiera"/"in chessboard". The difference is too small not to suspect Maghreb inspirations, in particular because the manuscript exhibits rather direct Arabic influence in other ways (cf. above, note 95).

whence 200ρ+700 = 40σ+280ρ; in the margin, this solution is written like this (the *censo* stands as σ; ρ is actually written ꝑ):

$$\begin{array}{lll}
100\rho & & \\
\underline{100\rho} & \underline{700} & \\
\underline{200\rho} & \underline{700} & \\
\quad 1\sigma & \underline{7\rho____} & 40 \\
 & & \\
200\rho & 700_____40\sigma\ \langle 280\rho\rangle &
\end{array}$$

(⟨280ρ⟩, omitted in the last line, is in the body of the text). The strokes before 40 and 40σ seem to have the function of equation signs; it would be more adequate, however, to speak of a general "confrontation sign" – in the margin of fol. 338^r,

$$\frac{3000}{1\rho\ 5000}\ \text{------}\ \frac{4000}{1\rho\ 6000}$$

means that the first of two commercial partners has $\frac{3000}{1\rho+5000}$, the second $\frac{4000}{1\rho+6000}$.

We find the same "equation sign" in a manuscript written by Luca Pacioli in 1478 (Vatican, Vat. lat. 3129), e.g. on fol. 67^v. We must assume that its use was widespread.

In the Regiomontanus-Bianchini correspondence [ed. Curtze 1902: 278] appears the problem

$$\frac{100}{1\rho}+\frac{100}{1\rho+8}=40.$$

Regiomontanus makes exactly the same schematic calculation; he writes (ρ, *cosa,* has now been extended to ꝑ; σ has become ꝺ for *census*):

$$\begin{array}{l}
\quad\ \frac{100}{\rho}\quad \frac{100}{1\ \rho\ et\ 8} \\
\quad\ 100\ \rho\ \text{et}\ 800 \\
\quad\ \underline{100\ \rho\qquad\quad} \\
\quad \frac{200\ \rho\ \text{et}\ 800}{1\ \rho\ \text{et}\ 8\ \sigma}__40 \\
40\ \sigma\ \text{et}\ 320\ \rho____200\ \rho\ \text{et}\ 800 \\
40\ \sigma\ \text{et}\ 120\ \rho____800 \\
1\ \sigma\ \text{et}\quad 3\ \rho_____20
\end{array}$$

There can be no doubt that Regiomontanus has copied fairly precisely from a source belonging to the same family as Ottobon. lat. 3307, even though he makes use of his own slightly modified abbreviation for *census* (which is not necessarily his own invention).

Often, Regiomontanus writes ꝑ and ꝺ superscript after the coefficient. The same is done with *cosa* by Pacioli in Vatican, Vat. lat. 3129, while his □ (for *censo*) is written above the coefficient. Already in 1424, Vat. Lat. 10488 (e.g., fol. 36^v–37^v, 38^v, 92^{r–v}) had

written both *cosa* and □ (the latter alternating with *cen*) above the coefficient.[107] Ultimately, the notation is probably an inheritance from Maghreb algebra – cf. for example [Tropfke/Vogel et al 1980: 376].

The question of Guglielmo de Lunis

Book XIII of Benedetto's *Trattato,* "La reghola de algebra amuchabale", begins (after a heading of three lines) with these words[108]

> Let us render thanks to the Almighty, thus begins the text of the Arabic Aghabar in the rule of Geber which we call algebra. Which rule of algebra, according to the translator Guglielmo de Lunis, embraces these 7 names, that is, *geber* [*al-jabr*]*, elmelchel* [perhaps *al-muqābalah*[109]], *elchal* [*al-qabilah*[110]], *elchelif* [perhaps *al-khalās,* "liberation"/"riddance"[111]], *elfatiar* [?], *diffarelburam* [*difa*^{c} *al-burhān,* "defense of the demonstration"[112]], *eltermen* [*al-tamām*[113]]. Which names according to Guglielmo are interpreted thus: *Geber* is as much as to say *recuperatione* because, as will be understood in the following, the case will be solved by the recuperation of two equal parts. *Elmelchel* is as much as to say *exenplo,* or *asomigliamento,* because the solution of the case is found by making similar [*asomigliare*] the quantity that is posited to the given case. *Elchal* is as much as to say *opositione,* because of two quantities that are found one is opposed [*oposta*] to the other, and when there are not two opposed quantities, then the case is insolvable. *Elchelif* is as much as to say *dispositione* because, even though there are two opposed quantities, if they are not disposed for the application of the rules, the

107 Chuquet's superscript numbers written after the coefficient [ed. Marre 1880: 737], where for example $12.^2$ stands for 12 *censo,* probably reflects the same habit.

108 Manuscript Siena, L.IV.21, fol. 368^r, edition [Salomone 1982: 1].

⟦The Siena manuscript is Benedetto's autograph: sometimes numerical calculations for problems can be seen to have been made in the margin before the text body was written (the structure of one such page is shown in Article II.13).⟧

109 Thus Ulrich Rebstock (personal communication). Paul Kunitzsch (personal communication) instead proposes *al-mit̠hāl* (whose general meaning is "parable", "allegory", "example", etc.); phonetically this makes less sense, but it fits the following, where *asomigliare* would correspond to Arabic *mat̠hala* ("be similar", "imitate", "compare", etc.).

110 The disappearance of the "b" shows that Iberian pronunciation of Arabic is rendered (Ulrich Rebstock, personal communication).

111 Thus Ulrich Rebstock. Paul Kunitzsch proposes *al-ta'līf* ("formation", "composition" etc.).

112 Proposed by Ulrich Rebstock.

113 "Completeness", "perfection", and similarly. Proposed by Paul Kunitzsch.

> case would be outside the rules and therefore there is a need that the quantities be ordered [*disposte*].[114]

In Raffaello Canacci [ed. Procissi 1954: 302] we find a similar but not identical passage:

> The role of algebra, which rule Guglielmo a Lunis has translated from the Arabic into our language. And the said Guglielmo and others say was composed by an Arabic master of truly great insight, even though some others say it was one whose name is Geber, to which Leonardo Pisano says that *algebra muchalbile* is the interpretation of the rule in this language. The rest of the said rules begins, Let us give thanks[115] to the Almighty, and following the said Guglielmo the said rule in the said language contains seven names, that is, seven parts, called like this in the said language, *geber, el melchel, elchal, elchelis, elfatiar, diffarel buran, eltiemen* […].[116]

In the following, Canacci's quotation from Guglielmo agrees with what we have seen in Benedetto, except that Canacci (like Jacopo as well as Biagio and Antonio as rendered

114 "Rendiamo gratie all'Altissimo, chosì chomincia el testo de l'Aghabar arabico nella reghola del geber, la quale noi diciamo algebra. La quale reghola d'algebra, secondo Guglielmo de Lunis traslatore, inporta di questi 7 nomi cioè: *geber, el melchel, elchal, elchelif, elfatiar, diffar al buram, eltermen.* E' quali nomi, second el detto Guglielmo, sono chosì interpretati. *Geber* è quanto a dire recuperatione inperoché, chome per lo seguente si chonprenderà, nelle recuperatione di 2 parti iguali s'asolve il chaso. *Elmelchel* è quanto a dire exemplo, overo asomigliamento, inperoché l'asolutione de' chasi si truova per asomigliare la quantità posta al chaso dato. *Elchal* è quanto a dire opositione perché, di 2 quantità trovate, l'una è oposta all'altra, e quando non sono 2 quantità oposte il chaso è insolubile. *Elchelif* è quanto a dire dispositione inperoché, benché le 2 quantità oposte sieno e non abbino dispositione a uso delle reghole, lo chaso sarebbe fuori delle reghole e però abisognia le quantità disposte".

The facsimile of New York, Columbia, Ms. Plimpton 189 ⟦another manuscript of Benedetto's *Trattato*⟧ in [Smith 1908: plate opposite to p. 462] is very similar. The orthography of both manuscripts for the transcription of all Arabic terms is identical with one exception (*eltermem* instead of *eltermen*).

115 Canacci has a meaningless "andano gratie" here, where Benedetto has the certainly correct "rendiamo gratie".

116 "*La regola* dell'algibra, la quale reghola Ghuolelmo a Lunis l'a traslato d'arabicho a nostra linghua, e sechondo el detto Ghuolelmo e altri dichono questa esser chomposta da uno maestro arabo invero di grande intelligenza, benché alchuno altri dichono esser stati uno del quale il nome era *Geber,* a che Lionardo Pisano dice che algebra muchalbile è'lla interpretatione della reghola in quella linghua. El resto della detta reghola inchominca *andano* gratie all'altissimo. E'ssecondo el ditto Ghuolelmo la ditta reghola in quella linghua chontiene sette nomi, coè sette partì chosi nelle ditta linghua nominati: *Geber, elmelchel, elchal, elchelis, elfatiar diffarel buran, eltiemen* […]".

by Benedetto) speaks of *aghuaglamento* instead of *asomigliamento*.⟦[117]⟧ The better ways to render *al-khalās* (if that is really the origin) as *elchelis* and *al-tamām* as *eltiemen* show that Canacci does not know Guglielmo's text from Benedetto (who writes *elchelif* and *eltermen*), or at least not through Benedetto alone.

In [1521: fol. 71^{v}],[118] Francesco Ghaligai repeats Canacci's introduction, but with explicit reference to Benedetto. Like Benedetto he writes *elchelif* and *eltermen,* and he translates *elmelchel* as *assimigliamento.* He must thus know Benedetto's text as well as that of Canacci, or a precursor.

It has repeatedly been assumed that Guglielmo's translation should be identical with the Latin text in Oxford, Bodleian, Ms. Lyell 52.[119] Several decisive arguments speak against this. Firstly, the Lyell manuscript uses *restaurare* throughout, never *recuperare.*[120] Secondly, as already observed by Karpinski [1910: 211], it contains no traces of Arabic terminology, while all sources for the existence of a translation from Guglielmo's hand[121] present precisely that and nothing else.[122]

There *may* be one more trace of Guglielmo's translation. In the last, algebraic part of the *Liber abbaci,* Fibonacci sometimes replaces his usual term *census* by the vernacular *avere.* Fibonacci may well be responsible himself for this – after all, he also uses *viadium*/

117 ⟦This agrees better with Rebstock's than with Kunitzsch's interpretation of *elmelchel,* see note 109.⟧

118 Excerpted and discussed in [Karpinski 1910: 209].

119 Without taking over the thesis, Wolfgang Kaunzner [1985: 10–14] gives a convenient overview. [Kaunzner 1986] is a critical edition of the text.

120 *Recuperatio* is found, as Karpinski [1910: 211] observes, in the title of New York, Columbia, Ms. Plimpton 188, fol. 73^{r} (an edited version of Gerard's translation written in 1456, at one moment in Regiomontanus's possession; cf. [Hughes 1986: 230] and [Folkerts 2006: V, 190]). Like the ensuing explanation of *almuchabala* as *oppositio, recuperatio* is clearly a secondary admix, possibly stemming from knowledge about the Guglielmo-translation, but certainly no witness. Four other Gerard manuscripts from the 15th and 16th centuries have been similarly "improved", see [Hughes 1986: 229–231].

121 With the exception of a gloss ascribing erroneously a manuscript of the Gerard-translation to Guglielmo [Hughes 1986: 223]). ⟦As Hughes observes, this is only evidence of awareness that Guglielmo had made a translation.⟧

122 For the same reason we must reject Jacques Sesiano's proposal [1993: 322*f*] to identify Guglielmo's algebra translation with the Latin translation of Abū Kāmil's algebra. There is thus no reason to doubt the identification of our Guglielmo with the (rather poorly) known translator from the early 13th century. The above-mentioned mis-attribution, apparently from the 13th century, also makes Sesiano's assumption chronologically impossible.

viagium a few times, as well as *guise/guice* (Latinised genitives of *guisa,* a word which is first testified in Italian in the 13th century but which was probably borrowed from Provençal *guiza*). But it is also possible that Fibonacci had recourse to an already existing terminology – that is, to a translation, either into an Italian dialect or into Latin coloured by the vernacular. *If* such a translation existed, then the guess is close at hand that Guglielmo was responsible for it – not least because he, like Fibonacci, was somehow connected to the Sicilian court.

The connection to German space

There is no doubt that the emergence of the *Rechenmeister* tradition and the development of German and Latin algebra in southern-German area in the later 15th century was inspired by Italy. That is obvious to everybody who opens the German writings and knows the Italian texts. It is more debatable through which and through how many channels the Italian inspiration flowed, and also *precisely* how much came from outside and how much was created in German area.

That Regiomontanus was interested in Italian (and, like the Florentine encyclopedias, in al-Khwārizmī's) algebra is familiar – see [Folkerts 2006: V, VI, XII]. Above we have seen how close he kept to the Italian model in the Bianchini-correspondence, with formal calculations, confrontation-/equation sign and superscript abbreviations. But there is more. His "minus-sign" is often interpreted as *ī* (that is, *in*) provided with a curlicue *us*, *ιq̄*. [123] The shapes in a photographic reproduction of the calculations for the correspondence with Bianchini [Cajori 1928: 96] – *m̄ȝ*, at times rather *m̄ȝ* – look more like a variant of the traditional Italian shape *m̄*,[124]; a page from the manuscript Plimpton 188[125], on the other hand, uses the shape  twice, four times however the shape  (*mīus*). On the same page, the abbreviation for *res* appears superscript once, but several times written on the line (more often in full writing, as *cosa*). On the whole, Regiomontanus appears to use abbreviations and formal calculations neither more nor less systematically than his Italian contemporaries. As regards schemes used for polynomial arithmetic he is clearly "behind" many Italians, but probably for the simple reason that he only uses algebra as a tool and offers no systematic presentation – abbacus writers when presenting the fully developed schemes always do so in systematic introductions to algebra.

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123 Thus [Tropfke/Vogel et al 1980: 206]; [Vogel 1954: plate VI].

124 This form is found for instance in the Chigi-manuscript of Dardi's *Aliabraa argibra.* Kinship with the equally traditional abbreviation *m̄ȝ* seems to be out of the question.

125 Fol. 85r, reproduced in high resolutions on http://columbia.edu/cgi-bin/dlo?obj=ds Columbia-NY.NNC-RBML.6662&size=large (accessed 19 March 2018)⟦This part of the manuscript was written by Regiomontanus, probably around 1456 [Folkerts 2006: XII, 4].⟧

However, Regiomontanus is not the only channel – and to which extent he is a direct channel is hard to know. In Friedrich Amann we thus find much more than he could have learned in a short meeting with the young Regiomontanus (as proposed in [Folkerts 2006: VIII, 414]) – including computation with formal fractions like $\frac{32\ \textit{res et}\ 45}{1\ \textit{census et}\ 3\ \textit{res}}$ [ed. Curtze 1895: 59]. Moreover, the heading on the manuscript page referred to in note 125 is *Regule de cosa et censo sex sunt capitula, per que omnis computatio solet calculari,* while Amann [ed. Curtze 1895: 50] writes more Italian (*Regule dela cose secundum 6 capitola*) – even Regiomontanus's *manuscript* is thus not what Amann has used.

In his edition of the "deutsche Algebra" from 1481 Vogel already pointed out [1981: 10] that it draws on several sources. Two observations can now be added to those made by Vogel. Firstly, that the "strange writing of the cossic symbols [for the algebraic powers] under a fraction line" is precisely what we first encountered in the *Trattato di tutta l'arte* and in Dardi; it is found neither in Regiomontanus nor in Amann. Secondly, the writing "℞ of ℞" for the fourth power is also clearly related to the "root" notation of the *Tratato sopra l'arte della arismetricha* (see text around note 96) but absent from earlier algebraic writings from German area. Thirdly, the idea to provide equations with a double solution with three examples agrees with Jacopo and Dardi but not with German predecessors.

Finally we may notice that Regiomontanus as well as Amann and the "deutsche Algebra" write *census,* a Tuscan spelling. The "Latin Algebra" [ed. Wappler 1887], on the contrary, makes use of an abbreviation that reflects the spelling *zenso* – that is, of a spelling that was current in the northern arc from Genoa over Milan to Venice). Precisely in this "Latin algebra" something interesting happens: while neither Regiomontanus nor the Italians apply their abbreviations systematically – that is, as a symbolism – the "Latin algebra" is systematic (as also the short Latin addition to Robert of Chester's Latin translation of al-Khwārizmī [ed. Hughes 1989: 67], presumably written in German area in the mid-15th century). One may imagine that the change of language, entailing loss of the everyday connotations of the terminology, has contributed to this technification; however, the integration with university teaching may also have played a role. Yet this – and many related questions – can only be decided after more thorough scrutiny of the sources.

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Chapter 14 (Article I.13)
The "Unknown Heritage" – Trace of a Forgotten Locus of Mathematical Sophistication

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Small corrections of style made tacitly
Additions touching the substance in ⟦...⟧

J. Høyrup, *Selected Essays on Pre- and Early Modern Mathematical Practice*,
https://doi.org/10.1007/978-3-030-19258-7_14

Abstract

The "unknown heritage" is the name usually given to a problem type in whose archetype a father leaves to his first son 1 monetary unit and $^1/_n$ (n usually being 7 or 10) of what remains, to the second 2 units and $^1/_n$ of what remains, etc. In the end all sons get the same, and nothing remains.

The earliest known occurrence is in Fibonacci's *Liber abbaci*, which also contains a number of much more sophisticated versions, together with a partial algebraic solution for one of these and rules for all which do not follow from his algebraic calculation. The next time the problem turns up is in Planudes's late 13th-c. *Calculus according to the Indians, Called the Great*. After that the simple problem type turns up regularly in Provençal, Italian and Byzantine sources. It seems never to appear in Arabic or Indian writings, although two Arabic texts (one from c. 1190) contain more regular problems where the number of shares is given; they are clearly derived from the type known from European and Byzantine works, not its source. The sophisticated versions turn up again in Barthélemy de Romans' *Compendy de la praticque des nombres* (c. 1467) and, apparently inspired from there, in the appendix to Nicolas Chuquet's *Triparty* (1484). Apart from a single trace in *Cardano's Practica arithmetice et mensurandi singularis*, the sophisticated versions never surface again, but the simple version spreads for a while to German practical arithmetic and, more persistently, to French polite recreational mathematics.

Close examination of the texts shows that Barthélemy cannot have drawn his familiarity with the sophisticated rules from Fibonacci. It also suggests that the simple version is originally either a classical, strictly Greek or Hellenistic, or a medieval Byzantine invention; and that the sophisticated versions must have been developed before Fibonacci within an environment (located in Byzantium, Provence, or possibly in Sicily?) of which all direct traces have been lost, but whose mathematical level must have been quite advanced. ⟦An addendum drawing on new evidence argues that the invention was made in the 12th century in Al-Andalus.⟧

Contents

In memoriam
MARSHALL CLAGETT
and
DAVID PINGREE

A starting point

In the final collection of mixed problems in the Vatican manuscript of Jacopo da Firenze's *Tractatus algorismi* (Vat. Lat. 4826),[1] we find the following (fol. 54^v-55^r):

> Io vo a uno giardino, et giongho a'ppede de una melarancia. Et coglione una. Et poi coglio el decimo del rimanente. Poi vene un altro dopo me, et coglene doy, et anchora el decimo de rimanente. Poi vene un altro et coglene 3, et anchora el decimo de rimanente. Poi vene un altro et coglene 4 et el decimo de rimanente. Et così venghono molti. Poi quello che vene da sezzo, cioè dercto, coglie tucte quelle che retrova. Et non ve ne trova nè più né meno che abiamo auti li altri. Et tanto ne colze l'uno quante l'altro. Et tanti homini quanti erano, tante melarancie ebbe per uno. Vo' sapere quanti homini forono, et quante melarancie colseno per uno, et quante ne colzeno fra tucti quanti. Fa cosi, tray uno de 10, resta 9, et 9 homini forono, et 9 melarancie colseno per uno. Et colzero in tucto 81 melarancie. Et se la voli provare, fa così.

			El primo ne colze i, restano
80.	El decimo	è octo,	et ày che illo n'ebbe 9, restano
72.	El secondo	2, restano 70, el decimo è 7,	et ebe ne 9, restano
63.	El terzo	3, restano 60, el decimo è 6,	et ebe ne 9, restano
54.	El quarto	4, restano 50, el decimo è 5,	et ebe ne 9, restano
45.	El quinto	5, restano 40, el decimo è 4,	et ebe ne 9, restano
36.	El sexto	6, restano 30, el decimo è 3,	et ebe ne 9, restano
27.	El sectimo	7, restano 20, el decimo è 2,	et ebe ne 9, restano
18.	Ell'octavo	8, restano 10, el decimo è 1,	et ebe ne 9, restano
9.	El nono, cioè quello da sezzo, colze quelle 9, né più né meno, che non ve n'erano più. Siché vedi che ella è bene facta. Et sta bene. Et cosi se fano le simiglianti ragioni.		

In literal translation:[2]

[1] This treatise was written in Montpellier in 1307. In spite of its Latin title and incipit, it is written in Tuscan (the orthography being somewhat coloured by the Provençal linguistic environment).

Two other manuscripts claim to contain the same treatise, Florence, Ricc. 2236 (undated) and Milan, Trivulziana, Ms. 90 (c. 1410) (see [Van Egmond 1980: 148, 166]; Van Egmond's dating of the Florence copy is misleading, since it merely repeats the date of Jacopo's original as it appears in the incipit). The Vatican manuscript is from c. 1450 but a meticulous copy of a meticulous copy, and linguistic and textual as well as mathematical homogeneity shows the Vatican manuscript to be quite close to the common archetype for all three manuscripts, whereas the other two descend from an abbreviated adaptation, probably adjusted to the curriculum of an abbacus school – see [Høyrup 2007: 12–23]. The final collection of supplementary problems ⟦including the present one⟧ is absent from the Florence and Milan manuscripts, as are the chapters on algebra.

[2] As all translations in the following where no translator is identified, this one is due to the present

> I go to a garden, and come to the foot of an orange. And I pick one of them. And then I pick the tenth of the remainder. Then comes another after me, and picks two of them, and again the tenth of the remainder. Then comes another and picks 3 of them, and again the tenth of the remainder. Then comes another and picks 4 of them and the tenth of the remainder. And thus come many. Then the one who comes last, that is, behind, picks all that which he finds left. And finds by this neither more nor less than we others got. And one picked as much as the other. And as many men as there were, so many oranges each one got. I want to know how many men there were, and how many oranges they picked (each) one, and how many they picked all together. Do thus, detract one from 10, 9 is left, and there were 9 men, and 9 oranges (each) one picked. And they picked in all 81 oranges. And if you want to verify it, do thus,

		the first picked 1 of them, left
80.	The tenth	is eight, and you have that this one got 9, left
72.	The second	2, left 70, the tenth is 7, and he got 9, left
63.	The third	3, left 60, the tenth is 6, and he got 9, left
54.	The fourth	4, left 50, the tenth is 5, and he got 9, left
45.	The fifth	5, left 40, the tenth is 4, and he got 9, left
36.	The sixth	6, left 30, the tenth is 3, and he got 9, left
27.	The seventh	7, left 20, the tenth is 2, and he got 9, left
18.	The eighth	8, left 10, the tenth is 1, and he got 9, left
9.	The ninth, that is, the last one, picked these 9, neither more nor less, as there were no more. So that you see that it is well done. And it goes well. And thus are done the similar computations.	

A modern reader encountering a problem of this kind for the first time is usually stunned. As Euler says about it in his didactical *Élémens d'algebre* [1774: 489], "this question is of a quite particular nature, and therefore deserves attention".[3] As we see, the rule works – still in Euler's words, "it fortunately happens that …" – and it does so for any aliquot part $\phi={}^{1}/_{n}$. Moreover, as we shall see, if only the absolutely defined contributions form an arithmetical progression and ϕ is any fraction and not too large it still works, in the sense that one can still find an initial amount T such that all shares except the last one are equal.

Jacopo probably did not know why his rule functioned – when he knows, he is fond of giving pedagogical explanations, and here he only presents the complete calculation as a verification. However, the original inventor must have known why, one does not stumble on the structure in question by accident.

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author.

[3] [Tropfke/Vogel et al 1980: 582–588] discusses it under the general heading of *Schachtelaufgaben*, "[nested] box problems", together with problems with the structure

$$(\ldots((x + a_1)\phi_1 + a_2)\phi_2 + \ldots)\phi_{n+1} + a_n = R\,,$$

admitting however that it is of "a particular kind". Actually, the mathematical structure is wholly different. Normal box problems are easily solved by stepwise reverse calculation; in the present case, this is impossible.

We cannot know where the idea came from,[4] but the arrangement of dots in Figure 1 (reduced for convenience to $\phi={}^1/_6$) is a possibility:

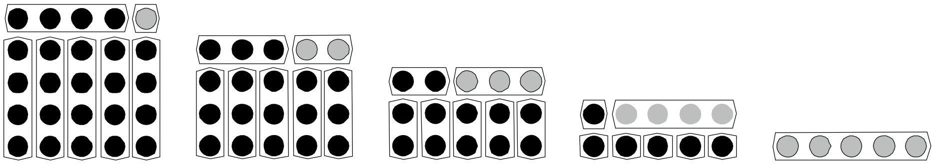

Figure 1

If we remove 1 small (grey) dot from a square pattern of $n\times n$ dots, what is left can be grouped as n+1 strips of n–1 (black) dots. Removal of one of these strips (${}^1/_{n+1}$ of what is left) leaves a rectangular system of $n\times(n$–1) dots. Removing 2 small (grey) dots from this rectangle leaves n+1 strips of n–2 (black) dots, and removing one of these strips (still ${}^1/_{n+1}$ of the remainder) leaves a rectangle $n\times(n$–2) dots, etc. In symbols, and for p = 0, 1, 2, …,

$$(n-p)\times n=(n-[p+1])\times n+(p+1)+(n-[p+1]).$$

This is obviously an argument of the same kind as those based on pebble counters or *psephoi* used in early classical arithmetic. Contemporary readers accustomed to working on paper with a square grid may prefer the version in Figure 2, in which the summary to the right shows that the square is divided into the sum of two triangular numbers, one of which – namely 1+2+…+n – consists of the absolutely and the other – namely (n–1)+(n–2)+…+1 – of the relatively defined contributions.

4 A direct arithmetical *solution* is possible, but it could never give rise to the idea. It only works because the overdetermined problem does possess a solution, and it cannot be generalized to similar but different situations; moreover, it only finds the sole *possible* solution without showing that this is indeed a solution:

Since the last visitor of the garden (say, no. N) leaves nothing, the remainder r_N of which he takes the fraction ${}^1/_d$ must be 0 (if not, $(1-{}^1/_d)r_N$ would be left over. But since each visitor picks as many apples as his number before taking ${}^1/_d$ of the remainder, no. N gets N apples, and so therefore do all the others. But the second-last visitor (no. $N-1$) only picks $N-1$ apples before taking the fraction ${}^1/_d$ of the remainder $r_{N\text{-}1}$. Therefore this fraction must be 1 (he has already picked $N-1$, but should have N). Further, he leaves N to the last visitor. In consequence r_{N-1} is N+1. ${}^{N-1}/_d$ is thus 1, whence N must be $d-1$.

No source or historian's discussion I have looked at contains the least hint that its author had seen this.

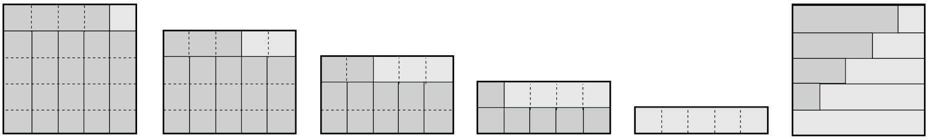

Figure 2

Leonardo Fibonacci

We shall return to the reasons that this argument may indeed be the one from which the problem was constructed. Initially, however, we shall have to look at other texts where problems of this kind turn up – beginning with the earliest specimen, Leonardo Fibonacci's *Liber abbaci* from 1228 [ed. Boncompagni 1857: 279–281]. Fibonacci first presents his reader with two versions dealing with an unknown heritage distributed to an unknown number of heirs (this, not fruit-picking, is the habitual dress for the problem), next with a sequence of structurally similar but more sophisticated pure-number problems.[5]

Fibonacci's first inheritance version shares the structure of Jacopo's fruit-picking problem (apart from the fraction being $^1/_7$ and the number of sons thus 6, each receiving 6 bezants). The method is also similar. However, Fibonacci does not give the information that the amount which each son receives equals the total number of sons, although his explanation presupposes it (which allows us to conclude that his source for the problem was even closer to Jacopo):

> For the seventh which he gave to every one you retain 7; from which detract 1, 6 remain; and so many were his sons; which 6 multiplied by itself makes 36; and so many were his bezants.

In the second inheritance problem, each son receives *first* $^1/_7$ of what is at disposal and *afterwards* respectively 1 bezant, 2 bezants, etc.; it is then stated (but no argument given) that 6 sons get 7 bezants each. The reader must be expected to identify 7 as the denominator of the fraction, and 6 as 7–1. Finally Fibonacci explains that if the absolutely defined contributions in the two cases had been instead 3 bezants, 6 bezants, etc., the number of sons would still have been 6, and the total possession 3×36 bezants and 3×42 bezants, respectively.

Even in the case where the fraction is taken first, a "proof" by means of pebble counters is possible – see Figure 3. Here, a number $n\times(n+1)$ is split into two triangular

[5] A full French translation of this part of the *Liber abbaci* is found in [Spiesser 2003: 711–718]. [Sigler 2002: 399–401] contains an English translation.

numbers of order n, one of which represents the successive absolutely defined, the other the relatively defined contributions.

In the ensuing pure-number versions, the fractions and absolute contributions are more intricate. In order to facilitate the further discussion we shall henceforth designate by $(\alpha, \varepsilon|\phi)$ the type where absolutely defined contributions $\alpha+\varepsilon i$ ($i = 0, 1, \ldots$) are taken first, and a fraction ϕ of the remainder afterwards; $(\phi|\alpha, \varepsilon)$ designates the type where a fraction ϕ of what is at disposal is taken first and absolutely defined contributions $\alpha+\varepsilon i$ ($i = 0, 1, \ldots$) afterwards. In this notation, Fibonacci's problems are the following (the inheritance problems are in the left column, the other columns contain the number problems):

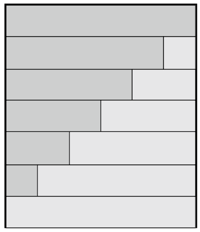

Figure 3

$(1,1\|\frac{1}{7})$	$(1,1\|\frac{2}{11})$	$(2,3\|\frac{6}{31})$	$(3,2\|\frac{5}{19})$
$(\frac{1}{7}\|1,1)$	$(4,4\|\frac{2}{11})$	$(\frac{6}{31}\|2,3)$	$(\frac{5}{19}\|3,2)$
$(3,3\|\frac{1}{7})$	$(\frac{2}{11}\|1,1)$		
$(\frac{1}{7}\|3,3)$	$(\frac{2}{11}\|4,4)$		

The problems in the second column (where $\alpha = \varepsilon$) are treated by the same rules as those of the first column, in the sense that the fraction $^2/_{11}$ is tacitly dealt with as $^1/_{5½}$. The trick is not explained, however, we only find the prescription (for the first problem)

> Divide 11 by 2, which are above 11, $5^1/_2$ result; from which take away 1, $4^1/_2$ remain; and so many were the shares; which multiplied together, were $20^1/_4$ for the divided number.

For the problem $(2,3|^6/_{31})$ in the third column, the solution is found by means of the *regula recta*, that is, first-degree rhetorical algebra in which the unknown is referred to as *a thing*. Fibonacci puts the number to be divided equal to this *thing*, and finds by successive computation the first two shares, which he knows to be equal. Resolving the resulting equation he finds the number to be $T = 56^1/_4$, the number of shares to be $N = 4^1/_2$, and each share $\Delta = 12^1/_2$. He has thus found the only possible solution, but his algebraic computation does not show that the subsequent shares will be as required, that is, that this *is* indeed a solution. Fibonacci makes no hint at this deficiency, but he performs a complete calculation step by step (similar to Jacopo's) which verifies that the first four shares are $12^1/_2$, after which $6^1/_4$ remains for the final $^1/_2$-share. Finally Fibonacci claims to "extract" the following rule from the calculation[6] ($\phi = {}^p/_q$):

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6 Obviously Fibonacci uses the specific numbers belonging to the problem when stating the rule, but since he identifies each number in the rule by pointing to its role in the computation, the

$$(1^{a}) \qquad T=\frac{[(\varepsilon-\alpha)q+(q-p)\alpha]\cdot(q-p)}{p^2},$$

$$(1^{b}) \qquad N=\frac{(\varepsilon-\alpha)q+(q-p)\alpha}{\varepsilon p},$$

$$(1^{c}) \qquad \Delta=\frac{\varepsilon(q-p)}{p}.$$

At closer inspection, the rule turns out *not* to be extracted. If one follows the algebraic calculation step by step, it leads to

$$(2a) \qquad T=\frac{q^2(\alpha+\varepsilon)-(q-p)q\alpha-(q-p)p\alpha-(\alpha+\varepsilon)pq}{p^2}$$

which (by means which were at Fibonacci’s disposal) can be transformed into

$$(2^{a}*) \qquad T=\frac{[q(\alpha+\varepsilon)-(p+q)\alpha]\cdot(q-p)}{p^2}$$

but not in any obvious way into the rule which Fibonacci pretends to extract – if anything, attempts at further manipulation would rather lead to the reduction

$$(3a) \qquad T=\frac{[\varepsilon q-\alpha p]\cdot(q-p)}{p^2}.$$

The implication appears to be that Fibonacci adopted a rule whose fundament he did not know, and that he pretended it to be a consequence of his own (correct but partial) solution.[7]

This inference is corroborated by what happens when Fibonacci treats the problem $(3,2|^{5}/_{19})$. Here, α cannot be subtracted from ε (the outcome is negative), and therefore Fibonacci (who knew well how to make elementary operations with negative numbers even though he did not fully accept them) replaces (1) by

symbolic formulae map his rule precisely and unambiguously.

[7] This case of minor fraud is not without parallel in Fibonacci’s works. In the *Pratica geometrie* [ed. Boncompagni 1862: 66], Fibonacci copies from Gerard of Cremona’s translation of Abū Bakr’s *Liber mensurationum* [ed. Busard 1968: 94] a fallacious solution to a rectangle problem $\ell - w = \alpha$, $\sqsubset\!\sqsupset(\ell,w) = \beta$ (the words are so close that Fibonacci’s copying is beyond question, here as in several other places). Afterwards Fibonacci undertakes an explication by means of algebra (which Abū Bakr does not give in this case even though he does so in others). When arriving to the point where the mistake becomes evident (but where Fibonacci appears not to know how it has come about nor how to repair it) he concludes the exposition with the words “et cetera”.

$$(4^a) \qquad T = \frac{[(q-p)\alpha-(\alpha\text{-}\varepsilon)q]\cdot(q-p)}{p^2} \ ,$$

$$(4^b) \qquad N = \frac{(q-p)\alpha-(\alpha-\varepsilon)q}{\varepsilon p} \ ,$$

$$(4^c) \qquad \Delta = \frac{\varepsilon(q-p)}{p} \ .$$

If Fibonacci himself had reduced the algebraic solution (2^a), why would he have chosen an expression which is neither fully reduced nor valid for all cases? Neither (2^a) nor ($2^{a}*$) nor (3^a) depends on whether $\alpha<\varepsilon$ or $\alpha>\varepsilon$.

For the case ($^6/_{31}|2,3$), Fibonacci just states and applies these rules

$$(5^a) \qquad T = \frac{[(\varepsilon-\alpha)q+(q-p)\alpha]\cdot q}{p^2}$$

$$(5^b) \qquad N = \frac{(\varepsilon-\alpha)q+(q-p)\alpha}{\varepsilon p} \ ,$$

$$(5^c) \qquad \Delta = \frac{\varepsilon q}{p} \ ,$$

and for ($^5/_{19},|3,2$)

$$(6^a) \qquad T = \frac{[(q=p)\alpha-(\alpha\text{-}\varepsilon)q]\cdot q}{p^2}$$

$$(6^b) \qquad N = \frac{(q\text{-}p)\alpha-(\alpha-\varepsilon)q}{\varepsilon p} \ ,$$

$$(6^c) \qquad \Delta = \frac{\varepsilon q}{p} \ .$$

Once again, if (1^a) had really resulted from the algebraic solution, why should (5) and (6) be set forth without being derived from the pertinent algebraic operations (which are evidently not the same as before)?

We must conclude that not only what we shall henceforth call the “simple versions” of the problem (Jacopo’s, and those in the first column of the scheme on p. 353, those where $\varepsilon = \alpha$ and where ϕ is an aliquot part) and their rules were “around” but also the much more sophisticated versions and rules in columns 2–4 of the scheme. The question then presents itself, *where*?

As is well known, most of the “recreational” problems found in the *Liber abbaci* and in the various abbacus treatises are widely disseminated, turning up in Indian, Persian and Arabic problem collections, some also in the *Greek Anthology*, in Ananias of Shirak’s collection, or in the Carolingian *Propositiones ad acuendos iuvenes*, some even in ancient

or medieval Chinese treatises. Not so in the present case. [Tropfke/Vogel et al 1980: 587*f*] and [Singmaster 2000] only list Byzantine and (Christian-)Occidental occurrences, and I have not been able to find parallel examples in sources from elsewhere, whether published before or after 1980. (Two Arabic "corrected" versions and their implications are discussed below, p. 366)

Maximos Planudes

Three Byzantine occurrences are known: one in Maximos Planudes's late 13th-century *Calculus According to the Indians*, *Called the Great* [ed., trans. Allard 1981: 191–194]; another one in a problem collection from the early 14th century [ed., trans. Vogel 1968: 102–105]; the last one in Elia Misrachi's book on arithmetic from c. 1500 ([ed., trans. Wertheim 1896: 59*f*]. The cases treated are $(1,1|^{1}/_{7})$ (all three) and $(1,1|\,^{1}/_{10})$ (Elia Misrachi alone). All follow the simple rule we know from Jacopo and Fibonacci, and in so far they are uninformative. It may be observed, however, that the 14th-century problem deals with apples served at lunch, not with a heritage - Jacopo was thus not quite alone in deviating from the inheritance dress.[8] More important is that Planudes – whose testator dies before he has finished his will, which Planudes takes to explain that the number of heirs is unknown – brings the problem as an illustration of the following arithmetical observation (almost a theorem):[9]

> When a unit is taken away from any square number, the left-over is measured by two numbers multiplied by each other, one smaller than the side of the square by a unit, the other larger than the same side by a unit. As for instance, if from 36 a unit is taken away, 35 is left. This is measured by 5 and 7, since the quintuple of 7 is 35. If again from 35 I take away the part of the larger number, that is the seventh, which is then 5 units, and yet 2 units, the left-over, which is then 28, is measured again by two numbers, one smaller than the said side by two units, the other larger by a unit, since the quadruple of 7 is 28. If again from the 28 I take away 3 units and its seventh, which is then 4, the left-over, which is then 21, is measured by the number which is three units less than the side and by the one which is larger by a unit, since the triple of 7 is 21. And always in this way.

This description does not refer explicitly to counters, but it is noteworthy that the whole passage fits the above geometric explanation of Jacopo's problem to the slightest detail. Without support by either symbolic algebra or a geometric representation it is also difficult

8 There is no reason to conclude from the common fruit theme that Jacopo and the Byzantine text were connected, in particular since the general settings (garden/lunch) are different. "Box problems" (see note 3) about apples were common; though roughly contemporary, the two authors (or their sources) probably made independent but analogous changes of the usual dress (Jacopo repeatedly uses familiar dresses for problem types with which they usually do not go together).

9 I try to make a very literal translation, conserving all quasi-logical particles even when they offend the modern ear; a somewhat less literal French translation accompanies Allard's edition of the Greek text.

to see that the "theorem" holds for "any square number", and only the geometric diagram makes it evident that the procedure will continue in such a way that exactly nothing remains in the end.[10]

It is also to be observed that the quasi-theorem and the illustrating problem come exactly at the point where Planudes goes beyond Indian calculus. In the section which follows (and which closes the treatise) Planudes treats the problem to "find a figure equal in perimeter to another figure and a multiple of it in area" – that is, for a given n to find two rectangles[11] $\sqsubset\sqsupset(a,b)$ and $\sqsubset\sqsupset(c,d)$ such that $a+b = c+d$, $n{\cdot}ab = cd$ (a, b, c and d being tacitly assumed to be integers). Two solutions are given, the second being stated to be Planudes's own invention – which implies that the first solution was not (as indeed we shall see). In this borrowed solution, the following choice is made (n being taken to be 4):

$$a = n-1 \qquad\qquad b = (n^3-1)-(n-1)$$
$$c = n^2-1 \qquad\qquad d = (n^3-1)-(n^2-1)$$

Planudes maintains that this solution is only valid for n = 4, 3 and 5. This is not true, Planudes must either have calculated badly or relied on bad information. In any case, he proposes the following alternative of his own (where t is arbitrary):

$$a = t \qquad\qquad b = n\cdot(n+1)\cdot t$$
$$c = (n+t)\cdot t \qquad\qquad d = n^2\cdot t$$

As Allard [1981: 235] points out, the second solution coincides with the first if t is replaced by n–1. Planudes is not likely to have noticed this, but it may explain how he guessed his own scheme for the correct solution for n = 3.

The statement of the problem and the first solution are found in almost exactly the same words in the pseudo-Heronic *Geometrica* Ch. 24 [ed., trans. Heiberg 1912:414–417], cf. [Sesiano 1998: 284–286]. The manuscripts ("S" and "V") from which this section of the conglomerate is taken are of Byzantine date (the 11th respectively 14th century), and the use of the late form πολυπλασιάζω instead of the classical πολλαπλασιάζω points to an origin of the text certainly no earlier than the second century CE, perhaps considerably later. The shape of the problem, however, is ancient, not medieval: even though it is not found in Diophantos's *Arithmetic*, the stylistic similarity is unmistakeable. The problem is likely to come from that already existing tradition of "theoretical arithmetic" within

10 A corresponding calculations in symbols based on the corresponding sequence of identities $n{\bullet}(n-p+1) = p+(n+1){\bullet}(n-p)$ *can* of course show it, but with much less ease. A purely verbal argument like that of Planudes and unsupported by a diagram would hardly give the idea.

11 Actually, χωρίον, here translated "figure", may have the more specific meaning "rectangular area".

which Diophantos tells to have found his names and abbreviations for powers of the unknown [ed. Tannery 1893: I, 4].

This does not prove that even Planudes's "theorem" for the inheritance problem goes back to Antiquity, but the vicinity and the absence of a claim that he invented it himself suggests it to have been at least traditional.

In his edition of Elia Misrachi's text, Wertheim [1896: 60] suggests that the problem might be inspired by one which is found in a late 14th- or early 15th-century Byzantine manuscript (the cod. Cizensis) containing also Nicomachos's *Introduction* and Philoponos's scholia to that work (for which reason Wertheim may have thought it ancient, even if he does not say so). This problem [ed. Hoche 1866:153*f*] deals with the legacy of a father with three sons and three daughters, who has disposed of his legacy as follows:[12]

– The first son puts into the chest as many coins as it already contains and then takes 250 coins;
– then the second son does the same;
– then the third son does the same;
– then the first daughter puts into the chest as many coins as she finds there, and takes 125 coins;
– then the second daughter does the same;
– finally, the third daughter does as much, after which nothing remains.

The text gives the solution (originally, the chest contained 232 $^1/_3$ $^1/_{12}$ $^1/_{192}$ gold coins) but does not explain how it is reached.

Beyond the occurrence solely in a manuscript from c. 1400, other reasons speak against an early dating of the problem. Firstly, the term for the coin is the medieval χρύσινος (known only from the fourth century CE onward – the ancient form is χρυσίον); secondly, according to Taisbak, the syntax is Byzantine and not ancient. The present problem might therefore well be a secondary derivation from the problem type we have dealt with so far – a reduction to the normal "box problem" type allowing a solution by stepwise reverse calculation. In any case, the striking feature of equal shares is absent from it (indeed, the youngest son and the youngest daughter get the greatest shares); the basic unknown-heritage problem could therefore at most have borrowed *the dress* of an unknown heritage: the mathematical structure must have been an independent discovery.

The mathematics of the full problem

Before we go on with the analysis of further sources, it may be convenient to have an exhaustive mathematical analysis at hand; it should be kept in mind that this is a mathematical analysis, and not an interpretation of any source.

[12] I am grateful to C. M. Taisbak for assisting me in the interpretation of the text.

Let us assume that a total T is distributed into shares (δ_1, δ_2, …, δ_n, …) in this way:

- The first share δ_1 receives a_1, and furthermore a fraction ϕ of what is left after a_1 has been given.
- The second share δ_2 receives a_2, and furthermore a fraction ϕ of what is left after subtraction of the first share and of a_2.

  . . .
- The n-th share δ_n receives a_n, and furthermore a fraction ϕ of what is left after subtraction of the preceding shares and of a_n.

  . . .

We want to find the condition imposed on the sequence a_1, a_2, …, a_n, … by the request that $\delta_1 = \delta_2 = \ldots = \delta_n = \ldots = \Delta$ (admitting that the last share may be fractional; furthermore, we ask for the value of the total T, of the value Δ of the single share, and of the number N of shares.[13]

Before the n-th take, S_n is at disposition $S_1 = T$). The n-th share is then

$$\delta_n = a_n + \phi(S_n - a_n) = \phi S_n + (1-\phi)a_n \ .$$

After it has been removed, the remainder is

$$S_{n+1} = S_n - \delta_n = (1-\phi)S_n - (1-\phi)a_n = (1-\phi)\cdot(S_n - a_n) \ ,$$

and the n+1-th share becomes

$$\delta_{n+1} = \phi S_{n+1} + (1-\phi)a_{n+1} \ .$$

Since we have required that $\delta_n = \delta_{n+1} = \Delta$, which implies that $S_n - S_{n+1} = \Delta$, we find that

$$(1-\phi)\cdot(a_{n+1} - a_n) = \phi(S_n - S_{n+1}) = \phi\Delta,$$

[13] If we go beyond the mathematics of the 13th and 14th centuries and admit negative numbers, we may instead investigate for instance three sequences $S(n)$, $a(n)$, *and* $U(n)$, coupled through the conditions

$$U(n) = S(n)\text{-}a(n),\ S(n+1) = S(n) = a(n) = \phi U(n),$$

with n running through the domain of all integers (negative as well as positive, ϕ being an arbitrary real number), and ask for the condition that $\delta(n) = S(n) – S(n+1)$ be constant. Further investigation of the properties of this system might perhaps present us with some interesting mathematics (though I doubt it), but it would lead us away from the problem of our texts.

The wider class of coupled progressions does contain interesting objects. For instance, self-references are removed from the "Fibonacci series" if it is dissolved into three cyclically coupled sequences S, T, and U, where

$$U(i) = S(i)+T(i),\ S(i+1) = T(i)+U(i),\ T(i+1) = U(i)+S(i+1).$$

This observation, and the fact that the side-diagonal-algorism for a square consists by its very nature of two coupled progressions S and D,

$$S(i+1) = S(i)+D(i),\ D(i+1) = 2S(i)+D(i),$$

suggests a link to continued fractions.

whence also $a_{n+1}-a_n$ must be constant and equal to $\varepsilon = {}^{\phi}/_{1-\phi} \cdot \Delta$. The absolutely defined contributions must therefore constitute an arithmetical progression, $a_n = \alpha+(n-1)\cdot\varepsilon$

For a given set of values for $T = S_1$, ϕ and $\alpha = a_1$ follows

$$\Delta = \delta_1 = \alpha + \phi(\mathrm{T}-\alpha) = \phi T + (1-\phi)\cdot\alpha \;,$$
$$\varepsilon = \alpha_2 - a_1 = \frac{\phi}{1-\phi}\Delta = \frac{\phi}{1-\phi}(\phi T + [1-\phi]\alpha) \;.$$

If the resulting Δ does not exceed ${}^1/_2T$, this gives us at least 2 full shares; the sequence can be constructed stepwise until the remainder becomes less than Δ (a strict proof of this asks for complete induction, but it should be possible to dispense with that tedium here).

However, the texts do not start from given values of T, ϕ and α but from ϕ, α and ε. From this they find T, Δ and N. We may do as much. From $\varepsilon = {}^{\phi}/_{1-\phi}\cdot\Delta$ follows

(7ᶜ) $$\Delta = \frac{1-\phi}{\phi}\varepsilon \;.$$

But since $\Delta = \delta_1 = \phi S_1 + (1-\phi)a_1 = \phi T + (1-\phi)\cdot\alpha$,

$$\phi T = (1-\phi)\cdot(\frac{\varepsilon}{\phi} - \alpha) \;,$$

(7ᵃ) $$T = \frac{1-\phi}{\phi}\cdot(\frac{\varepsilon}{\phi} - \alpha) \;,$$

and finally

(7ᵇ) $$N = \frac{T}{\Delta} = \frac{\frac{1-\phi}{\phi}(\frac{\varepsilon}{\phi} - \alpha)}{\frac{1-\phi}{\phi}\cdot\varepsilon} = \frac{1}{\phi} - \frac{\alpha}{\varepsilon} = \frac{\varepsilon - \phi\alpha}{\phi\varepsilon} \;.$$

The condition that at least two full shares can be found (that is, $N \geq 2$, all parameters taken to be positive) is then that

(8) $$\phi \leq \frac{1}{2 + \frac{\alpha}{\varepsilon}} \;,$$

which is clearly fulfilled in all examples we have seen.

In order to compare with Fibonacci’s rules, we put $\phi = {}^p/_q$. Thereby the formulae become

(9ᶜ) $$\Delta = \frac{q-p}{p}\varepsilon \;,$$

(9ᵃ) $$T = \frac{(q-p)\cdot(\varepsilon q - \alpha p)}{p^2} \;,$$

(9^b) $$N = \frac{\varepsilon q - \alpha p}{\varepsilon p}.$$

We may also express ϕ as $^1/_d$, in agreement with the trick Fibonacci used to treat cases in the second column of the scheme on p. 353, for instance $(1,1|^2/_{11})$. Then the formulae look much simpler:

(10^c) $$\Delta = (d-1) \cdot \varepsilon,$$

(10^a) $$T = (d-1) \cdot (d\varepsilon - \alpha)$$

(10^b) $$N = d - \frac{\alpha}{\varepsilon} ,$$

From (10^b) we see that if ϕ is an aliquot part (and d thus integer), N is integer if and only if ε divides α. For other cases we see from (9^b), presupposing that $^p/_q$ is reduced to minimal terms and thus that p and q are mutually prime, that

$$N\varepsilon = \frac{\varepsilon q}{p} - \alpha .$$

If α and ε are integer (as they always are in the texts), this can only be fulfilled if p divides ε, that is, if $\mu = {}^\varepsilon/_p$ is integer. Inserting this we see that

$$N\mu p = \mu q - \alpha ,$$

whence

$$\alpha = \mu \cdot (q - Np) .$$

For a given value of ϕ reduced to minimal terms $^p/_q$, all types leading to an integer solution for N are thus $(q,p|^p/_q)$ and those types which can be obtained by trivial means from it by multiplying all the absolutely defined contributions $q+ip$ by the same constant μ and/or by starting from a different point in the sequence $\mu q+i\cdot(\mu p)$ (taking care that N remain in the requested domain, that is, $N \geq 2$.[14] Since no sources contain a problem with non-integer d leading to an integer value for N, this was probably not known to the medieval calculators

All of this concerns the situation where the absolutely defined contributions are taken first and the fraction of the remainder afterwards. All calculations are similar in the case where the fraction is taken first. The corresponding formulae become:

(12^c) $$\Delta = \frac{\varepsilon}{\phi},$$

(12^a) $$T = \frac{\varepsilon - \alpha\phi}{\phi^2},$$

[14] For $\phi = {}^6/_{31}$, (α,ε) may thus be any one of the sets (1,6), (7,6), (13,6) and (19,6) or their multiples $(\mu, 6\mu)$ etc.

$$(12^b) \qquad N = \frac{\varepsilon - \alpha\phi}{\varepsilon\phi}.$$

$$(13^c) \qquad \Delta = \frac{\varepsilon q}{p},$$

$$(13^a) \qquad T = \frac{q(\varepsilon q - \alpha p)}{p2},$$

$$(13^b) \qquad N = \frac{\varepsilon q - \alpha p}{\varepsilon p}.$$

$$(14^c) \qquad \Delta = d\varepsilon,$$

$$(14^a) \qquad T = d^2\varepsilon - d\alpha,$$

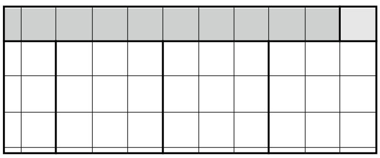
$$(14^b) \qquad N = d - \frac{\alpha}{\varepsilon}.$$

Since the formulae for N are the same, the condition for the number of shares being integer and being at least 2 are unchanged.

After this modern reconstruction one may ask how corresponding calculations could be made with the tools at hand in late Antiquity or the Middle Ages. Problems of the types $(\varepsilon,\varepsilon|^1/_d)$ and $(^1/_d|\varepsilon,\varepsilon)$ can of course be solved by means of counters for any integer value of ε, not only for $\varepsilon = 1$, just by taking the value of each counter to be ε instead of 1; so can problems of the types $(n\varepsilon,\varepsilon|^1/_d)$ and $(^1/_d|n\varepsilon,\varepsilon)$ – the procedures are exactly the same as in the previous case, just with omission of the first $n-1$ steps; anybody familiar with the operations on the square pattern might discover that.[15] Even for cases where d is non-integer and/or ε does not divide a it is possible to construct pebble justifications once the solution is known (on the condition of accepting fractional pebbles); but it is difficult to see how such pebble proofs could be found if one did not know the solution already.[16] What then?

15 On the other hand, anybody familiar just with the *rule* for the case $(1,1|^1/_d)$ might also observe that the solution to the case $(n,1|^1/_d)$ is obtained from the former case by omission of the first $n-1$ heirs. Solutions to the case $(\varepsilon,\varepsilon|^1/_d)$ is of course obtained from that for $(1,1|^1/_d)$ by simple proportionality, no new proof being needed; the same holds for the relation between the cases $(n\varepsilon,\varepsilon|^1/_d)$ and $(n,1|^1/_d)$.

16 For the relatively simple case $d = 4^1/_2$, $\alpha = 1$, $\varepsilon = 3$ (whence $\Delta = 3^1/_2 \cdot 3 = 10^1/_2$, $n = 4^1/_2 - ^1/_3 = 4^1/_6$, the square-grid diagram corresponding to the pebble justification looks as follows:



Each row is equal in area to Δ, and the number of rows is $N = 4^1/_6$. If we remove $\alpha = 1$ in the

The first step will be to show that the equality of shares implies that the absolutely defined contributions constitute an arithmetical progression. A possible means for showing this is used amply in the *Liber abbaci*, namely the line diagram (but not used for these problems). Let us first try (Figure 4) the more intricate case where the absolutely defined contribution is taken first; for convenience I shall use letter symbols, but pointing and words could do the same:

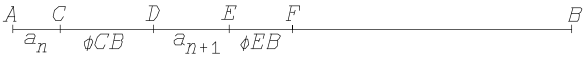


Figure 4

AB represents S_n, that is, the amount that is at disposition when the *n*-th share is to be taken, *n* being arbitrary (but possible)[17] This share is *AD*, consisting of $AC = a_n$ and $CD = \phi CB$. The following share is *DF*, consisting of $DE = a_{n+1}$ and $EF = \phi EB$. Since $AD = DF = \Delta$, $CB = CD+DB$, and $EB = EF+FB$, we find that

$$a_{n+1}-a_n=\phi(CB-EB)=\phi(CD-EF)+\phi(DB-FB)=\phi(a_{n+1}-a_n)+\phi\Delta\ ,$$

whence

$$(1-\phi)\cdot(a_{n+1}-a_n)=\phi\Delta$$

first row, $9^1/_2$ are left. The lower $3^1/_6$ rows can be divided into three columns with area $3\cdot3^1/_6 = 9^1/_2$, and a narrow column with area $1^1/_2\cdot3^1/_6 = 4^3/_4 = {}^1/_2\cdot9^1/_2$. The $9^1/_2$ left over in the upper row is thus, as it should be, ${}^1/_d$ of the remainder. When it is removed, we are left with the lower $3^1/_6$ rows. $\alpha+\varepsilon = 4$ is removed from the upper of these, leaving $6^1/_2$ in the same row and $3^1/_2$ times $6^1/_2$ in the following; etc.

After having gone through this operation I suppose that the reader, firstly, will find it unlikely that somebody should invent this diagram unless it be done (as here) from the already known result; and, secondly, will doubt that Fibonacci's formulae (or those we shall encounter below in the *Compendy de la praticque des nombres*) were derived from such diagrammatic considerations. One could ask for no better example of an *a posteriori* synthesis which is of no help whatsoever in the reconstruction of a corresponding analysis.

I also expect the reader to find new sympathy for Plato's insistence (*Republic* 525d–526a, ed., trans. [Shorey 1930: 162–165]) that it is a bad habit to transfer to the realm of theoretical arithmetic that breaking of the unit with which shopkeepers were conversant. "Visual" mathematics, seductive as it is in simple cases, becomes much more difficult than formal calculation as soon as intricacies arise; symbolic algebra conquered for good reasons.

[17] The reason Fibonacci offered no proof of this kind may be that the structures of secondary logic ("for any …", "for all …", etc.) were not integrated in his mathematical standard language and therefore did not offer themselves readily for the construction of proofs. The present line-diagram proof, if made during or before his times, is likely not to have looked at an arbitrary step but to have started from the first and then given an argument by quasi-induction. Fibonacci, making the calculation in numbers that change from step to step, could not generalize his result.

and further (in order to avoid a formal algebraic division) the proportion

$$\Delta : (a_{n+1} - a_n) = (1 - \phi) : \phi \ .$$

By means, for instance, of Euclid's *Data*, prop. 2 [trans. Taisbak 2003: 254], "If a given magnitude [here Δ] have a given ratio [here $(1-\phi):\phi$] to some other magnitude [here $a_{n+1}-a_n$], the other is also given in magnitude" (or applying simply the rule of three), we find that $a_{n+1}-a_n$ has the same value irrespective of the step where we are. In consequence, the absolutely defined contributions have to constitute an arithmetical progression.

If the fraction is taken first, we may use the line diagram in Figure 5:

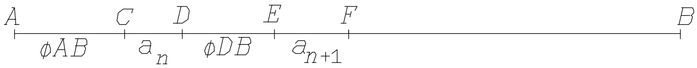


Figure 5

In this case, $\phi AB + a_n = \phi DB + a_{n+1}$, and therefore $a_{n+1} - a_n = \phi(AB - DB) = \phi\Delta$, which again means that the absolutely defined contributions must form an arithmetical progression.

In both cases, once we are so far it is legitimate to construct the rules from the equality of the first two shares only. This can be done by somewhat laborious but simple first-degree algebra – Fibonacci shows one way to do it, but there are alternatives. It can also be done by means of two false positions (see note 19), and probably by still other methods. Possibly, one might reconstruct the way that was actually followed in order to find the rules from the detailed make-up of these. I have not been shrewd enough to do so.

14th-century abbacus writings

In its basic inheritance shape, the problem turns up in quite a few 14th-century abbacus treatises. The earliest of these is the *Livero de l'abbecho* [ed. Arrighi 1989: 116].[18]

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[18] On the words of its compiler, this treatise purportedly written "secondo la oppenione de maiestro Leonardo de la chasa degli figluogle Bonaçie da Pisa" has been believed to be extracted from the *Liber abbaci*, and from internal evidence it has been supposed to be from 1288–1290. The internal evidence consists of loan documents which turn out to be copied from elsewhere (whether original documents or an earlier abbacus treatise cannot be decided), for which reason the real date must be somewhat later (hardly much, the language seems rather archaic). As regards the link to Fibonacci, the treatise does contain a number of advanced problems borrowed from the *Liber abbaci*; but these are external decoration, the stem of the treatise is independent of Fibonacci (the *Liber abbaci* as well as any other work he may have written, as revealed by linguistic analysis), and repeatedly the compiler reveals not to understand what he copies from Fibonacci [Høyrup 2005a]. The problem about the unknown heritage is located in a final collection of mixed questions, some of which are taken from the *Liber abbaci* and others not.

The problem type is *not* represented in the *Columbia Algorism* [ed. Vogel 1977], which now appears to be the earliest extant abbacus text (from c. 1280 or not much later, albeit the manuscript

Here we find a problem of type (7,1|$^1/_{24}$) dealing with a heritage consisting of an unknown number of sheep. The rule that is given is that "we should strike off one from the fraction, and $^1/_{23}$ remain, and we shall strike off 7 from 24, and 17 remain" (which gives 17 sons and 23 sheep for each son). These rules are clearly not derived from Fibonacci's rule (4^b) for the intricate case, which would give the number of sons as (24−1)•7−(7−1)•24. Instead they may come from the observation that the outcome corresponds to that of the distribution (1,1|$^1/_{24}$), the six first shares being omitted; but the mathematical quality of the rest of the treatise does not make it likely that the compiler was able to get that idea on his own.

Paolo Gherardi's *Libro di ragioni*, written in Montpellier in 1327, contains a problem of type (1,1|$^1/_{10}$) [ed. Arrighi 1987: 37*f*]; the story deals with a father who gives 1 mark of gold and $^1/_{10}$ of the gold that remains in his box to the first son, etc. The numbers are thus like those of Jacopo, but already the return to the traditional inheritance story shows that Jacopo is not the source – or at least not the only source.

The *Libro de molte ragioni* [ed. Arrighi 1973: 199], a conglomerate from Lucca from c. 1330, has another inheritance story with the same numbers (1,1|$^1/_{10}$), sufficiently different (both at the level of the story and in the formulation of the rule) to exclude any direct link.

In the *Istratto di ragioni* [ed. Arrighi 1964: 140*f*] – a problem collection written down in c. 1440 but claiming to go back to Paolo dell'Abbaco (c. 1340) and in any case likely to copy material from that period – we find two variants, namely (1000,1000|$^1/_{10}$) and ($^1/_6$|10,10). The former (about *bizanti*) is solved by the usual rule (the denominator of the fraction minus 1), the latter (about *fiorini*) by a double false position (using only the equality of the first two shares).[19]

In an anonymous problem collection belonging to the Vatican Library, Vat. Lat. 10488

we possess is a 14th-century copy), cf. [Høyrup 2005a: 27 n.5].

19 The formulation runs thus:

> We shall find a number such that, when $^1/_6$ is detracted and then 10, and from the remainder again $^1/_6$ and then 20, one [detraction1 is as much as the other; and therefore posit that this number be 60, seize $^1/_6$ of 60, it is 10, and 10 more, you get 20; you have when you detract 20 from 60, 40 remain, and now seize $^1/_6$ of 40, which is $6^2/_3$, and 20 more, you get $26^2/_3$. So that you see that he has $6^2/_3$ more than the first. And now posit another number, and let us posit that it is 120, and therefore seize $^1/_6$ of 120, it is 20, and 10 more, you get 30. You have that the remainder is 90, now seize $^1/_6$ of 90, which is 15, and 20 more, you get 35. You have that to the second falls 5 more than to the first; so that you will say: and for 20, 5 more. And now follows the rule you have heard several times in this book, according to which the true total heritage is
>
> $$\frac{6\frac{2}{3}\cdot 120-5\cdot 60}{6\frac{2}{3}-5}=300\,\textit{fiorini}.$$

The single share is then found as $^1/_6$•300+10 = 60, and the number of sons as 300/60 = 5.

(fol. 66^r), compiled in Venice in 1424 and likely to contain material which, when not copied directly from 14th-century material (part of it is) appears at least to reflect 14th-century manners, there is an example of type $(7,1|^1/_{30})$, with solution $N = 30-7$, $T = (30-7)\cdot(30-1)$. Both the type and the solution thus coincide with the sheep problem from the *Livero de l'abbecho*, but the formulation is so concise that only those who knew the genre would understand what was meant.[20] Somebody understood: there is a marginal commentary in a different hand containing a numerical proof.

Arabic pseudo-kin

Due to the kind assistance of Mahdi Abdeljaouad (personal communication), I have come to know about two Arabic problems obviously inspired from the simple version of the problem type we are discussing. Both replace it by something closer to the orthodox "box problem" (though not changing it as radically as the late Byzantine analogue discussed on p. 358), yet without taking advantage of the change.

One comes from ibn al-Yāsamīn's *Talqīh al-afkār fī'l ʿamali bi rušūm al-ghubār* ("Fecundation of thoughts through use of *ghubār* numerals") – written in Marrakesh in c. 1190. It runs as follows:[21]

> An inheritance of an unknown amount. A man has died and has left at his death to his six children an unknown amount. He has left to one of the children one dinar and the seventh of what remains, to the second child two dinars and the seventh of what remains, to the third three dinars and the seventh of what remains, to the fourth child 4 dinars and the seventh of what remains, to the fifth child 5 dinars and the seventh of what remains, and to the sixth child what remains. He has required the shares be identical. What is the sum?
>
> The solution is to multiply the number of children by itself, you find 36, it is the unknown sum. This is a rule that recurs in all problems of the same type.

The other is found in the *al-Maʿūna fī ʿilm al-ḥisāb al-hawāʾī* ("Assistance in the science of mental calculation") written by ibn al-Hā'im (1352–1412, Cairo, Mecca & Jerusalem, and familiar with ibn al-Yāsamīn's work):[22]

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20 "Somebody makes testament. To the first son he leaves 7 $^1/_{30}$, to the second 8 $^1/_{30}$, to the $^1/_3$ [thus for "third"] 9 and $^1/_3$, and continuing thus until there was neither more money nor more sons. How many *fiorini* did he have? Do thus, detract 7 from 30, remains 23, because he said first 7, and he had 23 sons. Now for $^1/_{30}$, detract 1 from 30, remains 29, multiply 23 times 29, it makes 667, and he had 667 *fiorini*. And it is done".

On fols 51^v, 68^v and 69^v, problems of types $(1,1|^1/_{10})$, $(5,1|^1/_{20})$ and $(3,3|^1/_{10})$ are formulated similarly but even more succinctly. The third of these gives the solution $N = (10-1)/1 = 9$, $T = 9\bullet9\bullet3$, wrongly referring the factor 3 to the first bequest α, not to ε.

21 My translation from Mahdi Abdeljaouad's French translation.

22 Still my translation from the French.

> An amount of money has been diminished by one dirham and the seventh [of what remains]; by two dirhams, and then the seventh of what remains; then three dirhams and the seventh of what remains; then four dirhams and the seventh of what remains; then five dirhams and the seventh of what remains. In the end, six remain.
>
> Take the square of the six that remain, it is the amount which was asked for.

The number of shares is thus given in both versions; none the less, both still use the same rule as the "Christian" version of the simple problem. As we observe, ibn al-Yāsamīn omits the information that the last share is determined according to the same rule as the preceding ones, whereas ibn al-Hā'im does not require the shares to be equal. Both pieces of information are indeed superfluous.

We also observe that ibn al-Hā'im's version is not overdetermined; it can be solved backwards step by step, in this way:

The fifth share is $5+{}^1/_7A$, where $A+5$ is what is left after the taking of the fourth share; but this remainder is also the sum of the fifth and sixth shares. Hence,

$$A+5=6+5+\tfrac{1}{7}A,$$

from which follows $A=7$. The fourth share is $4+{}^1/_7B$, where $B+4$ is what is left after the taking of the third share; but this is also the sum of the fourth, fifth and sixth shares; etc.

Obviously, a similar backward calculation could be made for varying fractions and for absolutely defined contributions that are not in arithmetical progression. However, the rule is only valid for a constant fraction ${}^1/_{N+1}$, where N is the given number of shares, and if the absolutely defined contributions are $1+(i-1)$. There is hence no doubt that ibn al-Hā'im's problem descends from the "Christian" problem and results from an attempt to assimilate it to a more familiar structure.

Ibn al-Yāsamīn's problem is overdetermined, but the evident way to solve it would still be a backward calculation: if S is what is left when the fifth share is to be taken, the fifth share is $5+{}^1/_7(S–5)$, and the sixth share is what is left, i.e., $S–5–{}^1/_7(S–5)$. From their equality follows that S is 12, each share thus 6, and the total 6•6. The rule, once again, is valid but not naturally adapted to the actual problem.

The conclusion is that mathematicians from the Maghreb or al-Andalus[23] had come to know about the problem type already before the *Liber abbaci* was written; but their use of a rule which is better adapted to the "Christian" version of the problem shows that this latter version with its unknown value of N was not derived from the "Islamic" box-problem versions but was indeed the original form. Whether ibn al-Hā'im knew the problem from Maghreb mathematicians or through other channels cannot be decided at

[23] Ibn al-Yāsamīn's "all problems of the same type" seems to prove that he was not the only mathematician in his area to know about them. He had been active in Morocco and in al-Andalus (Muslim Spain); he may have encountered the derived problem type in either place.

present. In any case, the aberrant character of the two Arabic problems is strong evidence that Fibonacci and Planudes did not get *their* problem from the Arabic world – if it was known and accepted there, why should our two authors need to make it more familiar by making N a given magnitude? Ibn al-Yāsamīn confirms that the problem type which inspired him was indeed familiar (in a place that might inspire him and where he expected to find readers) before the *Liber abbaci* was thought of.

Provence and Barthélemy de Romans

The problem type $(1,1|^{1}/_{8})$ turns up in a manuscript of the *Trattato di tutta l'arte dell'abacho* from 1340 (Rome, Accademia Nazionale dei Lincei, Cors. 1875, fol. 85^{v}). The rule is once again that the number of sons is found by subtracting one from the denominator – "if he had said $^{1}/_{9}$ to them, you would subtract one from 9, but because he said $^{1}/_{8}$, subtract one from 8, 7 remains, and 7 were the sons". It is likely but not certain that the author picked up the problem in Avignon, where the original was written around 1334.[24] In any case the genre is well represented in treatises written in Provence in the early 14th century (Jacopo in 1307, Paolo Gherardi in 1327), being absent only from the *Liber habaci* [ed. Arrighi 1987], written around 1310, almost certainly in Provence and almost certainly *not* by Paolo Gherardi.[25] It is absent from most other 14th- and 15th-century treatises from the Ibero-Provençal area I know about – thus from the Castilian *Libro de arismética que es dicho alguarismo* [ed. Caunedo del Potro & Córdoba de la Llave 2000: 133–213],P ⟦[26]⟧ from Francesc Santcliment's *Summa de l'art d'aritmètica* from 1482 [ed. Malet 1998]; from Francés Pellos's *Compendion de l'abaco* from 1492 [ed. Lafont & Tournière 1967]; and (as far as can be concluded from the description in

24 For this dating, see [Cassinet 2001]. The problem is not in what appears to be a draft autograph of the treatise (Florence, Biblioteca Nazionale Centrale, fond. prin. II,IX.57), but since this draft does not represent the finished treatise its author may well have added even the actual problem afterwards (other material with no parallel in the main draft but in the same hand as the main treatise has been added in the Lincei manuscript; when metrologies are referred to in these problems, they are the same as in the main treatise, and of Provençal rather than Tuscan type).

25 The date being rather late and the orthography purely Tuscan, it is not certain whether we should count as genuinely Provençal an occurrence of a problem $(1,1|^{1}/_{7})$ in Francesco Bartoli's *Memoriale*, written down in Avignon before 1425 and copied from unidentified abbacus material [ed. Sesiano 1984a: 1381]. We may notice, however, that Bartoli's problem shares with Paolo Gherardi's version (and with no other) that everything is measured in weight units of gold, not in coin (here ounces, in Gherardi marks of gold).

Bartoli's rule is the usual one – that subtraction of 1 from 7 gives both the number of sons and the amount each one receives; maybe the Papal courtly environment is the reason that his testator is a count.

26 ⟦This is a mistake. I had overlooked that $(1,1|^{1}/_{10})$ as well as $(1,1|^{1}/_{11})$ are found on p. 169.⟧

[Sesiano 1984b]) from the “Pamiers Algorism”.[27] However, it is represented in the 15th-century *Traicté de la praticque d’algorisme* by four problems of the types $(1,1|^1/_{10})$, $(3,1|^1/_{10})$, $(^1/_{10}|1,1)$ and $(^1/_{10}|2,2)$;[28] in Barthélemy de Romans’ *Compendy de la praticque des nombres;*[29] and in the problem collection which Nicolas Chuquet attached to his *Triparty en la science des nombres*.

No known source ever treated the genre as fully as Barthélemy de Romans’ *Compendy*. Maryvonne Spiesser [2003] not only offers an edition of the pertinent part of the text (pp. 391–423) and a translation into modern French (pp. 543–579) but also a substantial commentary (pp. 139–156), of which I shall take advantage so as to concentrate on what is important in the present context; page references to the treatise refer to Spiesser’s edition.

In general, Barthélemy prefers to present first the general principles of a matter, and afterwards the examples. Thus also to some extent here, but with the proviso that this part of the text falls in two major sections, each of which contains general principles and examples.

Barthélemy gives the genre a name not known from earlier sources and probably his own invention, *progressions composees*;[30] he also gives a name to the quantity $^1/_\phi = {}^q/_p = d$, the *vray denominateur* or “true denominator”. Since this entity was used by Fibonacci in a way that suggests the idea not to be his own and since the name is close at hand it is less certain that even this term was Barthélemy’s invention.

Barthélemy starts by distinguishing between *deux manieres*, “two modes”, in the first of which the absolutely defined contributions (*les nombres de la progression*) are taken first and the fraction of what remains (*la partie ou les parties que l’on veut du demourant*)

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27 It is also absent from two 12th-century Latin works prepared in Iberian area where it could have been expected to turn up if it had been known, the *Liber augmenti et diminutionis* [ed. Libri 1838: I, 304–369] and the *Liber mahameleth* (at least in as far as can be determined from the description of the latter work in [Sesiano 1988]. ⟦Nor did I notice it when preparing my review [Høyrup 2015] of Anne-Marie Vlasschaert’s edition [2010].⟧

28 I used the transcription in Stéphane Lamassé’s unpublished dissertation, for access to which I am grateful.

The rule given in the *Traicté* for the case $(^1/_{10}|2,2)$ is mistaken, and corresponds instead to the case $(^1/_{10}|2,1)$.

29 Barthélemy probably wrote this treatise around 1467, but what we possess is a revised redaction from 1476 due to Mathieu Préhoude – see [Spiesser 2003: 26, 30]. Barthélemy himself presents his work as an extension of an earlier treatise from his own hand (possibly the just-mentioned *Traicté de la praticque d’algorisme*) aimed at giving his readers profounder understanding.

30 Firstly, the topic is never grouped together with arithmetical progressions in other sources; secondly, there are some suggestions in Barthélemy’s text that he might be accustomed to find it together with the double false position, in agreement with the occasional use of this method to solve the problems – see below.

afterwards; in the second, the "part or the parts" are taken first, and afterwards "the numbers that make the progression" from what remains. Then the "true denominator" is explained and exemplified, and it is pointed out that in the first mode, *four numbers* are fundamental: the true denominator (d), "the number that is one less than the denominator" (d–1), "the number which makes the progression" (ε) and the "number by which the progression starts" (α); he does not forget to say that the latter two may be equal, but they should none the less be treated as different. He also points out that three hidden numbers are sought for, "the number that can be divided by this progression" (T), "how much there will be in each place" (Δ) and "how many places there will be in the progression" (N); he claims as a general fact that $T > \Delta > N$.[31]

Thereby he has come to the enunciation of a "general rule" for progressions of the first kind:

(15^c) $$\Delta = (d-1)\cdot\varepsilon\ ,$$
(15^a) $$T = ([d-1]\varepsilon-\alpha)\cdot d+\alpha\ ,$$
(15^b) $$N = T/\Delta\ .$$

(15^c) coincides with (10^c), and (15^a) easily reduces to (10^a), whereas Fibonacci's (4^a) reduces to ([d–1]α–[α–ε]d)•(d–1) if we introduce into it the true denominator d. The rule is illustrated by three examples of types $(3,3|{}^1/_7)$, $(2,3|{}^2/_{11})$ and $(3,2|{}^3/_{13})$. The first example is told to deal with the division of a number according to the progression – in the end it turns out that a division among N "men" is thought of; the two others only speak about "making a progression". We notice that in the first problem, α = ε, in the second α > ε, in the third α < ε . This principle is pointed out by Barthélemy. He also observes, however, that the first deals with "one part", the second with "two parts", the third with "three parts"; this is wholly unimportant as long as the "true denominator" is used, and could be a reminiscence of the similar distinction (though only between "a part" and "parts") in Boethian arithmetic.

Then Barthélemy points out that the problems where α = ε "can be done by another practice, for which this is the appurtenant rule":

(16^b) $$N = d-1,$$
(16^c) $$\Delta = (d-1)\cdot\varepsilon\ ,$$
(16^a) $$T = N^2\cdot\varepsilon\ ,$$

This rule is then applied to a final example of the first mode, $(3,3|{}^2/_9)$, and it is pointed out that the outcome would have been the same if rule (15) had been applied. From Barthélemy's words and argument it is fairly obvious that he did not arrive at the specific

31 As we have seen, this is not strictly true – if α = ε = 1, N = Δ. But for all other integer positive values of α and ε (the only ones considered by Barthélemy and our other authors) it is true for acceptable values of d.

rule by reducing the general one; but is seems likely that he himself formulated *as a rule* a practice that he had only encountered in the shape of particular problems (since the inheritance problems are all of this type, many with $\varepsilon = 1$ but others with $\varepsilon = 10$, $\varepsilon = 100$ or $\varepsilon = 1000$, and since they commonly find N, Δ and T in this order, this is quite possible). He does not bother the reader with any argument that one set of rules can be derived from the other by reduction, and the formulation of such an argument would indeed be quite cumbersome in the absence of algebraic symbolism (provided Barthélemy had the idea, which is far from certain – mathematical intuitions are rarely more than one step in advance of that which established familiar terminology and concepts can grasp).

For the "second mode" this rule, valid for the case $\alpha = \varepsilon$, is given first:

$$N = d - 1, \tag{17$^{\mathrm{b}}$}$$

$$\Delta = d \cdot \varepsilon, \tag{17$^{\mathrm{c}}$}$$

$$T = (d-1) \cdot d \cdot \varepsilon, \tag{17$^{\mathrm{a}}$}$$

which is then applied to the cases $({}^{1}/_{7}|2,2)$ and $({}^{2}/_{11}13,3)$. Nothing is said about this rule corresponding to a practice, but that may be because the corresponding general rule has not yet been presented – indeed, when all the rules with appurtenant examples have been explained, they are spoken of as *les praticques precedants*. In any case there is no doubt that this is the counterpart of the simplified rule (16) for the case $(\varepsilon,\varepsilon|d)$.

There may be a good reason for giving separately the rule for the case $\alpha = \varepsilon$. Afterwards, indeed, separate rules are given for the cases $\alpha < \varepsilon$ and $\alpha > \varepsilon$ – and these rules *have* to be stated separately, because they are of the same type as Fibonacci's (5) and (6) though not exactly the same – respectively

$$T = \frac{[(\varepsilon-\alpha)q + (q-p)\alpha] \cdot q}{p^2}, \tag{18$^{\mathrm{a}}$}$$

$$N = \frac{(\varepsilon-\alpha)q + (q-p)\alpha}{\varepsilon p}, \tag{18$^{\mathrm{b}}$}$$

$$\Delta = \frac{\varepsilon q}{p}, \tag{18$^{\mathrm{c}}$}$$

and

$$T = \frac{[(q-p)\alpha - (\alpha-\varepsilon)q] \cdot q}{p^2}, \tag{19$^{\mathrm{a}}$}$$

$$N = \frac{(q-p)\alpha - (\alpha-\varepsilon)q}{\varepsilon p}, \tag{19$^{\mathrm{b}}$}$$

$$\Delta = \frac{\varepsilon q}{p}. \tag{19$^{\mathrm{c}}$}$$

The examples are $({}^{1}/_{7}|3,5)$, $({}^{2}/_{9}|3,5)$, $({}^{6}/_{31}|2,3)$, $({}^{1}/_{6}|5,3)$, $({}^{2}/_{11}|5,2)$ and $({}^{5}/_{19}|5,3)$.

The totally different approaches to the two modes, one by means of the true denominator and the other one (except when $\varepsilon = \alpha$) by means of p and q, suggests that all the rules presented here are borrowed (this is also proposed by Maryvonne Spiesser [2003: 152]). The discrepancy between Barthélemy's treatment of the two modes makes it implausible that the *Liber abbaci* was his source.[32]

What comes from this point (p. 402) onward is likely to be Barthélemy's own original contribution. First he offers a systematic exposition of the principles of rules (18) and (19) together with their counterparts for the first mode (almost coinciding with (1) and (4) as set forth in the *Liber abbaci*) and summarizes everything in a single rule; even here, the ambiguities of a verbal expression makes him insert an example ($^6/_{31}|3,3$). Next, "for the practice of this rule and in order to see rapidly how one should make the necessary multiplications for the three numbers that should be divided by the three dividers to get the three hidden numbers", he shows "how the necessary numbers can be put into a

[32] Maryvonne Spiesser [2003: 156] finds it to be a "very plausible" hypothesis that the *Liber abbaci* was the direct source – a conclusion which I endorsed in [Høyrup 2005b] because of the lack of evidence for alternatives. Since the present broader investigation of the question shows that non-Fibonacci solutions even to the sophisticated versions *must* have circulated, this argument can no longer be considered valid. Spiesser takes the shared occurrence of uncommon fractions like $^5/_{19}$ and $^6/_{31}$ as supplementary evidence for an intimate connection; however, Barthélemy's range of non-aliquot-part fractions ($^2/_{11}$, $^3/_{13}$,$^2/_9$,$^6/_{31}$,$^5/_{19}$,$^3/_{11}$,$^4/_{15}$,$^5/_{21}$,$^6/_{25}$,$^4/_{27}$) goes far beyond what we find in Fibonacci ($^2/_{11}$,$^5/_{19}$,$^6/_{31}$) but remains within the same vaguely defined family – all denominators are odd, most are prime, the values fall between 0.148 and 0.272 (all but one between 0,181 and 0.272), the numerators being evidently larger than 1; no denominator except 13 occurs in more than one fraction. If we restrict ourselves to those of Barthélemy's fractions which appear in the part of his text discussed so far, that is, the part which seems to build upon borrowed rules and therefore perhaps also on borrowed examples ($^2/_9$,$^2/_{11}$,$^3/_{13}$,$^5/_{19}$,$^6/_{31}$), the characteristics are even more narrowly defined: all values fall between, 0.181 and 0.263, no denominator appears more than once and all denominators are prime. Strikingly, *all but two* non-reducible fractions with denominators below 37 which fulfil these (partly mathematical, partly aesthetic) criteria are used – the exceptions being $^5/_{23}$ and $^7/_{29}$. If both Fibonacci and Barthélemy drew on a fund of problems defined by these criteria, simple statistics shows us that the coincidences are not striking: if Fibonacci were to select 3 from the list of 7 possible fractions, the probability that all three would fall within the range of 5 values used by Barthélemy is $\binom{5}{3} \div \binom{7}{3} \approx 28\%$. The uniformity of the possibly borrowed examples in Barthélemy's text shows that such aesthetic and mathematical criteria were efficient (his own probably added examples, though widening the limits of the permissible a bit, also confirms that the criteria were felt, since his deviations from the canon that is implicit in the first part are quite modest).

Further, *if* Barthélemy had really borrowed from Fibonacci problems with ϕ equal to $^2/_{11}$, $^5/_{19}$ and $^6/_{31}$, one should also expect him to have borrowed the appurtenant sets (α,ε) - but this only happens in 1 of 9 instances (1 of 7 if we count pairs $(\alpha,\varepsilon|\phi)$ and $(\phi|\alpha,\varepsilon)$ with coinciding parameters as a single instance, namely for the case ($^6/_{31}|2,3$)). Given how often the set $(\alpha,\varepsilon) = (2,3)$ is used, this is once again no more than could be expected from a random distribution.

diagram", as shown in Figure 6 – at first in general form, with the numbers described in technical verbal terms defined by Barthélemy (here replaced by our usual symbols).

A new sequence of numerical examples follows in which the diagram is used, all in pairs representing the two modes.[33] At a certain point (p. 413) he shows how the diagram applies to the rules based on a "true denominator". He explains that the three numbers in bottom (not counting those in [ ]) are integers and the others actually fractions, a denominator equal to p being tacitly understood, and that there is only one divisor (*viz* εp, which reduces to ε, p and p^2 being both reduced to 1). The exposition corresponds to what is shown here in Figure 7, and so do the diagrams used in the subsequent numerical examples.[34]

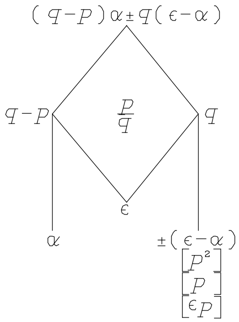

Figure 6

The whole treatment of division according to progressions is made under the general heading of "two false positions", whose rule is simply stated (p. 390) as *plus et plus*, *meins et meins*, *sustrayons*. *Plus et meins*, *adjoustons* – "More and more, less and less, we shall subtract. More and less, we shall add". The meaning is that if both initial guesses lead to an excess or to a deficit, the rule with ⟦subtraction⟧ is to be used. If one leads to an excess and the other to a deficit, the variant with ⟦addition⟧ should be used. The rule itself (weighing the two guesses in inverse proportion to their error) is not presented, instead Barthélemy goes directly first (briefly) to "simple" (that is, arithmetical) and then to the composite progressions discussed here.

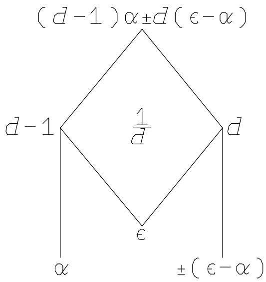

Figure 7

On p. 420 Barthélemy returns to the topic of the heading and legitimizes it by a claim that distributions according to progressions cannot be made by means of the rule of three or a single false position but only by a double false position. As regards the distribution proportionally to a given arithmetical progression this is evidently false. However, Barthélemy asks for something different, namely for the starting point α of an arithmetical

33 The three divisors written in [ ] in the diagram – sometimes as here to the right, sometimes to the left – are not in the general diagram but only in the particular examples.

34 Maryvonne Spiesser [2003: 148] finds that "the author gets lost and loses us in an exposition that seems to lead nowhere" in this change between two representations of the problem. Once we have accepted that both sets of rules offered in the first part of the chapter are inherited, one might rather find the present discussion to be a praiseworthy (and, on the conditions of the terminological difficulties, mathematically blameless) verification that the two approaches are equivalent. This time Barthélemy does not satisfy himself with a control that the two ways lead to the same numerical result (as earlier on, when the equivalence of rules (15) and (16) were argued, and as commonly done in the abbacus tradition).

progression $\alpha+(\alpha+\varepsilon)+\ldots+(\alpha+4\varepsilon)$ with given sum (e.g., 60) and given ε (e.g., 3), and then he is right. After that he submits the composite progressions to the double false. His method is not the one used in the *Istratto di ragioni* (see above, note 19) and not independent of the rules that he has already set forth (and hence it presents no alternative to these). Indeed, Δ is first found by (15^c) or (17^c), depending on the mode; afterwards, two guesses for T are used, and for each the first share ($\alpha+\phi\cdot(T-\alpha)$ or ϕT+a, depending on the mode) is calculated; from the two errors the true value of T can then be determined. In order to show how convenient guesses depend on the value of ϕ, two examples follow – $(2,3|{}^2/_7)$, for which the guesses are $T_1=\alpha=2$, $T_2=\alpha+q=9$, and $({}^1/_4|5,3)$, with guesses $T_1=q=4$, $T_2=2q=8$).[35]

The first, general part of the discussion of the use of the double false position is illustrated by a truncated version of the diagram (Figure 8), containing what is needed for the determination of Δ. Already for this reason – but also because of the rather pointless introduction of an alternative that is no proper alternative, we must presume Barthélemy to be responsible for the chimaera in question. However, the precedent of the *Istratto di ragioni* makes it plausible that the use of the rule of double false for such problems was known; this would also explain why Barthélemy dealt with the topic under a heading with which it has preciously little to do, and where the fragile connection that does exist is only shown in the very end.

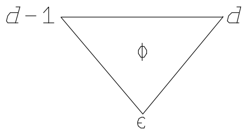

Figure 8

[35] Thereby, the complete list of Barthélemy's examples is:

$(3,3\|\frac{1}{7})$	$(\frac{1}{7}\|3,5)$	$(\frac{6}{31}\|3,3)$	$(2,3\|\frac{5}{21})$	$(\frac{3}{11}\|3,3)$
$(2,3\|\frac{2}{11})$	$(\frac{2}{9}\|3,5)$	$(3,3\|\frac{3}{11})$	$(\frac{5}{21}\|2,3)$	$(3,5\|\quad)$
$(3,2\|\frac{3}{13})$	$(\frac{6}{31}\|2,3)$	$(\frac{3}{11}6\|3,3)$	$(5,3\|\frac{6}{25})$	$(\frac{6}{31}\|3,5)$
$(3,3\|\frac{2}{9})$	$(\frac{1}{6}\|5,3)$	$(2,2\|\frac{4}{15})$	$(\frac{6}{25}\|5,3)$	$(5,3\|\frac{6}{25})$
$(\frac{1}{7}\|2,2)$	$(\frac{2}{11}\|5,2)$	$(\frac{4}{15}\|2,2)$	$(3,2\|\frac{4}{27})$	$(\frac{6}{25}\|5,3)$
$(\frac{2}{11}\|3,3)$	$(\frac{5}{19}\|5,3)$	$(3,5\|\frac{6}{31})$	$(\frac{4}{7}\|3,2)$	$(2,3\|\frac{2}{7})$
		$(\frac{6}{31}\|3,5)$	$(3,3\|\frac{3}{11})$	$(\frac{1}{4}\|5,3)$

The two columns to the left contain what is likely to be borrowed material, the three to the right what he probably constructed himself in order to illustrate the general rule and the use of the diagram.The somewhat wider limits for the choice of ϕ was already discussed; everywhere, we notice, α and ε are chosen among the numbers 2, 3 and 5.

Chuquet

Apart from Barthélemy, nobody dedicates as much space to the genre as does Chuquet. The place where he does so is in the problem collection attached to his *Triparty* from 1484. The problems, as listed in [Marre 1881: 448–451], are of the following types:

$(1,1\mid\frac{1}{10})$ $(2,1\mid\frac{1}{7})$ $(2,3\mid\frac{1}{8})$	$(2,3\mid\frac{2}{11})$ $(3,2\mid\frac{3}{13})$ $(\frac{1}{7}\mid 2,2)$ $(\frac{2}{11}\mid 3,3)$	$(\frac{1}{7}\mid 3,5)$ $(\frac{2}{9}\mid 3,5)$ $(\frac{6}{31}\mid 2,3)$	$(\frac{1}{6}\mid 5,3)$ $(\frac{2}{11}\mid 5,2)$ $(\frac{5}{19}\mid 5,3)$

The first problem in the left column is the one we encountered repeatedly, one of the two paradigmatic types – the other being $(1,1|\,^1/_7)$; the second problem is of the same kind as the one found in the *Livero de l'abbecho*. The third still has an integer denominator and looks simple, but this appearance is already deceitful: the parameters lead to a non-integer value of Δ. None of these are found in Barthélemy's text. The rest are identical with problems in the *Compendy* which Barthélemy is likely to have borrowed. Of these presumably borrowed problems only one is omitted by Chuquet – namely $(3,3|^2/_9)$; moreover, Chuquet brings them in exactly the same order as Barthélemy. This can only have one of two explanations: either Chuquet copied from Barthélemy, or both build (with or without written intermediaries) on the same written source – no oral tradition would conserve the order of 10 problems intact when this order is not dictated by some inner principle. Given that Chuquet stops exactly at the point in Barthélemy's list where the latter appears to begin his own contributions, a shared source might seem to be the most likely explanation. On the other hand, Chuquet was familiar with other parts of the *Compendy* – he refers to Barthélemy by name when discussing his solution to a problem coming shortly before the composite progressions [ed. Marre 1881: 442], and Chuquet may have chosen to stop where Barthélemy goes into a "theoretical" exposition which did not agree with Chuquet's taste. All in all, the shared source is a superfluous hypothesis which should fall victim to Occam's razor.

Chuquet's treatment of the material differs from Barthélemy's. Firstly, all Chuquet's problems are dressed in the traditional way, as dealing with a father distributing the unknown contents of a chest to an unknown number of children; even when *N* is not integer, Chuquet speaks of it as "the number of children". Secondly, he appears to enunciate only one rule,[36] after the second problem:

[36] "Appears" because Marre's transcription is incomplete, leaving out the calculations; however,

> Multiply the number which is 1 less than the denominator of the common part by the number which makes the progression. Which multiplication [i.e., product] you put aside, because it is the number of deniers which each one shall receive. Then subtract from this multiplication the number which the first one takes when he goes to the box, that is the number by which the progression begins. And multiply the remainder by the denominator of the common part, to which multiplication join the number by which the progression begins, because the addition [i.e., sum] is the number of deniers in the box. Which number divide by the multiplication which was put aside, that is, by the share which each one gets, and you have the number of children.

In symbols once more:

$$\Delta = (d-1)\cdot\varepsilon, \tag{15^c}$$

$$T = ([d-1]\cdot\varepsilon\text{-}\alpha)\cdot d+\alpha, \tag{15^a}$$

$$N = T/\Delta, \tag{15^b}$$

that is, Barthélemy's "general rule" for the first case (above, p. 370); Chuquet, however, speaks of d simply as the denominator, not as a "true denominator", and at this place in his text only integer values for d have in fact occurred.

What can be concluded is, firstly, that Chuquet knew the genre not only from the *Compendy* but also from elsewhere; secondly, that he was not very fond of Barthélemy's ways of transforming it into some kind of coherent *theory* – as we know, he had his own ways.[37] He actually closes the sequence by the remark [ed. Marre 1881: 451] *Toutes telles raisons facilement se peuent faire par la rigle des premiers*, "all such calculations can easily be done by the rule of algebra".

As we have seen, it is not quite easy to make a genuine complete algebraic solution. Whether Chuquet thought of making it is uncertain; he may well have been satisfied with incomplete solutions like the one offered by Fibonacci.

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since Marre includes one rule he would probably have included others if they had been there. This inference was confirmed to me by Stéphane Lamassé (personal communication), who has inspected the manuscript.

[37] There is indeed a fundamental difference between Barthélemy's and Chuquet's aims. Barthélemy's schemes are similar in spirit to the schemes used in Indian medieval mathematics, schemes which Nesselmann [1842: 302] saw as a kind of genuine *symbolic* algebra but which do not allow embedding and therefore can express only that which is already known as an algorithm – Barthélemy's transformation of the scheme when he replaces $^p/_q$ with $^1/_d$ is the maximal flexibility it allows and already strains it. Chuquet's use of underlining with parenthesis-function and his arithmetization of the designation of roots and powers of the unknown, on the other hand, are first steps in the development of productive symbolization (the term "productive" understood as in linguistics).

The Aftermath in Italy

The "unknown heritage" did not disappear after Chuquet, its appeal caused it to be repeated in several Italian problem collections from the 15th and the 16th centuries.[38]

One of these collections is Tomaso de Jachomo Lione's *Libro da razioni* from 1430 (Vat. lat. 4825, fol. 24^{r}), which solves a problem of the type ($^{1}/_{12}$|1,1) by means of a partially corrupt variant of the standard rule, namely to subtract 1 from 12 in order to find the number of sons, and then to multiply the outcome by itself in order to find what is due to *each* heir.

In Pierpaolo Muscharello's *Algorismus* from 1478, the last problem before the geometry section is an inheritance problem of the type (1,1|$^{1}/_{9}$) [ed. Chiarini et al 1972: 204*f*], solved by means of the standard rule (without mistake) for $d = 9$ and $d = 7$. Almost contemporary is Filippo Calandri's *De arimethrica opusculum* from 1491, republished in 1518. Here [Calandri 1518: i 5] we find a problem of type ($^{1}/_{10}$|1000,1000), with mere indication of the answer. Finally, Francesco Ghaligai's *Summa de arithmetica* from 1521

38 Among the abbacus works which I have looked through *without* finding it, Piero della Francesca's *Trattato d'abaco* [ed. Arrighi 1970], Benedetto da Firenze's *Tractato d'abbacho* [ed. Arrighi 1974] and Luca Pacioli's *Summa* [1494] should be mentioned. It is also absent from Pedro Nuñez' *Libro de algebra* [1567].

⟦In the encyclopedic *Trattato de praticha d'arithmetica* (1463; autograph Siena, Biblioteca Comunale degli intronati L.IV.21, fols 289^{v}–290^{v}), Benedetto da Firenze deals with the cases (1000,1000|$^{1}/_{7}$), ($^{1}/_{7}$|1000,1000), (3000,3000|$^{1}/_{7}$), ($^{1}/_{7}$|3000,3000), (1000,1000|$^{2}/_{11}$), ($^{2}/_{11}$|1000,1000), (2,3|$^{6}/_{31}$), ($^{6}/_{31}$12,3), (3,2|$^{5}/_{19}$) and ($^{5}/_{19}$|3,2). For the case (2,3|$^{6}/_{31}$) he offers an algebraic calculation and ensuing control clearly inspired by what we find for the same type in the *Liber abbaci*, whose selection of questions with non-aliquot fractions he also follows rather precisely though using even for these the inheritance dress).

The roughly contemporary encyclopedia Florence, Bibl. Naz., Palatino 573 (fols. 237^{v}-239^{v}) presents the cases ($^{1}/_{7}$|100,100), (100,100|$^{1}/_{7}$), (1,1|$^{2}/_{11}$), ($^{2}/_{11}$|1,1), (2,3|$^{6}/_{31}$), ($^{6}/_{31}$|2,3), (3,2|$^{5}/_{19}$) and ($^{5}/_{19}$|3,2) – similarly with calculation and control of the case (2,3|$^{6}/_{31}$) borrowed from Fibonacci.

Pacioli's Perugia manuscript from 1478 [ed. Calzoni & Gavazzoni 1996: 292*f*] presents the cases ($^{1}/_{10}$|1,1), (10,10|$^{1}/_{8}$), (10,10|$^{1}/_{10}$) and (1,1|$^{1}/_{8}$). In connection with the first it is said that "you may follow it by the *cosa* [that is, by algebra] but you cannot reduce it to a determined quantity" (showing that Pacioli has *not* used Fibonacci). Instead he gives a rule that pretends to be valid for the general case ($^{p}/_{q}$|α,α) – namely that the number of sons is q–p. This rule is claimed also to hold in the second case, and the student is exhorted to make the calculation by himself by means of the *cosa*.

The wrong rule is not Pacioli's own invention. It represents an undue reduction of a rule which we can trace back to Giovanni de' Danti [ed. Arrighi 1985: 70], that is, to c. 1370. For a problem (1,1|$^{1}/_{10}$) Giovanni states the rule "detract the 1 that is above from the 10, 9 remain, divide 9 by 1 that is above in $^{1}/_{10}$, 9 results, and 9 were the sons". The division by 1 shows that Giovanni's rule is a contraction of the valid rule $^{q-1}/_{p}$ for the case (α,α|$^{p}/_{q}$) (it follows from (16^{b}) – and from 1(1^{b}) if α = ε). Since Giovanni only presents the contracted form, he is obviously not the inventor.⟧

(later editions under the title *Praticha d'arithmetica*) has the problems $(1000{,}1000|{}^1/_7)$ and $({}^1/_7|1000{,}1000)$ and offers a slight elaboration of the usual simple rules [Ghaligai 1552: 65^r].[39] All four deal with an inheritance; for later use we observe that Ghaligai's testator is a *padre di famiglia*, a paterfamilias, and that the equality of the shares is only discovered by the heirs after the death of Muscharello's and Ghaligai's testators – two details which are not found in any of the examples mentioned so far except in part in Chuquet, who has a *père de famille*.[40]

Even the various rules for the sophisticated cases must still have been accessible in Italy (though perhaps in corrupt versions) well into the 16th century in ways we do not know about – in the *Practica arithmetice et mensurandi singularis* [1539: fol. FF ii^r], Cardano deals with the case $({}^1/_7|100{,}100)$ not according to the usual rule but in agreement with this one:[41]

$$(20^b)\qquad N = q-p,$$

$$(20^a)\qquad T = \frac{[(q-p)q]\cdot\alpha}{p^2}.$$

(20^a) results if $\alpha = \varepsilon$ is inserted in Fibonacci's rule (5^a) though with a different order of the factors, which in itself makes the *Liber abbaci* an unlikely source. However, (20^b) is a mistake[42] for

$$(20^{b}*)\qquad N = \frac{q-p}{p}.$$

[39] "Do thus, always subtract 1 from 7, that is ${}^1/_7$, 6 remains, and so many were the sons, which 6 multiply by itself, it makes 36, and this multiply by s. 1000, it makes s. 36000, and so much money was in the box; and in order to know how much is due to one, divide s. 36000 by 6, s. 6000 results"; and "subtract again 1 from 7 that have signified ${}^1/_7$, 6 remain, and so many were the sons, then multiply 6 by 7, it makes 42, …".

[40] Later on in the 16th century, Tartaglia presents the simple problem, both in the *Quesiti et inventioni* [1546: 98^{r-v}] – $(1{,}1|{}^1/_8)$, saying that it had been proposed to him in 1524 by one fra Raphaelle; and in the *General trattato* [1556: I, 245^v–246^f] – $(1{,}1|{}^1/_6)$, told here about a merchant who finds a purse and distributes the *ducati* it contains to his sons). In both works, the rule is said to be that the subtraction of 1 from the denominator gives the number of sons as well as the amount each one receives; also in both works, the outcome of variations of the denominator (${}^1/_7$ being the alternative in the former work, ${}^1/_7$ and ${}^1/_{13}$ in the latter) is explained.

[41] The story is singular: "Some dying man left sons and *aurei*, not knowing how many, and ordered that when the first returned, he should receive ${}^1/_7$ of the total and 101 [sic, typo for 100] more, and the second …". The equality of the shares is only discovered as the sons have returned, implicitly thus after the death of the testator.

[42] Since the division by 1^2 is dutifully performed in (20^a), we are really entitled to speak of a mistake.

This mistake makes it utterly implausible that Cardano would have used Fibonacci's work directly. Nor, as we see, can he have used any of the two recently printed works where the problem type is present – Filippo Calandri's and Ghaligai's. ⟦[43]⟧

Elsewhere

In a personal communication, Maria do Céu Silva has kindly informed me about two 16th-century Portuguese occurrences of the simple version and provided me with copies of the texts. The first occurrence is in Gaspar Nicolás' (or Nycolas') *Tratado da pratica Darismetyca* from [1519: fol. 59^{v}–60^{v}], the second in Bento Fernandes' *Tratado da arte de arismética* from [1555: fol. 102^{r}]. Both deal with an inheritance, and the formulations suggest them to be mutually independent. Nicolás deals with the case $(1,1|\,{}^{1}/_{7})$, with the alternative $(1,1|{}^{1}/_{9})$, Fernandes with $(1,1|{}^{1}/_{14})$. Both undertake a complete verification of the result similar to what was offered by Jacopo and by none of the later Italian authors we have considered, and Nicolás even introduces it with the same phrase,[44] but already the inheritance dress shows that Jacopo is not their source. Nor are they based directly on any of the occurrences discussed above, but both share characteristic phrases with the *Trattato di tutta l'arte dell'abacho* (above, p. 368) – phrases which are also somewhat similar to what we find in Chuquet.[45] Fernandes shares with Ghaligai the idea that the equality of the shares is discovered after the death of the testator.[46]

These similarities suggest that the Portuguese writers draw on an Ibero-Provençal rather than Italian traditions (for the present problem – in other respects it is highly probable that Fernandes drew on Italian material [Silva 2006]). The German occurrences of the problem are more likely to be based on Italian inspiration.

43 ⟦Instead, we recognize an expanded version of Pacioli's wrong rule in the Perugia manuscript, and thus the same illegitimate reduction of Giovanni de' Danti's rule – see note 38. (20^{a}), however, is neither given by Giovanni nor by Pacioli; Cardano must have had access to it in some other way.⟧

44 Jacopo, "se la voli provare", Nicolás, "se quyseres prouar". Fernandes has "como podeis prouar".

45 Where *Trattato de tutta l'arte* starts "A man has his sons, I do not know how many [non so quanti], and gives them *denari*, I do not say how many", Nicolás' problem runs "There is a man who has sons, I do not say how many [nam dygo quantos], and he also has *cruzados*, I do not say how many". Chuquet, in the same vein but not quite the same, tells that "there is a paterfamilias, who has children, one does not know [on nescet] the number. And there is in his chest a sum of *deniers*, of which one does not know the amount [le compte]".

Where the *Trattato* tells about the absolutely defined contributions that they "grow [crescie] for each one *fiorino*", Fernandes state that they "grow [vay crecendo] for each son one cruzado". Chuquet has "en augmentant tousious la porcion de ses enffans de 1 denier".

46 As we remember, Ghaligai shares the "paterfamilias" with Chuquet, who however only lets the children "discover" the equality of their shares (in fact, Chuquet does not speak of a testament but of money distributed from a chest).

The first of these is among the supplementary problems which Friedrich Amann inserted in the *Algorismus Ratisbonensis*, ms. Clm 14908 [ed. Vogel 1954: 64*f*] in 1461.[47] Friedrich (like Chuquet) does not speak of an inheritance but of a distribution of money (florins) from a *wechselpanck*. He gives two examples, one, concerning sons, of type $(1,1|^1/_{10})$, and one, concerning daughters, of type $(1,1|\,^1/_6)$. He gives the usual rule, but after the second problem he adds the rule for (a slightly corrupted version of) the problem type $(^1/_6|0,1)$, namely $N = \Delta = d$ – correct but not found in any other source.

In 1467–68, Magister Gottfried Wolack held a lecture in Erfurt University which is the earliest public presentation of abbacus mathematics we know about in Germany (unless we count the copying of manuscripts of the *Algorismus ratisbonensis* as such); its Latin manuscript appears to have had a certain influence.[48] As a “tenth rule called ‘of equality of parts’” he presents a problem of type $(1,1|\,^1/_{10})$ [ed. Wappler 1900: 52*f*], which must either be a slightly paraphrased translation of Friedrich Amann’s first problem or build on a close source for this problem; Wolack’s rule is also formulated in very similar words.

Since Johannes Widmann knew Wolack’s manuscript very well [Wappler 1900: 54*f*], Wolack *could* be behind the appearance of the same type in Johannes Widmann’s *Behend und hüpsch Rechnung vff allen Kauffmanschaften* from 1489 [ed. Gärtner 2000: 452*f*]. Widmann’s formulation, however, is quite different from what we find in Amann and Wolack – Widmann starts by explaining that the intention of the testator was to give the same to all his children. He also offers a sketch of a numerical proof similar to that of Jacopo, which none of the others have; after all, his source is thus likely to be another one. Widmann, on his part, is certainly the direct or indirect source for Christoff Rudolff [ed. Stifel 1615: 416] – Widmann’s unusual initial explanation and other particulars are borrowed. But Rudolff (whose aim it is to show the efficiency of *coss*, algebra) does not refer to a rule, instead he offers an algebraic solution (based on the equality of the first two shares, and as usually not controlling the validity of the solution).[49]

[47] For the description of the various manuscripts of the *Algorismus* and the dating of this particular part of the relevant manuscript, see [Vogel 1954: 10–12, 14]. For the identification of the frater Fridericus who wrote the manuscript with Friedrich Amann (and not with Friedrich Gerhart), see [Gerl 1999].

[48] According to Menso Folkerts (personal communication), at least six manuscripts exist (Leipzig 1470; Dresden C 80; Munich Clm 4387, Clm 26291, Clm 26639; Augsburg, StB 4° Cod. 21). Moreover, it was studied by Johannes Widmann, who may even have used it for his teaching – see [Wappler 1900: 47, 54*f*].

[49] This is at least what is found in Michael Stifel’s “improved and expanded” edition (1553); I have not been able to inspect Rudolff’s original from 1525, but [Tropfke/Vogel et al 1980: 588] signals no difference between the two versions; in both editions, the problem is no. 110 ⟦– all of which I can confirm after having had acces to [Rudolff 1525].⟧

After Rudolff and Stifel, few German authors seem to have been interested in the unknown heritage.[50] In France it had a more persistent success after having been taken up by mathematically interested humanists (like the Germans, they stick to the simple versions). The earliest examples I know about are in Joannes Buteo's *Logistica* from 1559 and Claude Gaspar Bachet's *Problemes plaisans et delectables qui se font par les nombres* from 1612 (I used the second edition from [1624]). Buteo as well as Bachet and Jacques Ozanam, the latter in 1694, take up some of the typical Ibero-Provençal formulations (not the same ones!), suggesting that the whole French branch did not depend on Italian inspiration.

Buteo [1559: 286–288], unprecedented but quite reasonably, thinks the testator must be a *vir logisticus*, a calculator; his testament is of the type ($^1/_6$|100,100). Quite exceptionally, the first share is that of the youngest heir; the equality of shares is discovered only after the testator has passed away – suggesting that the heirs/readers are supposed to expect the youngest to have received the least, with only 100 *aurei* beyond the $^1/_6$ which everybody gets (no writer for merchants and merchant sons had ever expected such mathematical naivety!).

Buteo, well versed in much more than abbacus mathematics and the abbacus norm for what constituted an adequate explanation, starts by pointing out that if each had received only $^1/_6$, the number of heirs would have been 6; under the actual circumstances, however,

> the rule is that you always remove a unit from the name of the fraction, which is 6, 5 remains for the number of sons.[51] And hundred *aurei* in addition can be nothing but the sixth of the share. This will therefore be 600 *aurei*. Multiply 600 by 5, the number of sons, it results that there were 3000 *aurei* in the money.

As we see, no argument is given for the rule N = d−1; the assertion "hundred *aurei* … can be nothing but" uses that $\Delta = ^1/_5T = ^1/_6T+100$ (whence $\Delta = ^5/_6T+100$, and therefore $100 = T-^5/_6T = ^1/_6T$). Finally it is added that the fraction cannot exceed $^1/_3$, because there

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[50] I have noticed it in Simon Jacob von Coburg's *Ein neu und Wolgegründe Rechenbuch* [1612; 236], whose first edition is from 1565. Von Coburg says that his source is Giorgio Valla's *Arithmetic* (a part of his *De expetendis et fugiendis rebus opus* from 1501, which I have not beeen able to get hold of). The ultimate source, however, is nothing but Planudes's *Calculus*; both theorem and the example are repeated. ⟦It is indeed in book IV, ch. xiii, in [Valla 1501: I].⟧

[51] The vocabulary shows Buteo to be rooted ideologically in the particular environment of French lawyer humanism – *arithmetica* is regarded as vulgar/vernacular for *logistica* (the title of the work), an aliquot part is a *particula* instead of *pars*, its denominator *particulae nomen* instead of *denominatio*, the number one to be detracted is a *monas* and no *unitas*, an amount of money (or the chest containing the money?) is *as*(!). Molière's *précieuses ridicule*s had spiritual grandfathers who were taken very seriously in their times (and afterwards) – but Buteo, prudish as a linguist, was a good mathematician.

cannot be less than two heirs, and that the denominator of the fraction always exceeds the number of heirs by a unit.

Bachet’s problem [1624: 221–226] is of the type $(1,1|^1/_7)$, dealing with “a man who is going to die”; the equality is discovered after his death. After stating the rule ($N = \Delta = 7-1$) he gives a proof that it works, very similar to that of Planudes but using (as elsewhere in the work) the particular letter formalism developed by Jordanus of Nemore.[52] After the proof Bachet points out that one may choose a different denominator (if only the same fraction is used for all children, and if only the numerator is 1 – otherwise, the problem is told to be impossible) or take different absolute contributions, if only (in our terminology) $\alpha = \varepsilon$; then N is still $d-1$, but Δ becomes $\alpha{\bullet}(d-1)$. The proof of the corresponding rule is left to the reader.

After that, the rule for the case $(^1/_7|1,1)$ is stated, and it is said that the proof is analogous. Bachet goes through the generalization to cases $(^1/_{\mathrm{d}}|\alpha,\alpha)$, and once more states (in our terminology) that d must be integer and $\alpha = \varepsilon$.

Already closer to the Enlightenment and its use of science as polite leisure is Ozanam’s *Récréations mathématiques et physiques* from 1694. The genre is represented once [Ozanam 1778:1,185], namely by the type $(10000,10000|^1/_7)$. The testator, as with Chuquet and Ghaligai, is a *père de famille*; as with Ghaligai, Buteo and Bachet, the equality of the shares is discovered after the death of the testator.

Ozanam does not state the usual rule, nor any other. Instead, his explanation runs as follows:

> One finds, by the analysis, that the possession of the father was 360000 *livres*; that there were six children, and that each of them received 60000 *livres*. Indeed, the first taking 10000, the remainder of the possession is 350000 *livres*, the seventh part of which is 50000 which, with 10000, makes 60000 *livres*.The first child having taken his share, 300000 *livres* remain; from which sum, when the second has taken 20000 *livres*, the remainder is 280000, the seventh part of which is 40000 which, with the above 20000, still makes 60000 *livres*. And so on.

It is possible (but barely) that the calculation which follows upon the phrase “indeed” (*en effet*) is meant to represent the “analysis” referred to initially (which would evidently be a misuse of this high-flown concept but might sound well in the ears of that public upon which Ozanam depended for his living); it is also possible that he did perform some kind of analysis or thought of Bachet’s proof (which indeed is no analysis but a synthesis *a posteriori*) but did not find it adequate for the same public; most likely the term is an

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[52] Bachet may have known it from Lefèvre d’Étaples’ edition of Jordanus’s *Arithmetica demonstrata* [1514]. The formalism should not be mistaken for an algebraic symbolism, since each operation leads to the introduction of a new letter. In the present case, B is thus 7, B–1 becomes A, A–1 becomes C, $A{\cdot}A$ becomes F, $B{\bullet}C$ becomes G, etc. The symbolism allows generality of the argument, not algebraic manipulation.

empty claim. In any case it presents us with no evidence that Ozanam understood the matter better than, say, Jacopo da Firenze.

As mentioned initially, Euler deals with "this question [which] is of a quite particular nature and therefore deserves attention" in the *Élémens d'algebre* [1774:488–491]. Unlike all writers on the topic since the 15th century except Bachet, Euler gives a mathematical argument for the solution. In a problem of type $(100,100|1/_{10})$, he introduces the variables z (our T) and x (our Δ), concluding that the successive remainders are z, $z-x$, $z-2x$, …, $z-5x$, …, finds the successive shares according to the prescription, and detects that the successive differences between these are "fortunately" all equal to $100-{}^{x-100}/_{10}$. Since they *should* be 0, he finds $x = 900$ (etc.).

Euler certainly *could* make a theoretically complete and coherent analysis which did not appeal to the good luck of a strongly overdetermined problem – but apparently he could not do it in a way that would fit an elementary treatise.

Theoretically complete analyses (still only of the simple version) turn up in the 19th century. Labosne [1959: 158] gives one in his paraphrase of Bachet, but there are others. Most illuminating is perhaps the treatment of the matter which is offered by Pierre Louis Marie Bourdon in his *Élémens d'algebre* from 1817 (a university textbook). Bourdon starts [1831: 66–71] by an easier version, almost the same as the box-problem version proposed by ibn al-Hā'im (see above, p. 366): The number of children is given (3), the fraction is an abstract ${}^{1}/_{n}$, the absolutely defined contributions (assigned before the fraction) are the equally abstract a, $2a$ and $3a$.[53] Afterwards [Bourdon 1831: 71–73] comes the problem $(\alpha,\alpha|{}^{1}/_{n})$, which Bourdon points out to be overdetermined; as all algebraic predecessors, Bourdon constructs an equation from the equality of the first two shares; afterwards, he shows the validity of the solution he obtains by an algebraic version of Planudes's quasi-induction – no impressive advance in an abundant half-millennium.

Whence?

We may have given up the Comtean belief in general guaranteed progress. None the less, we are accustomed to believe in over-all progress in mathematical insight since the 13th century, caused by at least three factors:

– The general intellectual climate engendered by increased schooling and literacy at all levels;
– the recovery and digestion of the ancient mathematical legacy;

[53] It is not told explicitly, as by ibn al-Ha'im, that the last share consists of *nothing but* the absolutely defined contribution; but since nothing remains after the taking of ${}^{1}/_{n}$, this should be evident. Since the calculation runs over more than five pages (whereas my complete backward calculation of ibn al-Hā'im's six-child version could be made on an A6-sheet of paper), this is hardly a proof of the superiority of Bourdon's algebra.

– the creation of new tools, first of all symbolic analysis.

The story surrounding the unknown heritage is a strange exception to this rule of progress, though admittedly concerned with a trifle which cannot change the overall picture significantly.

Indeed, our very first source for the genre – the *Liber abbaci* – also shows it in its fullest bloom, in the double sense of possessing already all the rules even for the sophisticated versions and of presenting a partial algebraic solution for one of these (showing it could be made for all cases). In the 15th century, Barthélemy also knew the rules for simple as well as sophisticated versions but offered no reasoned solution (apart from one depending on the rules); the *Istratto*, from the same century but probably going back to c. 1340, offers a partial solution of one of the simple cases by means of a double false position; dealing with a simple case, Euler does as well as Fibonacci on one of the simple cases, and uses a method which would also work for the sophisticated ones (although Euler does not say so and does not mention these) – but like Fibonacci's, Euler's approach only "fortunately happens" to work for the overdetermined problem. Well before Euler, Cardano had demonstrated some mutilated version of the full rules but not the reason they worked – which indeed *his* version of the rules would not have done for non-integer *d*.

Bodily organs which over time are gradually reduced by the combined force of mutations and selection are known as rudiments – and rudiments point back to a situation where their counterparts were fully efficient. Speaking of "efficiency" when we deal with a useless mathematical riddle may be unwarranted, but Fibonacci's and Barthélemy's rules are much too complicated to have been found by trial and error. Those who found them must have been good at mathematics – *very* good indeed, given that they found them without having symbolic calculation at their disposal. Whatever technique they used must have been quite refined, and thus carried by a competent environment – which should allow us to characterize them in a vague sense not only as "very good at mathematics" but as "very good mathematicians".

This leaves us with a first difficult question: Since the problem type appears to be unknown in the Arabic world (except for a clearly derivative, distorted import) and left no traces in pre-1500 Spain which we know about,⟦[54]⟧ *where* and *in what epoch* should we search for this environment and for these very good mathematicians?

All we can safely conclude is that they must be anterior to Fibonacci and ibn al-Yāsamīn, and that Fibonacci had access to their results. Among the places where Fibonacci declares [ed. Boncompagni 1857: 1] he had pursued the study of abbacus matters – in his boyhood Bejaïa, and afterwards "Egypt, Syria, Greece, Sicily and Provence" – only Greece (i.e., Byzantium) and Provence fall outside the Arabic orbit with certainty, while

54 ⟦Wrong, we remember, see note 26.⟧

Sicily had a mixed Arabic-Byzantine heritage).[55] The frequency with which the problem turns up in writings from early 14th-century Provence and the links between these, Chuquet and the Portuguese writers suggests that the encounter could have happened here, while Planudes's presentation of a proof that might reflect the original invention of the problem suggests the simple version might have been transmitted within the Byzantine orbit. There is also some evidence that Fibonacci encountered the simple variants in Byzantium, or wanted to suggest a link to that place. He presents these variants in inheritance dress, not as pure number problems, and the monetary unit he uses is the *bizantium*. This could seem to be a trifle until one discovers that *every* time Constantinople is mentioned together with some coin in the *Liber abbaci*, this coin is exactly the *bizantium* [Boncompagni (ed.) 1857: 94, 161, 203, 249, 274, 276]. Fibonacci's choice of coins was thus meant to correspond to real life. On its part, ibn al-Yāsamīn's problem makes it plausible that the problem was present somewhere in the Western Mediterranean before 1190.

However, neither Planudes's *Calculus* nor the writings from 14th-century Provence contain any trace of the sophisticated variants; this *could* suggest that Sicily was their cradle but proves nothing. Barthélemy's familiarity with two complete sets of rules could seem to speak in favour of Provence as an important focus, not least because Italian sources from the 140 years that separate him from Fibonacci tell us nothing about the sophisticated types. ⟦[56]⟧ Nor do the Italian sources give any information, however, about the way Cardano acquired his partial knowledge of the sophisticated rules, which none the less he did acquire.[57]

All in all, the most certain result we get from the analysis is a general admonition that known written sources may perhaps provide us with an adequate picture of what went on in mathematics in the Christian cathedral school and incipient university environment and of that level of Arabic and Byzantine mathematics that was linked to madrasa learning, recognized scholarship and astronomy; but they do not thereby provide us with anything like a complete canvas of what went on in mathematics. Even if we limit our interest to advanced matters, much remains to be known – if it *can* be known at all.

55 Fibonacci is also known to have drawn verbatim on the scholarly translations into Latin from the 12th century even though he does not mention them (see, for instance, above, note 7), but no Latin source of this kind appears to be relevant for the question.

56 ⟦Also wrong, cf. note 38 on Giovanni de' Danti. Even Benedetto da Firenze and the compiler of Palatino 573 write a few years before Barthélemy – but since they copy from the *Liber abbaci*, this is unimportant.⟧

57 ⟦Cf. note 43.⟧

Who used pebbles?

The formulation of "a first difficult question" promises that there will be at least one more question. The first question asked for the environment where the sophisticated versions of our problem were formulated and solved; the second one is a return to the question of the first origin of the simple version.

Planudes's "theorem" corroborates the hypothesis that the invention was based on pebble counters arranged in a square pattern. It constitutes no absolute proof, but let us take the hypothesis for granted for a while. Should we then make the further inference that we are confronted, if not necessarily with a Pythagorean discovery then at least with a discovery belonging within the circuit of early Greek theoretical arithmetic? This is the second question.

Prima facie, the answer need not be affirmative. Pebble arguments were certainly used within that environment – but not exclusively, as we shall see. Evidence that the general Greek public (and not only some closed Pythagorean circle) could be supposed to be familiar with them in the early decades of the fifth century BCE is offered by Epicharmos Fragment B 2 (ed. [Diels 1951: I, 196], a passage from a comedy fragment dated c. 475 BCE or earlier). It refers to the representation of an odd number ("or, for that matter, an even number") by a collection of ψῆφοι, pebble counters, as something trivially familiar.

Evidence that might seem to link the simple versions of our problem to Pythagoreanism is an observation made by Iamblichos in his commentary to Nicomachos's *Introduction*[58], and by various modern editors and commentators on Greek arithmetical writings[59] – namely that 10×10 laid out as a square and counted "in horse-race" as shown in Figure 9 demonstrates that

$$10 \times 10 = (1+2+\ldots+9)+10+(9+\ldots+2+1),$$

whence

$$10 \times 10 + 10 = 2S_{10},$$

S_n being the triangular number of order n. Rearranging and generalizing we get

$$S_n = \sum_{i=1}^{n} i = \frac{n^2+n}{2}$$

[58] Ed. [Pistelli 1975: 75^{25-27}], cf. [Heath 1921: 113*f*].

[59] The diagram described by Iamblichos is identical with what we find in J. Dupuis's edition of Theon of Smyrna's *Expositio* [1892: 69 n. 14] and in Ivor Bulmer Thomas's commentary to an excerpt from Nicomachos [1939: I, 96 n. a].

instead of the usual alternatives derived from the pairwise coupling as (1+n)+(2+[n−1])+…,

$$S_n = \frac{n \cdot (n+1)}{2} = \frac{n}{2} \cdot (n+1) = n \cdot \frac{n+1}{2}.$$

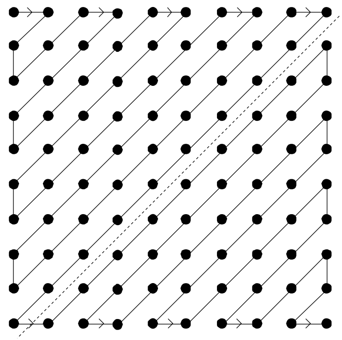

Figure 9

That the sum of two consecutive triangular numbers is a square number can be found in other authors close to the neo-Pythagorean and Platonizing current;[60] it is unlikely that anybody interested in figurate numbers would miss the point. The expression of the sum S_{10} in a way that depends on this observation is more interesting. In order to see that we shall leave the Greek cultural area for a moment.

In the cuneiform tablet AO 6484[61] (a mixed anthology text dated to the early second century BCE, thus to the Seleucid epoch), we find among other things summations of series "from 1 to 10". In obv. 1−2, 1+2+…+2^9 is found, while obv. 3–4 determines Q_{10}= 1+4+…+10^2. The latter calculation follows the formula

$$Q_{10}= \sum_{i=1}^{10} i^2 = (1 \cdot \frac{1}{3} + 10 \cdot \frac{2}{3}) \cdot 55\,,$$

which can be interpreted as a special case of the formula

$$Q_\mathrm{n}= \sum_{i=1}^{n} i^2 = (1 \cdot \frac{1}{3} + n \cdot \frac{2}{3}) \cdot S_n\,,$$

S_n being still the triangular number of order n. The determination of the factor $1 \cdot \frac{1}{3} + n \cdot \frac{2}{3}$ is described in precise detail; we may therefore be confident that the unexplained number 55 was indeed found as S_{10} in an earlier problem of the original text from which the anthology has borrowed its two summations.

P. British Museum 10520,[62] a Demotic papyrus probably of early Roman date, begins by stating that "1 is filled up twice to 10", that is, by asking for the sums

$$S_{10}= \sum_{i=1}^{10} i \quad \text{and} \quad P_{10}= \sum_{i=1}^{10} S_i$$

and answering from the correct formulae

$$S_n = \frac{n^2+n}{2} \quad \text{and} \quad P_n = (\frac{n+2}{3}) \cdot (\frac{n^2+n}{2}).$$

60 For example, Theon of Smyrna, *Expositio* I.xxviii, ed., trans. [Dupuis 1892: 68*f*].

61 Ed. [Neugebauer 1935: I, 96–99].

62 Ed., trans. [Parker 1972].

This does not overlap with the series dealt with in AO 6484, but the four summations are sufficiently close in style to be reckoned as members of a single cluster. Moreover, the cuneiform formula for Q_n follows from the Demotic formula for P_n when combined with the observation that $i^2 = T_i + T_{i-1}$.

The two texts just cited postdate the Epicharmos fragment by centuries. Their use of a formula apparently derived from a pebble-based argument might in principle represent a borrowing of results obtained by early Greek arithmeticians. However, the total absence in the same texts of anything else which recalls Greek theoretical mathematics makes such a borrowing unlikely. Independent adoption of the same type of Greek material in Egypt and in Mesopotamia is also hard to imagine, given the general absence of such borrowings from both the Seleucid cuneiform and the Demotic mathematical traditions.

Another piece of evidence also speaks against a Greek invention. The determination of

$$Q_{10} = 1^2 + 2^2 + \ldots + 10^2 \text{ as } (1 \cdot \frac{1}{3} + 10 \cdot \frac{2}{3}) \cdot \sum_{i=1}^{10} i$$

turns up again in the pseudo-Nicomachean *Theologumena arithmeticae* (X.64, ed. [de Falco 1975: 86], trans. [Waterfield 1988: 115]), in a quotation from the mid-third-century bishop and computist Anatolios of Alexandria (in a passage dealing with the many wonderful properties of the number 55). Anatolios, however, gives the sum in abbreviated form, as "sevenfold" $\sum_{i=1}^{10} i$, that is, in a form from which the correct Seleucid formula cannot be derived; this in itself does not prove that earlier Greek arithmeticians did not know better; but it shows that the Seleucid-Demotic cluster cannot derive from the form in which the formula was known to Anatolios. In addition, the absence of the formula from any earlier Greek source derived from the theoretical or Pythagorean tradition (including Theon of Smyrna and Nicomachos) suggests that the learned Anatolios has picked it up elsewhere.

All in all, the only argument in favour of a Greek theoreticians' discovery of these summation formulae is that their shape points with high certainty toward a derivation or proof based on pebbles, and only if this observation is combined with the axiom that no mathematics not inspired by the Greeks can have been based on proofs. If this axiom is given up, we may conclude the other way around: that (heuristic) proofs based on pebbles were no Greek or Pythagorean invention but part of the heritage which the Greeks adopted from the cultures of the Near East – most likely from that practitioners' melting pot of which the various shared themes and formulae of Seleucid (or earlier Babylonian) and Demotic mathematics bear witness.[63] If this is true, and *if* the inheritance problem

[63] See [Høyrup 2002].

was inspired by pebble arithmetic, then the idea might, according to the arguments given so far, just as well have arisen in the wider Near Eastern area as in a Greek environment.

However, an argument *ex silentio* supports a Greek invention.[64] Such arguments are usually weak, but the present one is not without force. Triangular and square numbers and the corresponding pyramid numbers P_n and Q_n turn up together (and always together with the sum $\sum_{i=1}^{n} i^3 = T_n^2$) in Indian sources and in al-Karajī's *Fakhrī*.[65] Higher polygonal numbers, on the other hand, are absent from these sources (of which the Indian ones, Āryabhata as well as Brahmagupta and Bhāskara II, are more systematic than can be expected from the surviving random fragments of clay tablets and papyri), although they normally go together with the triangular and square numbers and their pyramids in Greek and Greek-derived writings. This difference makes it natural to suppose that the higher polygonal numbers represent a Greek theoretical elaboration, whereas triangular and square numbers and their pyramids are part of a shared Near Eastern heritage which was to spread widely.

The Seleucid and Demotic mathematical sources also contain a number of quasi-algebraic geometric problems; even these spread widely, at least to India (more precisely to Jaina mathematics as we known it through Mahāvīra), Arabic practical geometry and Greco-Roman agrimensors.[66] The total absence of anything similar to our inheritance problem therefore speaks against its presence in the shared heritage of Near Eastern calculators.

Admittedly, the problem is also absent from those Greek and Greek-derived sources where it might have been expected to turn up – the arithmetical epigrams of *Anthologia Graeca* XIV [ed. Paton 1979: V, 25–107] and Ananias of Shirak's problem collection [ed. Kokian 1919]. But absent from these – probably because they were too difficult – are also a number of problem types which we know from their traces in Diophantos's *Arithmetica* I and elsewhere to have been known in the Greek world – the "purchase of a horse" etc.[67] Like these, the "unknown heritage" may simply have been too difficult to be included. But the invention might also be medieval – the fact that Byzantine

[64] Cf. also above, p. 358, on the apparently "traditional" character of Planudes's problem and proof.

[65] See [Clark 1930: 37] (Āryabhaṭa), [Colebrooke 1817: 290–294] (Brahmagupta), [Colebrooke 1817: 51–57] (Bhāskara II), and [Woepcke 1853: 61] (*Fakhrī*).

[66] A detailed exploration of this theme would lead much too far, but see [Høyrup 2001] [= Article I.3⟧, [Høyrup 2002], and [Høyrup 2004] ⟦= Article I.4⟧,

[67] The undressed "purchase of a horse" is *Arithmetica* I.24–25. Further evidence that such problems must have been known is offered by the "bloom of Thymarides" presented by Iamblichos in his commentary to Nicomachos's (*Introduction* [ed. Pistelli 1975: 62–67], cf. [Heath 1921: 94*f*]), and by a passage in Plato's *Republic* (333b-c, ed., trans. [Shorey 1930:I, 332*f*]), in which the purchase *in common* of a horse is said to be an occasion in which one needs an expert. ⟦Cf. article I.10.⟧

mathematical *scholarship* was not at the level of ancient theoretical mathematics – see, e.g., [Tihon 1988] – does not prove that mathematical intelligence was absent from all strata of Byzantine society.

All answers to our second question remain hypothetical, but it appears that the most plausible hypothesis is that the simple version of the problem type was invented either in Greek Antiquity or in medieval Byzantium (and perhaps transmitted from there to Sicily or Provence for further sophistication). However, any discovery of the genuine problem type (not the box version) in a medieval Indian, Persian or Arabic source would force us to evaluate probabilities anew. Even though ibn al-Yāsamīn did not know about such a thing it may after all be easier to imagine that one of the mathematicians from al-Andalus who are known by name only could have derived the intricate rules than to believe in the existence before 1200 of a mathematical environment in the non-Arabic Mediterranean world which understood mathematics better than Fibonacci.

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⟦Addendum⟧

⟦In the years after this article was finished and published I happened to work on two other topics which both point to production of advanced arithmetical theory in al-Andalus during the 12th century but have left no recognizable traces in the rest of the Arabic world (much like the philosophy of ibn Rušd, which is much better known in Latin and Hebrew translations than in Arabic). Firstly, chapter 15 part 1 in the *Liber abbaci* (see article I.15), next the *Liber mahameleth*, which I read thoroughly when preparing a review [Høyrup 2015] of [Vlasschaert 2010], and described more in depth in [Høyrup 2013]. At first I did not connect the dots, but at one moment the affinity struck me – see [Høyrup 2018]. The combined evidence leaves little doubt that the sophisticated version of the "unknown heritage" was created in al-Andalus.⟧

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Chapter 15 (Article I.14)
A Diluted Al-Karajī in Abbacus Mathematics

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

In several preceding Maghreb *colloques* I have argued, from varying perspectives, that the algebra of the Italian abbacus school was inspired neither from Latin algebraic writings (the translations of al-Khwārizmī and the *Liber abbaci*) nor directly from authors like al-Khwārizmī, Abū Kāmil and al-Karajī; instead, its root in the Arabic world is a level of algebra (probably coupled to *mu*ʻ*āmalāt* mathematics) which until now has not been scrutinized systematically.

Going beyond this negative characterization I shall argue on the present occasion that abbacus algebra received indirect inspiration from al-Karajī. As it will turn out, however, this inspiration is consistently strongly diluted, and certainly indirect.

Contents

Al-Khwārizmī, Abū Kāmil and al-Karajī

Let us briefly summarize the relevant aspects of what distinguishes al-Karajī from his algebraic predecessors.

Firstly, there is the sequence of algebraic powers. Al-Khwārizmī [ed., trans. Rashed 2007], as is well known, deals with three powers only: *census* (to adopt the translation which will fit our coming discussion of abbacus algebra), roots, and simple numbers. So do ibn Turk [ed., trans. Sayili 1962] and Thābit ibn Qurrah [ed., trans. Luckey 1941] in their presentations of proofs for the basic mixed cases, which indeed involve only these same powers.

Abū Kāmil [ed. Sesiano 1993: 398] adds to these *cubus*, *census census* and *cubus cubi*, explaining them respectively as the product of *thing* (not root) and *census*, of *census* and *census*, and of *cubus* and *cubus*; later (*ibid*. p. 404) *census census census* (the same as *cubus cubi*, as observed by Abū Kāmil) and *census census census census* turn up without being explained.

In the *Kāfī* [ed., trans. Hochheim 1878], al-Karajī stays with the original three powers; so do a number of later elementary presentations of *al-jabr*. I shall therefore say nothing more about these works. The *Fakhrī*, in contrast, starts by presenting the endless sequence of ascending powers and their reciprocals [Woepcke 1853: 48*f*], while the *Badī*' makes use of them on the basis of a reference to the *Fakhrī* [trans. Hebeisen 2008: I, 108].

Next, there is polynomial calculus. Al-Khwārizmī [ed., trans. Rashed 2007: 122–133] teaches how to multiply monomials and binomials of no more than the first degree, and how to add and subtract polynomials of no more than the second degree; since he has no negative powers and no power beyond the *census*, this exhausts what can be asked for within his framework. Though having no negative powers, he is able to divide by a first-degree polynomial and to get rid of the division as the first step in the reduction of an equation – the first of the mixed problems where it happens [ed., trans. Rashed 2007: 164–167] can for convenience be summarized as

$$\frac{2\tfrac{1}{2}\,\mathit{thing}}{10-\mathit{thing}}+5\,\mathit{thing}=50\ ,$$

which al-Khwārizmī immediately translates into

$$\frac{2\tfrac{1}{2}\,\mathit{thing}}{10-\mathit{thing}}=50-5\,\mathit{thing}\ .$$

Therefore, since "when you multiply that which results from a division by that which is divided, it gives back your possession",[1]

$$2½\ thing = (10 - thing) \cdot (50 - 5\ thing)\ .$$

But this is as far as he manages to go. A previous problem [ed., trans. Rashed 2007: 162*f*] with the structure

$$\frac{thing}{10 - thing} + \frac{10 - thing}{thing} = 2\frac{1}{6}$$

is replaced immediately and without argument by

$$thing^2 + (10 - thing)^2 = thing \cdot (10 - thing) \cdot 2\frac{1}{6}\ .$$

Possibly, al-Khwārizmī could mentally deal with two polynomial divisors at a time (but his hidden argument could also have been geometric, as it can be seen to be elsewhere in a similar case). However, the algebraic apparatus he expounds does not allow him to convey the argument to the reader.[2]

As said above, al-Khwārizmī shows the addition and subtraction of polynomials until the second degree [ed., trans. Rashed 2007: 136–143]. For binomials a geometric two-axis argument is given, for trinomials the corresponding figure is stated to be incomprehensible, while the argument from words is compelling.

What Abū Kāmil does on this account in the introduction is not very different.[3] He explains the same three powers, gives some new geometric arguments for the multiplication of polynomials, and on the other hand omits the explicit explanation of the additive and subtractive operations. The elimination of a division by an algebraic expression is explained within the problem section as done by al-Khwārizmī (and in the context of same problem) [ed. Sesiano 1993: 360*f*], and the elimination of two divisions at a time once again goes unexplained (and again in the same problem) [ed. Sesiano 1993: 365]. When the problem introducing *cubus*, *census census* and *cubus cubi* ([ed. Sesiano 1993: 398*f*], cf. above) has been reduced to

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[1] My translation into English, as all such translations in what follows.

[2] Both of these problems are also in Gerard of Cremona's translation [ed. Hughes 1986: 251*f*]. Even though some of the mixed problems are likely to have crept in later, there is thus no reason to doubt that these two belong to al-Khwārizmī's original stock.

[3] No good edition of a good manuscript exists. I have consulted [Levey 1966], [Sesiano 1993] and [Chalhoub 2004]. However, the deficiencies of manuscripts and/or editions should play no role for the present argument. My references will be to [Sesiano 1993], an edition of the certainly far from perfect Latin translation, which however presents the advantage to point out where the Latin text differs from the Arabic manuscript. ⟦Since this was written, [Rashed 2013] has appeared.⟧

$$1 \; census \; census + 12^1/_4 \; census = 9 \, cubi \; ,$$

Abū Kāmil asks to "reduce everything to 1 *census*", using the same expression as when an equation is normalized through division by the coefficient of the highest-degree member, and giving no further explanation. Even though he has not introduced the topic before, Abū Kāmil thus sees division of higher powers by a *census* as something trivially familiar. If this was a new technique developed by Abū Kāmil himself, he would certainly have explained it, and probably pointed out his own role; we may conclude that it was not.

In the *Fakhrī*, al-Karajī explains that the sequences of ascending powers and their reciprocals form two geometric series starting from unity, and shows how to divide a polynomial by a power [Woepcke 1853: 53]; formulates the general rules for the addition and subtraction of polynomials (ibid., pp. 55*f*); and points out that three-term equations in general can be solved in the same way as second-degree equations if one of the powers involved is the mean proportional between the other two (*ibid*. pp. 71*f*). He also (*ibid*. p. 63) states the theorem which in symbolic writing becomes

$$(\frac{a}{b} + \frac{b}{a}) \cdot a \cdot b = a^2 + b^2 \; ,$$

which both al-Khwārizmī and Abū Kāmil use but do not enounce explicitly.

In the *Badīʿ*, the whole of book II [trans. Hebeisen 2008: I, 105–137] is dedicated to the extraction of roots of a polynomial – which evidently presupposes everything developed in the *Fakhrī* concerning the algebraic powers and the arithmetic of polynomials, and which goes beyond it when it comes to the division by a polynomial and in its more explicit use of the notion of degree. Polynomial arithmetic, though less explicitly and with more restricted scope, also underlies much of book III on indeterminate analysis [trans. Hebeisen 2008: I, 138–187].

Thirdly, we may look at how radicals and polynomials consisting of number and radical(s) are dealt with.

Already al-Khwārizmī was (at least practically) aware that polynomials containing radicals behave like algebraic polynomials under additive and subtractive operations[4] – he treats the two together, and goes directly from the statement (and later, from the proof) that

$$(20 - \sqrt{200}) - (\sqrt{200} - 10) = 30 - 2 \cdot \sqrt{200}$$

to the statement respectively the proof that

4 We may safely assume that the linguistic coincidence – both the first-degree power and the radical being a "root" – has facilitated this insight, which then ran into no trouble in calculational practice. Later, as book X of the *Elements* was assimilated, theoretical reasons would enforce the point – but since this did not affect abbacus algebra, there is no reason to elaborate.

$$(50+10\,roots-2\,census)+(100+census-20\,roots) = 150-10\,roots-census$$

[ed., trans. Rashed 2007: 130*f*, 138–143]. He also (*ibid*. pp. 130–137) gives (explicitly paradigmatic) examples illustrating the rules

$$n\sqrt{a} = \sqrt{(n\cdot n)a}$$

and

$$\frac{1}{n}\sqrt{a} = \sqrt{(\frac{1}{n}\cdot\frac{1}{n})a}$$

as well as

$$\frac{\sqrt{p}}{\sqrt{q}} = \sqrt{\frac{p}{q}}\,,$$
$$\frac{a\sqrt{p}}{\sqrt{q}} = \sqrt{\frac{(a\cdot a)p}{q}}\,,$$
$$\sqrt{p}\cdot\sqrt{q} = \sqrt{p\cdot q}$$

and

$$\sqrt{\frac{1}{p}}\cdot\sqrt{\frac{1}{q}} = \sqrt{\frac{1}{p}\cdot\frac{1}{q}}\;.$$

Abū Kāmil [ed. Sesiano 1993: 349–355] does much the same, in part formulating things in general terms, and basing himself on geometric arguments. However, he also observes [ed. Sesiano 1993: 355–358] that expressions $\sqrt{a}\pm\sqrt{b}$ can be simplified if ab (or $^{a}/_{b}$) is a perfect square;[5] for instance,

$$\sqrt{4}+\sqrt{9} = \sqrt{4+9+2\sqrt{4\cdot 9}} = \sqrt{25} = 5,$$

while

$$\sqrt{8}+\sqrt{18} = \sqrt{8+18+2\sqrt{8\cdot 18}} = \sqrt{50}\;.$$

For numbers not fulfilling the conditions he makes the terse observation [ed. Sesiano 1993: 358] that “the question is better than the answer” – namely, that the transformation

$$\sqrt{10}+\sqrt{2} = \sqrt{12+\sqrt{80}}$$

is of no help (Abū Kāmil’s example).

5 He also mentions the possibility that both a and b are perfect squares, without pointing out that this is a stronger condition.

This last innovation, just as Abū Kāmil's occasional use of higher powers, may perhaps be seen as a starting point for some of al-Karajī's more radical innovations, and probably as evidence that Diophantos was not the only inspiration for these. At first, however, the *Fakhrī* generalizes al-Khwārizmī's rules for multiplying and dividing square roots with each other or with numbers to cube and quartic roots [Woepcke 1853: 56*f*]. It then (*ibid*. pp. 57*f*) goes on with the addition and subtraction of square roots, formulating the rules for when they are useful more clearly than Abū Kāmil, and with similar rules (and restrictions) for the addition and subtraction of cube roots, proving them by means of the development of $(a \pm b)^3$ (in the paradigmatic case $a = 3$, $b = 2$).

The *Badīʿ* goes well beyond that. When transferring the theory of irrational magnitudes of *Elements* X to an extended realm of numbers, al-Karajī adds in the end an observation about the uncountable other bi- and polynomials similar to $\sqrt{10} + \sqrt[3]{15}$ and $\sqrt{10} + \sqrt[3]{15} + \sqrt[4]{20}$ [trans. Hebeisen 2008: 70]; afterwards he gives the same rules as the *Fakhrī* for multiplying and dividing monomials (*ibid*. pp. 76–79) and for adding and subtracting binomials, now however going until quartic roots (*ibid*. pp. 80–84). Then he goes on with the multiplication of non-algebraic polynomials (*ibid*. pp. 86–89); with the division by quadratic and even quartic binomials (*ibid*. pp. 90–94), giving up in front of trinomials; with the extraction of the square root of bi- and polynomials (*ibid*. pp. 95–102); and with the cubes of the Euclidean classes of irrationals.

Abbacus algebras

Considerations similar to several of al-Karajī's innovations turn up in Chapter 14 of Fibonacci's *Liber abbaci* [ed. Boncompagni 1857: 352–387]. However, since abbacus algebra did not take its inspiration from that work,[6] I shall not engage in analysis of similarities and differences but instead turn to the abbacus treatises.

From the earliest beginning – namely the algebra chapter in Jacopo da Firenze's *Tractatus algorismi*[7] – abbacus algebra dealt not only with al-Khwārizmī's six fundamen-

6 This is not the place to argue for this claim, but see my [2007] ⟦and articles I.12 and II.11⟧.

7 [Ed. Høyrup 2007]. In principle, this algebra chapter need not belong with Jacopo's original treatise from 1307 – in any work where we do not possess the author's autograph anything can in principle have been added or changed between the preparation of the original and the writing of the shared archetype for existing manuscripts or editions. But even if this algebra should be a secondary insertion it still belongs to the early 14th century, predating all other known abbacus writings on algebra (the manuscript copy can be dated by watermarks to c. 1450); since it is obviously taken over wholesale (presumably translated from a Catalan or a Provençal source), it is uninteresting for anything but a biography of the otherwise unidentified Jacopo whether he or a near-contemporary of whom we do not even know the name carries responsibility for its adoption.

Van Egmond's attempt [2009] to date this algebra after 1390 builds on failing willingness not only to consider the sources (he only refers to equation types, never to the actual equations or examples nor to their words, perhaps simply trusting old notes of his) but even his own past

tal cases (the equation types of the first and the second degree) but also with those that can be obtained from them through multiplication by a *thing* or a *census*, and with the biquadratic obtained from the fourth case through the substitution (*thing*, *census*) -> (*census, census census*). It thus makes use of the third as well as the fourth power of the unknown and gives correct rules for all these cases – just as al-Karajī had taught, but without stating the arguments as he had done. Very soon, certain abbacus masters also invented (false) rules for solving cubic and quartic equations that cannot be solved in this way. Our earliest source for this phenomenon is Paolo Gherardi's treatise from 1328.[8] However, Gherardi (who does not go beyond the third degree) does not present us with the full range of these fanciful inventions: a list of cases put together by Giovanni di Davizzo in 1339 (borrowed into the manuscript Vatican, Vat. lat. 10488 from 1424) is so close to Jacopo yet different on certain characteristic point that it can be seen to descend from a close source of Jacopo but not from the "Jacopo-algebra" itself – but it also contains one rule which is almost illegible[9] but which is certainly false and is certainly not dealt with by Gherardi. Giovanni di Davizzo is also interesting for what precedes the list of algebraic cases: first a sequence of rules for the multiplication of algebraic powers, next misshaped rules for the division of a lower by a higher power, where negative powers are identified with roots;[10] and finally arithmetic of (numerical, not algebraic) binomials, going (now correctly) until the reduction of $\sqrt{18} \pm \sqrt{8}$ and the determination of $\frac{35}{\sqrt{4}+\sqrt{9}}$ ($\sqrt{4}$ and $\sqrt{9}$ being treated as if they were irrational).

The full gamut of powers until the eighth is mastered by Dardi of Pisa in his *Aliabraa argibra* from 1344,[11] who also explains how to reduce higher-order equations through division [ed. Franci 2001: 78*f*]. Apart from 194 "regular" cases,[12] Dardi treats of four "irregular" cases of the third and fourth degree, cases whose rules are only valid under

publications. See [Høyrup 2009b].

[8] Known from a later copy, of which [Arrighi 1987] contains the complete text and [Van Egmond 1978] an edition of the algebraic section together with an English translation and a mathematical commentary.

Gherardi's false rules emulate those for the second degree blindly, suggesting that neither he nor their inventor understood why the rules for the reducible third- and fourth-degree cases work.

[9] Somebody discovered it was wrong and glued a piece of paper over it. This scrap has disappeared, but the glue has darkened the paper, making most of the writing illegible.

[10] See [Høyrup 2009a: 56–59] ⟦= article I.12⟧.

[11] [Franci 2001] contains an edition of one of the three main manuscripts (Siena, Biblioteca Comunale, I.VII.17). The other two (Vatican Library, Chigi M.VIII.170, Arizona State University Tempe) do not differ from the one published by Raffaella Franci in ways that concern the present discussion.

[12] Dardi reaches this impressing number by making ample use of radicals, *viz* of square and cube roots of numbers as well as algebraic powers).

specific circumstances (circumstances not specified by Dardi). The dress of two of them is so different from what Dardi does elsewhere that we may be confident Dardi did not invent the group; on the other hand, their inventor, though cheating by pretending the rules are generally valid,[13] must have been quite competent in polynomial algebra.

Dardi's treatise also contains a long section about the arithmetic of numerical binomials, mostly consisting of examples in great number but also with more theoretical observations. Most place is taken up by the multiplication of second-degree monomials and polynomials, but we also find a multiplication of a cubic with a quartic root [ed. Franci 2001: 51] and examples of the addition and subtraction of square roots. In contrast to Giovanni di Davizzo, Dardi explains the condition under which the reduction is possible (*ibid*. p. 53]. He even explains (*ibid*. p. 59) how to divide by a binomial (the first example being $\frac{8}{3+\sqrt{4}}$; here, Dardi gives an argument which is likely to be his own invention and in any case not a borrowing from a tradition going back al-Karajī: it is based on the rule of three (the presence of a similar division in the Giovanni fragment suggests that the trick itself comes from tradition).

Toward the end of the 14th century, we find explicit expression of the idea that the ascending algebraic powers constitute a geometric series, namely in the algebra section of the manuscript Florence, Bibl. Naz., Fond. Princ. II. V. 152 [ed. Franci & Pancanti 1988], which on one hand contains some suggestions of fresh but indirect connections to the Arabic world,[14] but which on the other hand begins to replace the Arabic multiplicative naming of powers by naming according to the embedding principle (without doing so consistently ⟦cf. article I.14⟧). Whether the idea of the geometric progression is an independent observation or a borrowing is thus quite unclear.

Interest in the sequence of inverse powers is documented in three encyclopedic abacus treatises from around 1460–70, one of which is Benedetto da Firenze's *Trattato de praticha d'arismetrica*.[15] All three depend on Antonio de' Mazzinghi's work in various respects (taking over, so it appears, even some of his marginal annotations), and it is a fair guess that the (indubitably existing) shared source for their successful treatment of negative

13 Indeed, nobody else of the many who copy them (see article I.12) explains their restricted validity.

14 *Censo* is used in one problem about a sum of money without the author understanding to the full (when having found this *censo* he feels obliged to find its root, only having to square it afterwards); and a scheme for the multiplication of trinomials which is very similar to what was made in the Maghreb.

15 No complete edition exists, but several chapters from the manuscript Siena, Biblioteca Comunale degli Intronati, L.IV.21 have been published. [Arrighi 2004/1965] is a thorough description of the complete manuscript. As it turns out at closer analysis, this manuscript is Benedetto's working original (sometimes extensive marginal calculations were made before the main text was written).

The other two encyclopaediae are Vatican, Ottobon. lat. 3307 and Florence, Bibl. Naz. Centr., Palat. 573. Both can be seen in the same way to be their respective author's working originals, and no copy of either is known.

powers and the ratio between powers should also be identified with Antonio – thus going back to the later 14th century. (Antonio, being the first to construct tables of composite interest, understood geometric progressions to the full.)

If we count the treatment of powers in Benedetto's and the two similar treatises as a reflection of what had been done by Antonio, I know of nothing in the 15th century abbacus record which elucidates our question – from 1400 onward, the Italian development can be considered fully autonomous. What we know from the 14th century, however, shows that abbacus algebra took over ideas, problems and results which do not come from al-Khwārizmī nor from Abū Kāmil, but which are all present in al-Karajī's advanced writings. On the other hand, what is reflected in the abbacus tradition is only the elementary aspect of al-Karajī's innovations (neither root extraction of polynomials nor division by a polynomial beyond what we have seen in Giovanni di Davizzo and Dardi turns up in any abbacus source I know of). Though thus belonging to what can be characterized as an "al-Karajī tradition", it has nothing to do with the tradition which was carried by al-Samaw'al and his like. What is reflected in the abbacus tradition is *a diluted al-Karajī-tradition:* a perfect example of *Gesunkenes Kulturgut* – a product of the cultural elite of society (a category to which at least historians of science believe not only prophets and bards but also the theoretical scientists of earlier epochs should be counted) which has been adopted in depreciated shape by other social strata

– unless, of course, the few suggestions in Abū Kāmil of what was to turn up in al-Karajī's writings should indicate that the "diluted al-Karajī" was indeed already a current practice at al-Karajī's time, and that only those aspects of al-Karajī's work which are not reflected in the abbacus tradition were developed by al-Karajī himself. Since all aspects form a rather coherent theoretical whole in al-Karajī's writings, I doubt this should be the case.

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Chapter 16 (Article I.15) “Proportions” in and Around the Italian Abbacus Tradition

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Corrections of style (not least for references) made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

The language and notion of “proportions”, in the senses ascribed to the term during the epoch, are traced both in ordinary abbacus books and in those extensive works which were written in the vicinity of the abbacus culture by authors with erudite or Humanist ambitions, such as Fibonacci’s *Liber abaci*, Benedetto da Firenze’s *Trattato d’aritmetica* and Pacioli’s *Summa.* The very language turns out to have been initially absent from general abbacus culture as reflected in the ordinary books, but slowly and modestly crept in. The authors of the extensive works took up the topic, as indeed they had to if they wanted to connect to university and Humanist mathematics; but even in their case it generally remained isolated and did not penetrate their presentation of abbacus mathematics broadly to any noteworthy extent.

Contents

1 – Preliminaries

Before taking up the substance of my topic, I shall make three preliminary remarks: one on terminology, one on notation, and one on delimitation.

Terminology first. As other texts from the epoch, those I am going to consider speak of a ratio/λόγος (understood as a relation between two integers, not as a single number) as *proportio/proportione*. Some of them use *proportionalità* where we would speak of a proportion and the Greek mathematicians of ἀναλογία, that is, an affirmation that two ratios are "the same" or "similar"; others, however, use the term *proportio/proportione* even here, or speak of the numbers involved as *proportionales*. In the case of numbers being in continued proportion (ἑξῆς ἀνάλογον), moreover, our texts speak of *numeri continui proportionale*[1]*/numeros in continua proportione,* etc. An attempt to enforce a modern terminology would either divide the field in a way which does not correspond to the thought of the authors of the period, or it would force us to speak of "numbers in continued ratio" – which certainly makes sense, but is *not* modern terminology. It would also impose the modern conceptual confusion, more misleading than the medieval one, which uses "ratio" both in the historically proper sense, about the relation between *two* numbers, and about their quotient. I shall therefore translate *proportio/proportione* as "proportion", etc. – while still speaking in modern ways of ratio and proportion outside direct and indirect quotations when the relation between two numbers respectively the "similitude" between two such relations is meant; the single-number "ratio" I shall refer to as the "quotient".

Second, notation. When designating explicitly a proportion, our texts mostly say that "the first number is to the second, as the third to the fourth",[2] or use some equivalent expression. For typographical convenience, I shall use instead the notation $\frac{a}{b} : \frac{c}{d}$, which should be read as representing the frame

a	c
b	d

corresponding to what is found regularly in the margin in the *Liber abbaci*[3] and (according to Rodet as cited in [Silberberg 1895: vi, 109]) consistently in a pre-1400 manuscript of ibn Ezra's *Sefer ha-mispar* – whence probably more widespread.[4] The

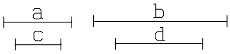
[1] Thus *Liber abbaci,* [ed. Boncompagni 1857: 171, 399].

[2] Thus the *Liber abbaci,* [ed. Boncompagni 1857: 1701]; as everywhere in the following, translations with no identified translator are mine.

[3] E.g., [ed. Boncompagni 1857: 170].

[4] It is *not,* however, in the *Liber mahameleth* (Paris, Bibliothèque Nationale, ms latin 7377A), even though this work makes use of the rectangular frame for other purposes.

two notations – as well as the line diagram used both in the *Liber abbaci* [ed. Boncompagni 1857: 395 and *passim*] and by Campanus [ed. Busard 2005: 161 and *passim*]



are equally fit to serve the visualization and automation of the various operations that can be performed on the proportion:[5]

$$\textit{e contrario}: \quad \frac{b}{a} : \frac{d}{c} \qquad\qquad \textit{conversa}: \quad \frac{a}{a+b} : \frac{c}{c+d}$$

$$\textit{permutata}: \quad \frac{a}{c} : \frac{b}{d} \qquad\qquad \textit{eversa}: \quad \frac{a}{a-b} : \frac{c}{c-d}$$

$$\textit{conjuncta}: \quad \frac{a+b}{b} : \frac{c+d}{d} \qquad\qquad \textit{aequa}: \quad \frac{a}{b} : \frac{a+c}{b+d}$$

$$\textit{disjuncta}: \quad \frac{a-b}{b} : \frac{c-d}{d}$$

and also of the equality of the products $a\cdot d = b\cdot c$ (to which I shall refer in the following as the “product rule”). The typographically convenient notation thus involves no serious anachronism – *a:b::c:d,* while fitting the phrase “the first to the second, as the third to the fourth”, corresponds less well to the diagrams on which the medieval authors based their operational thinking. In order to distinguish, I shall write fractions (including “ratios” understood as quotients) as $^{a}/_{b}$. Ratios (not understood as quotients, and not constituents of a proportion) I shall denote *a:b,* and numbers in continued proportion will stand as *a:b:c:…*.

Third, delimitation. Any applied arithmetic which goes beyond the simplest accounting runs into problems of proportionality – say, of the type “for *a* [coins], *b* [units], for *c* [coins], how much? In Near Eastern and Greek Antiquity, this would normally be solved in an intuitively transparent way: For *a* [coins], *b* [units], for 1 [coin] therefore $^{b}/_{a}$ [units], and for *c* therefore $c\cdot{}^{b}/_{a}$ [units]. Some Arabic reckoners[6] would prefer the argument “by *nisbah* [‘ratio’]”, for *a* [coins], *b* [units], for *c* therefore $^{c}/_{a}$ as much, that is, $(^{c}/_{a})\cdot b$ [units]. From India, however, probably via the trade routes and possibly with ultimate roots in China, Arabic merchants and after them theoretically inclined Arabic mathematicians from al-Khwārizmī onward adopted the *rule of three*, stating that *c* must yield $(b\cdot c)/a$.[7] Indian practical reckoners appear to have used a formulation in the style “multiply the thing [whose counterpart] you want to know by that which is not similar [to it in kind] and divide by that which is similar”. This is not the main formulation of the erudite Sanskrit writers (Āryabhaṭa, Brahmagupta, Mahāvīra, etc.), but the formulations of the latter two betray that they know it. Even in the Arabic world, it appears to have

5 This way to present them is taken from the Campanus *Elements* [ed. Busard 2005: 171*f*].

6 Thus ibn Thabāt [ed., trans. Rebstock 1993: 43–45], and al-Karajī [ed., trans. Hochheim 1878: II, 17].

7 This, and the remains of the paragraph, builds on [Høyrup 2007b: 1–8]. ⟦See also article I.5.⟧

been the formulation of merchants. The theoretically trained Arabic mathematicians soon saw that the whole matter can be based on proportion theory as found in *Elements* VII – if only we forget about the numbers being concrete and indeed being of two different kinds (for instance, dinars and cloth), and not abstract. None the less, many of the Arabic mathematicians betray familiarity with the traditional formulation, in spite of its conflict with the Euclidean approach (which requires ratios to be between quantities of the same kind, e.g., abstract numbers[8]).

In the European (that is, Italian and Ibero-Provençal) abbacus environment, the rule also arrived in "non-Euclidean" interpretation – in Italy and perhaps in Provence in the traditional ("non-similar/similar") formulation, in Spain (as we shall see) apparently in a different shape; even in the Christian world, however, theoretically trained writers interacting with the abbacus environment, from Fibonacci to Chuquet, made use of the Euclidean formulation. This, however, I shall not discuss in any depth – not because it is not interesting but because it is a separate topic, and treated at best together with other aspects of the approach to the rule of three. ⟦See article I.5.⟧

2 – Fibonacci's *Liber abbaci*

I have argued on other occasions – for example in [Høyrup 2005] ⟦see also article II.11⟧ – that Fibonacci is not the founding father of abbacus culture but rather an early (towering) exponent of a culture which already flourished in his time, if not in Italy (which seems unlikely) then in Provence, Catalonia and the Maghreb and al-Andalus, perhaps even in Egypt, Syria and Byzantium, and which was connected to a culture of commercial arithmetic ranging at least as far as Iran and India; on the present occasion I shall refer to this as the "proto-abbacus culture".

However, the *Liber abbaci* is not just an early abbacus book. Fibonacci writes *in a mathematically educated perspective* about the kind of mathematics thriving in the environment in question; but his scope is much larger, encompassing not only what he encountered on business travels to Egypt, Syria, Constantinople, Sicily and Provence [ed. Boncompagni 1857: 1] but also topics which almost certainly fell outside the horizon of the proto-abbacus culture.[9] At least part of his treatment of proportions falls in that category (but see the beginning of Section 3 for a sharpening of this statement).

The first time numbers in proportion turn up in the *Liber abbaci* is in the explanation of the algorithm for multiplying multi-digit numbers [ed. Boncompagni 1857: 15].

[8] Of course, the Euclidean approach is saved if only we use the equivalent proportion $\frac{a}{c} : \frac{b}{d}$. However, the sources never bother to perform this transformation.

[9] Bartolozzi & Franci [1990: 5], though regarding the *Liber abbaci* as the archetype for abbacus books, align it more adequately with fifteenth-century encyclopediae like Benedetto da Firenze's *Praticha d'arismetricha* and the anonymous MS Florence, Palatino 573 – on both of which below.

Combining the product rule, for which he gives an unspecific reference to Euclid, with the observation that the “degrees” or decimal levels form an infinite continued proportion, Fibonacci concludes that multiplication of the first degree by the third gives as much as that of the second degree by itself, while the second by the third gives as much as the first by the fourth, etc.

The argument could be original; I do not remember having seen it in any earlier source, not even in hints.[10] Nice though it is, it also seems to have been a historical dead end, not to be repeated by any later writer.

A next passing reference [ed. Boncompagni 1857: 82] to (four) numbers in proportion and to the product rule turns up in the explanation of the decomposition of a fraction – once more with the unspecific reference to Euclid. This is followed closely by the presentation of the rule of three in simple and composite shape, which I shall not treat in depth (but see [Bartolozzi & Franci 1990: 5–7]). I shall merely mention

– that Fibonacci does *not* use what was to become the standard formulation of the abbacus school (the one which refers to the non-similar and the similar) – the formulations [ed. Boncompagni 1857: 83*f*] are likely to be his own;
– that Fibonacci makes use of the rectangular frame mentioned above, leaving the position for the unknown number empty and indicating the cross-multiplication by a diagonal;
– that the treatment of the non-composite rule is argued from the product rule “which has been proved in the arithmetical [books of the *Elements*] and in the geometry”;
– that the composite rule (used in barter problems) is presented with a reference to *figura cata, scilicet sectoris* [Menelaos’ theorem] “which Ptolemy teaches in the *Almagest*”;

Whereas barter problems employ the rule of three “sequentially”, partnership problems use it “in parallel”; in this case [ed. Boncompagni 1857: 114*f*, 135–143], however, Fibonacci speaks of neither “proportions” nor proportionality – nor indeed of the rule of three itself, but since in general he has no name for that rule this is not astonishing. However, in connection with a problem about the alloying of three monies [ed. Boncompagni 1857: 149*f*], the first and the second in ratio 2:3, the second and the third in ratio 4:5, he speaks of “proportional alloying” and teaches how to harmonize these as easily composable ratios by means of multiplication. The idea of “proportional alloying” turns up repeatedly in the following pages. Proper interest in our topic only returns in Chapter 12, Part 2 [ed. Boncompagni 1857: 169–173].

This chapter starts by explaining equal, major and minor ratios, and gives the examples 3:3, 8:4, 9:3, 16:5, 4:8, 3:9 and 5:16 – providing them with names which are not in the

[10] It may have been inspired by analogous reasoning about the sequence of algebraic powers. The parallel between the powers of the algebraic *thing* and the powers of ten is pointed out by al-Karajī [Woepcke 1853: 48] and may have been common lore among Arabic writers 200 years later.

Boethian tradition but come close to the "denomination" (though not using this word). For instance, 16:5 is a "triple proportion and a fifth". It goes on with the problem of finding the number to which 6 has the same "proportion" as 3 to 5, giving first the numerical solution (5·6)/3 and saying then that this question is stated "in our vernacular" (*ex usu nostri vulgaris*[11]) in the phrase "if 3 were 5, what would then 6 be?". Similarly, it asks for the number to which 11 has the same ratio as 5 to 9, and gives it the vernacular formulation "if 5 were 9, what would 11 be?".

This formulation is remarkable (cf. full documentation in [Høyrup 2007a: 64–67]). Only one Italian abbacus treatise I know of identifies the rule of three by means of the same phrase, namely the *Columbia Algorism* [ed. Vogel 1977] – also untypical in other respects, almost certainly dated no later than 1290 [Høyrup 2007a: 31 n. 70] and thereby probably the earliest extant abbacus text (though known only from a 14th-century copy). Counterfactual questions – and even "counterfactual calculations" in the style "if 7 were the half of 12, what would the half of 10 be?" [ed. Boncompagni 1857: 10] – are certainly not absent from the Italian abbacus record, but they invariably turn up long after the rule of three is explained, or as secondary examples (the primary examples confronting either different currencies or goods and their monetary value). In all Ibero-Provençal treatises from before 1500 which I have inspected,[12] on the other hand, the rule of three is introduced first by counterfactual or abstract-number questions, "If 3 were 4, what would 5 be?" or "if $4^1/_2$ are worth $7^2/_3$, what are $13^3/_4$ worth?". All the Provençal specimens also know the formulation in terms of the non-similar and the similar, and so does Santcliment's Catalan *Summa* [ed. Malet 1998: 163]. Besides that, however, Santcliment informs us that this is spoken of "in our vernacular" by the phrase "if so much is worth so much,

[11] A complete survey of the references to *modus vulgaris* and its cognates in the *Liber abbaci* shows that the genuine meaning is not the generic spoken vernacular but with one exception the simple ways of practical reckoners (the exception (p. 111) is the information that an alloy of silver and tin is called "false silver *vulgariter*"). Simple, stepwise calculation is meant in four places (pp. 115, 127, 204, 364). In the last place, the *modus vulgaris* is confronted explicitly with how one procedes *magistraliter*.

[12] In chronological order

- the Castilian *Libro de arismética que es dicho alguarismo* [ed. Caunedo del Potro & Córdoba de la Llave 2000];
- the "Pamiers Algorism" [partial ed. Sesiano 1984];
- the mid-15th-century Franco-Provençal *Traicté de la praticque d'algorisme* (I used the transcription in Stéphane Lamassé's unpublished dissertation, for access to which I am grateful).
- Barthélemy de Romans' Provençal *Compendy de la praticque des nombres* [ed. Spiesser 2003: 264];
- Francesc Santcliment's *Summa de l'art d'aritmètica* [ed. Malet 1998];
- Francés Pellos's *Compendion de l'abaco* [ed. Lafont & Tournerie 1967: 132–134].

I also looked at Chuquet's *Triparty en la science des nombres* [ed. Marre 1880], not strictly Provençal but in the Provencal tradition.

how much is so much worth" (*si tant val tant: que valra tant*). The same phrase (*sy tanto faze tanto, ¿qué sería tanto?*) is also used in the Castilian *Libro de arismética que es dicho alguarismo*.[13] Wherever Fibonacci encountered the vernacular tradition he refers to, it left no conspicuous traces in Italy, but many in the Ibero-Provençal orbit, most clearly in its Iberian section.

Next [ed. Boncompagni 1857: 170], Fibonacci presents the counterfactual calculation that was just quoted ("if 7 were the half of 12, what would the half of 10 be?"), and another counterfactual simple question. He goes on with procedures for finding four and six integers in proportion if the first two of them are given; shows how to divide 10 into four unequal parts in proportion – namely by scaling an arbitrary proportion $\frac{a}{b} : \frac{c}{d}$ by the factor $^{10}/_{(a+b+c+d)}$; explains how to construct a continued proportion with an arbitrary number of terms (explaining again the product rules); and finally demonstrates how to find two or three numbers so that $^{1}/_{p}\, n_1 = {}^{1}/_{q}\, n_2$ (and, in the case of three numbers, $^{1}/_{r}\, n_2 = {}^{1}/_{s}\, n_3$) – in a different formulation, not used by Fibonacci but common in later Italian abbacus algebra, $\frac{n_1}{n_2} : \frac{p}{q}$ (and $\frac{n_2}{n_3} : \frac{r}{s}$).

On the whole, what Fibonacci does in this chapter is thus to connect procedures and problem types belonging to the "vernacular" proto-abbacus tradition(s) he had encountered with the notion of "proportions". The theoretical field itself is not explored in any way.

Theoretical exploration of a kind comes in Chapter 15, Part 1 [ed. Boncompagni 1857: 387–397], which claims to treat of "the proportions of three and four quantities, to which the solution of many questions belonging to geometry are reduced" [ed. Boncompagni 1857: 387]. Actually it deals with problems about *numbers* in proportion. These numbers are spoken of as "the first/second/third/fourth number" (or, when the numbers are three numbers, "minor/middle/major"). In most cases, they are represented by letter-carrying line segments drawn in the margin – for brevity we may designate them *P*, *Q, R* and (when needed) *S.* At first proportions involving three numbers are presented, afterwards (much fewer) questions involving four numbers are dealt with. By means of conjunction, disjunction, permutation etc., the given proportion is transformed in such a way that the numbers can be found from the product rules by means of addition or subtraction or, more often, *Elements* II.5–6 (II.6 being sometimes preferred even in cases where II.5 would seem the obvious choice). Strikingly, Fibonacci never refers to Euclid here, which he is otherwise fond of doing [Folkerts 2006: IX].

We may divide this part of the chapter into 50 sections, of which some 5 contain theorem-like observations (the delimitation is not quite sharp) and the remainder solve or show the solvability of problems.

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13 Ed. Caunedo del Potro, in [Caunedo del Potro & Córdoba de la Llave 2000: 147].

At first ((1)–(3)) come questions about three numbers in continued proportion, $P:Q:R$. One of the numbers is given together with the sum of the other two. The naming of segments presupposes the alphabetic order *a, b, c, …*.

(4)–(38) still treat of three numbers, but now differences between the numbers are among the given magnitudes. The alphabetic order underlying naming changes to *a, b, g, d, …*.

In (4)–(5), the three numbers are still in continued proportion, but now one of the numbers and the difference between the two others are given. (7)–(38) are more astonishing. They fall in groups of three, divided by separate headings by Fibonacci. To each heading corresponds one of the non-arithmetical "means" of ancient Greek mathematics [Heath 1921: II, 85–88] – geometric, harmonic, their subcontraries, etc. – and it is shown how each mean can be found from the two extremes, or one of the extremes from the other extreme and the middle. Fibonacci deals with all the non-arithmetical means defined by Nicomachos [ed. Hoche 1866: 124–144] (as followed by Boethius), but also with a mean defined by Pappos [Hultsch 1876: I, 70–73, 84–87] but left out by Nicomachos – see the scheme on page 457. However, Fibonacci does not speak of means, even though he is likely to know about them from Boethius; his order is different from those of Nicomachos and Pappos; and he does not observe that his (27)–(29) represent the geometric mean, which he has already dealt with in (4)–(5). This, together with the change of underlying alphabetic order, suggests that he has not constructed this sequence on his own under inspiration from the ancients but borrowed for an Arabic or Greek treatise on the matter, which – in the interest of completeness – had also added the case (26) even though it defines no genuine mean (as pointed out by Fibonacci, the condition $\frac{R}{Q} : \frac{R-P}{Q-P}$ implies that $R = Q$).

(39)–(50) consider four numbers in proportion, $\frac{P}{Q} : \frac{R}{S}$. The underlying alphabetic order is still *a, b, g, d, …*. At first, the *e contrario* and *permutata* transformations are set out, and it is explained how any one of the numbers can be found from the three others via the product rule. Then follow problems where two of the numbers are given together with ((40)–(45)) the sum of respectively ((46)–(49)) the difference between the two others; finally, in (50), two numbers and the sum of the squares of the remaining two is given.

In (39)–(50) and in (4)–(5), in contrast to (7)–(38), some segments are labelled by a single letter, and the letter *c* is used during the manipulations. We may therefore assume that these sequences come from Fibonacci's own pen, or (less likely, I would say) from different source than the one for (7)–(38).

Chapter 15, Part 2 is claimed to deal with "questions concerning geometry". Actually, a number of its problems have nothing to do with geometry, apart from having solutions based on line diagrams; several of these – all dealing with composite gain – involve proportions.

The first of them [ed. Boncompagni 1857: 399] is very simple. Somebody goes to one place of trade with 100 £ and earns, and afterwards earns proportionally in another

place, and then has a total of 200 £. A continued proportion shows the possession after the first travel to be $\sqrt{}(100\cdot200) \approx$ £ 141, s. 8, d. $5^1/_8$.

The next case [ed. Boncompagni 1857: 399] is somewhat more tricky. The initial capital is still 100 £, but after the first travel a partner invests 100 £ in the enterprise, and after the second travel the total amounts to 299 £. This gives the proportion (represented by lines) $\frac{100}{Q} : \frac{Q+100}{299}$. The product rule and *Elements* II.6 (still unidentified) lead to the solution Q = 130 £. Interchange of left and right would reduce this to case (46) above, but Fibonacci does not establish the link.

Then follows [ed. Boncompagni 1857: 399*f*] an example with three travels (100 £ growing to 200 £) and no extra investments, which leads to a continued proportion with four terms and thus, with reference to Euclid (namely *Elements* VII.12), a solution expressible in cube roots. A digression follows discussing numbers allowing an exact solution (24 and 81) and the notions of duplicate and triplicate proportion. Fibonacci goes on to the case of four travels, involving five numbers in continued proportion and a quadruplicate proportion; and to the concepts of quintuple and sextuple proportion.

A final problem about composite gain [ed. Boncompagni 1857: 401] deals with two travels with initial capital *P,* final total *R* and intermediate possession Q = 80 £, with $\frac{P}{R} : \frac{5^2}{9^2}$. Fibonacci calculates $5\cdot9 = 45$ and claims without explanation that $\frac{45}{80} : \frac{25}{P}$, $\frac{45}{80} : \frac{81}{R}$. The trick is of course that $\frac{25}{45} : \frac{45}{81}$, while $\frac{P}{80} : \frac{80}{R}$; a scaling with the factor $^{45}/_{80}$ conserves the ratio between the extreme terms and adjusts the value of the middle term. Finally, Fibonacci explains it to be an equivalent problem to find two numbers p and q (namely, $p = \sqrt{P}$, $q = \sqrt{Q}$) so that $^1/_5\ p = {}^1/_9\ q$, $p\cdot q = 80$.[14] This is solved via a single false position, $p' = 5$, $q' = 9$, and subsequent scaling by the factor $\sqrt{80/(5\cdot9)}$.

The notion of "proportion" or proportionality turns up in two further places in this "geometric" section. In none of them, anything profound is meant. First, a rule is given [ed. Boncompagni 1857: 401] for producing "two integer roots whose squares together make the square of a number" – that is, for finding Pythagorean triples (triangles are *not* spoken of). Second, in the last problem of the section [ed. Boncompagni 1857: 405*f*], three numbers (say, *a, b* and *c*) are asked for, so that $^1/_2a = {}^1/_3b$, $^1/_4b = {}^1/_5c$, $abc = a+b+c$. This is solved by a single false position, $a' = 8$, $b' = 12$, $c' = 15$, with consecutive proportional scaling. Similarly to what he did in the last travel problem, Fibonacci goes on to discuss what to do when there are four, five and six numbers, using once again the notions of double, triple, quadruple and quintuple proportion.[15]

[14] We recognize the structure $^1/_pn_1 = {}^1/_qn_2$, dealt with already in Chapter 12, Part 2 (see above, p. 416).

[15] Most remarkable in this problem is presumably the use of *tetragonus* in the sense of a numerical square: everywhere else in the work this is spoken of as *quadratus,* while *tetragonus* invariably refers to a geometric square (often, [ed. Boncompagni 1857: 175*f*, 368, 408*f*, 421, 426*f*, 453]) or cube (once, [ed. Boncompagni 1857: 403]). We can presume that Fibonacci used a source written

The third and final (and most famous) part of Chapter 15 [ed. Boncompagni 1857: 406–459] deals with "certain problems according to the method of algebra and almuchabala, that is, by proportion and restoration".[16] This identification of *algebra* with "proportion" and *almuchabala* with "restoration" is almost certainly Fibonacci's own invention.

Fibonacci knows the term "restoration" from Gerard of Cremona's translation of al-Khwārizmī (with which he was familiar, see [Miura 1981: 60]) and also uses it himself quite often about the cancellation of a subtractive term by addition to both sides of an equation[17] (alternatively he employs a mere "add"); but Gerard will not have helped him discover that it translates *al-jabr*.[18] On the other hand, the term used by Gerard to translate *al-muqābalah* and the corresponding verb *qabila* – that is, *oppositio/opponere* – only occurs thrice in Fibonacci's algebra chapter [ed. Boncompagni 1857: 429, 436, 457], every time in the sense of confronting the two sides of an equation (in all probability the original function of the term, but not Gerard's normal interpretation[19]).

This explains that there was space for Fibonacci's mistaken guess – he had two slots for only one technical operation. It does not explain why he used the other slot for "proportion", but at least this choice suggests him to have seen proportions as an important tool in the field. Why?

One hypothesis can be rejected straightaway. It has nothing to do with the proportional reduction of all coefficients when an equation is normalized. For this, Fibonacci uses *redigere,* as quoted in note 17, *reintegrare* [ed. Boncompagni 1857: 420], or performs the operation without naming it; neither "proportion" nor "proportional" ever occurs in this context.[20]

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in Greek without bothering to adjust its style.

16 […] *pars tertia de solutione quarumdam questionum secundum modum algebre et almuchabale, scilicet ad proportionem et restaurationem*.

17 The "equation" as a mathematical *object* is of course *our* concept and thus strictly speaking an anachronism. Fibonacci only has the action of equating – the isolated appearance of *equatio* [ed. Boncompagni 1857: 407] is to be understood as a corresponding verbal noun – *pace* Barnabas Hughes [2008: xxix, 361], who is seduced by Boncompagni's mistaken punctuation (*reddigi ad equationem. Vnius* (sic) *census per diuisionem* […] should be simply *reddigi ad equationem unius census per diuisionem* […]).

18 That Fibonacci does not discover on his own should downplay his Arabic skills, *pace* Barnabas Hughes [2008: xix].

19 There is one exception [ed. Hughes 1986: 255].

20 Barnabas Hughes suggests [2004: 324 n. 43] that Fibonacci understood "*proportio* as a kind of operation" because "the two verbs *proportionari* and *equari* […] are synonymous" in the Latin translation of Abū Kāmil's algebra. Hughes overlooks that the verb *equari* is used as an editorial explanation by Jacques Sesiano [1993: 325]. What is relevant is that the 14th-century translator

We may observe instead that Fibonacci inserts occasional pieces of reasoning based on proportion theory within algebraic or other calculations, and occasionally solves problems by means of proportion theory instead of algebra.

A simple example of the first type is found in the solution of the problem, to divide 60 *denarii* first among a number of men and then among 2 men more, by which the share of each man decreases by $2^1/_2$ *denarii*. Al-Khwārizmī [ed. Hughes 1986: 255; ed. Rashed 2007: 190–193] solves an analogous problem via (implicit) subtraction of fractions containing algebraic expressions in the denominator; Abū Kāmil [ed. Levey 1966: 106; ed. Chalhoub 2004: 76–78, 197; ed. Sesiano 1993: 370*f*] makes use of subtraction of areas within a geometric diagram; Fibonacci [ed. Boncompagni 1857: 413] replaces this "geometric arithmetic" by operations on a proportion.

A more advanced instance of the first type deals with the gains of a complex partnership: Somebody invests 12 £, and has a certain gain after 3 months. Then somebody else invests 11 £, and after another 12 months with gain at the same monthly rate, the total gain for the two is 9 £. This is expressed in line diagrams and treated *inter alia* by operations on proportions, which in the end allow the establishment of an algebraic equation.

A simple instance of the second type is an alternative solution to the problem to find two numbers with difference 6 and quotient $^1/_3$. The primary solution goes via algebra: the smaller number is posited as a *thing,* the larger is thus a *thing* plus 6, etc. Alternatively, the larger is a segment *ab,* the smaller the partial segment *ac,* whence $bc = 6$, $\frac{ab}{ac} : \frac{3}{1}$, and *disjunctim* $\frac{3}{ac} : \frac{2}{1}$, etc. For somebody as familiar with proportion techniques as Fibonacci, this may indeed have been as easy as the primary solution, and for those not yet familiar with algebra it may have been easier.

Another alternative [ed. Boncompagni 1857: 423*f*, this time to an algebraic method which is mentioned but not presented, asks for a number which, when $^1/_3$ of it and 6 are removed and the remainder multiplied by itself, yields twice the original number – in symbols,

$$(x - {}^1/_3 x - 6)^2 = 2x \ .$$

In a line diagram, Fibonacci transforms this into a proportion which in symbols becomes

$$\frac{\frac{2}{3}x}{x - \frac{1}{3}x - 6} : \frac{x - \frac{1}{3}x - 6}{3} \ .$$

Disjunctim, this allows him to apply *Elements* II.6 (unidentified once again). This time,

uses the verb *proportionari* the sense of "giving/having comparable size" a single time, in agreement with possible Italian usage (of *proporzionare*) of the late Middle Ages. It is not totally excluded – though quite improbable, given that there are no other traces of this meaning in Fibonacci's text – that Fibonacci did so too. Why should he coin a semantic neologism in the heading and never use it afterwards?

only a reader who had understood nothing of the algebra that precedes would be likely to prefer the alternative. If we observe that the underlying alphabetic order is *a, b, g, d* (which it rarely is in this section) and that the problem belongs to a family which was widespread in the "supra-utilitarian" stratum of proto-abbacus arithmetic inside as well as outside algebra – see [Høyrup 2007a: 131–133] – one may speculate whether Fibonacci found it in a source written in Greek ⟦or Arabic⟧ and presented it for the sake of completeness (which would correspond to a general practice of his).

All in all, we may conclude that "proportions" had nothing to do with algebra as Fibonacci encountered it. He writes, however, as if he thought they *should* have. Nothing suggest him to have entertained the idea that existing algebra should be illegitimate because it was Arabic, nor that he had a consistent program to replace it with something more "magisterial", legitimately belonging within the Greek realm[21] – but his global view of mathematics, coloured by his understanding of the *Elements,* and his possession of a level that enabled him to merge different approaches in a not fully eclectic manner, still made him go part of the way taken eventually with greater resolve by some Renaissance writers on algebra.

3 – Early abbacus books

Examination of early Italian abbacus books reveals that Fibonacci glued proportions not only onto algebra but also more generally to the proto-abbacus tradition, from which they were equally absent.

The *Columbia Algorism* – almost certainly the earliest extant abbacus text, cf. above, p. 415 – does not speak of "proportions" a single time, not even in the sense of ratio; the rule of three, as explained, is referred to through the counterfactual "vernacular" structure – for instance [ed. Arrighi 1989: 32] "if 25 were 12, what would 12 be?".

Slightly but hardly much younger[22] is a *Livero de l'abbecho secondo la oppenione de maiestro Leonardo de la chasa degli figluogle Bonaçie da Pisa,* "Abbacus book according to the opinion of master Leonardo of Pisa from the house of the Fibonacci" [ed. Arrighi 1989]. This treatise is a mixed compilation – see [Høyrup 2005]. A little less than half (if we count lines – well over half if we count problems) has nothing at all to do with the *Liber abbaci;* the remainder is borrowed very closely but often demonstrably without understanding from that book. Apart from the contents of a final chapter containing mixed recreational problems, everything independent belongs on the basic level, the level corresponding to what should be taught in an abbacus school. What comes from Fibonacci

[21] That is, nothing like the ideal which shines through in Jordanus's *De numeris datis* and to which Regiomontanus, Viète and others paid lip service through references to Diophantos and *analysis* – see [Høyrup 1998].

[22] For this revised dating, see [Høyrup 2007a: 31 n. 70].

is sophisticated, advanced – roughly speaking, adornment serving to show off (a purpose also ministered to by the reference to the famous predecessor in the title).

In the part of the text that is not borrowed from Fibonacci, the notion of “proportion” does not occur in any sense. The rule of three is presented in terms of the similar and the non-similar. The part copied from Fibonacci does borrow a number of references to the notion, translated either *propositione* (sometimes *prepositione*) or *proportione*. *Propositione* also occurs as translation of *petitio* or *propositio* (both referring to requests or propositions that somebody give part of his possessions to somebody else). The mix-up of *propositione* and *proportione* also turns up in other abbacus texts, facilitated probably by the possibility to abbreviate both in the same way, which might of course mislead a copyist who did not understand the text he copied. However, a global survey of the relevant passages suggests that the present compiler did not know he was sometimes speaking of proportions, glaringly misunderstood as they are occasionally (see the scheme on the next page). In any case, they were not part of his own mathematical upbringing and culture as reflected in that part of the compilation which is not copied (or miscopied) from Fibonacci.

I shall leave aside for a moment Jacopo da Firenze’s *Tractatus algorismi,* written in Montpellier in 1307, in which the notion of “proportion” does turn up a few times in particular contexts, and go on with other early abbacus treatises.

Two of these were also written in Provence: a *Liber habaci* from c. 1310, and Paolo Gherardi’s *Libro di ragioni* from 1328.[23] None of them speaks of “proportions” at all, neither under this name nor as *propositioni* – with one specific kind of exception in Gherardi’s book to which we shall return. The former gives the rule of three almost exactly as the *Livero de l’abbecho,* but differs from all other known abbacus writings on one singular account: all its integer numbers are written with Roman numerals, and all its fractions are spelled out in full words. Even the brief explanation of the place value system [ed. Arrighi 1987b: 155] does not show a single Arabic numeral. This might (but need not) reflect a style preceding the *Columbia Algorism*.

A *Libro de molte ragioni d’abaco* written around 1330 in or around Lucca by three different hands [Van Egmond 1980: 163] (and thus, we must presume, fairly representative as a total of the linguistic habits of the local environment of the time) contains two passages of interest: for the digging of a well, the toil is said [ed. Arrighi 1973: 29] to be *aproportionata* to the depth; and it is said [ed. Arrighi 1973: 31] to be necessary for a certain problem solution to be valid that Florence and Lucca are either both *proportionata* as circles or both as squares. The latter request thus refers to geometric shape, considered generically as a “proportioning”. In the former case it turns out in the following that

[23] Both are in [Arrighi 1987b]. Arrighi ascribes both to Gherardi, but gives no convincing reasons that the *Liber habaci* should come from his pen.

Occurrences of *proportione/propositione* in the *Livero de l'abbecho* (right, with page numbers from [Arrighi 1989], and corresponding passages in the *Liber abbaci* (left, with page numbers from [Boncompagni 1857].

270	que multiplica per 6 de proportione superius inventa	49	el quale multiplica per 6 de la prepositione de sopre trovata
131	que proportio est composita ex duabus datis proportionibus. Et cum proportio aliqua est composita ex quotcumque proportionibus; tunc proportio proportionum ipsa appellatur: que compositio qualiter fiat, lucidius demonstrabo	30	la quale proportione ène conposta da doie prepositione e proportione dell propositione è chiamata perch'ella se mostra chiaramente
145	argenti uncias, que fuerint in omnibus prepositis monetis, addiscas	34	le onzie de l'argento che sonno en tutte le propositione e le monete en prende
229	positis petitionibus ipsorum [for the purchase of a horse]	69	noie devemo ponere le propositione
200	ex petitionibus ex proportionibus reliquorum hominum	78	de la petitone e de la proportone degl'altre huomene
201	ex petitionibus et ex propositionibus reliquorum	78	de la petitone e da propositione degl'altre
288	secunda aliquam datam proportionem [...] qui sunt in dicta proportione	81	secondo l'altra propositione [...] che sonno ella ditta propusitione
205	hec positio per priman regulam, hoc est per modum arborum, solui possit; tamen qualiter aliter soluatur demonstrare cupimus	87	quista propositione overo quistione se può fare per la regola del primo albero, el quale mostramo chusì
286	ut invenias proportionem, quam habent ad inuicem primum, et secundum uas	100	truova la propositione che àggiono emsieme el primo e'l sechondo vaso
133*f*	proportio uniuscuiusque numeri prime coniunctionis ad 6, qui est tertius ex numeris secunde, est composita ex duabus proportionibus quattuor reliquorum numerorum	137	la propositione de ciascuno numero de la prima congiontone a 6 el qual'è el terço numero

the toil for each cubit is almost but not quite directly proportional to its depth, since the total work for depth n is as $1+2+\ldots+n$.[24] However, this numerical specification comes afterwards, the word *aproportionata* seems to stand as an explanatory everyday term, meaning loosely “corresponding to”. Apart from these two passages, due to the same hand, neither “proportion” nor “proposition“ (in any sense) can be found anywhere in the compilation. More or less specific notions of proportionality thus seem to have penetrated general language (perhaps coming in particular from the visual arts[25]).

Only texts in modern print allow text recognition and thus (at least fairly reliable) complete search. Regarding the extensive *Trattato di tutta l’arte dell’abacho* (written in Avignon in c. 1334, as shown by Jean Cassinet [2001]), only existing in manuscript form, I am therefore not able to assert that “proportion”, “proportionality” and “proposition” used for “proportion” are totally absent; however, I have consulted such passages in the earliest manuscript (Florence, Bibl. Naz. Centr., fond. prin. II,IX.57 – the author’s draft autograph) where the concepts could be expected to turn up if they belonged to the standard vocabulary of the author, without finding any of them. All in all it seems a reasonable conclusion that the relation of early abbacus culture with proportions and proportionality was like that of Molière’s Monsieur Jourdain with prose – he had spoken it for forty years without knowing anything about it. In other words: *We* may find reasoning based on proportions, but this observation of ours does not correspond to the conceptual world of the abbacus teachers.

It seems reasonable to infer that Fibonacci glued proportions not only onto algebra but onto the whole of proto-abbacus mathematics.

4 – Jacopo da Firenze and early abbacus algebra

Three manuscripts exist which claim in identical colophons to contain Jacopo’s *Tractatus algorismi,* written in Montpellier in September 1307: Milan, Trivulziana MS 90, dated by watermarks to c. 1410; Florence, Riccardiana MS 2236, undated; and Vatican, Vat. Lat. 4826, dated by watermarks to c. 1450.[26] Editions of all three are in [Høyrup

24 The toil is thus supposed to be proportional to the depth of the bottom of the stratum, not to its average depth.

25 The word family derived from “proportion” has one representative in Dante’s *Commedia divina,* namely a reference (XXX.56) to the *proporzione* of a giant, and one in Boccaccio’s *Decameron* (sesta giornata, novella sesta), a reference to the duly *proporzionati* faces produced by a painter. Both have to do with geometric shape, the latter with being well-shaped.

Not clearly linked to shape and aesthetic proportionality, however, are the observation in Dante’s *Convivio* IV, that the human intellect is *improporzionalmente* surpassed by the divine intellect, and the one in his *Vita nuova* XXV that “rhyme” in the vernacular is as much as “verse” in Latin, with the added proviso *secondo alcuna proporzione* – a *mutatis mutandis* with quantitative connotations. (I used the electronic versions of the texts on http://www.liberliber.it).

26 For these datings, see [Van Egmond 1980: 225, 148, 166]. Van Egmond gives the date 1307

2007a][27]. The Vatican manuscript contains an algebra, a chapter with problems about wages in growing continued proportion and a final collection of mixed problems which is absent from the others. As I have argued in [Høyrup 2007a: 5–25], the Vatican manuscript is a faithful copy of a shared archetype at least antedating 1328 considerably (and thus likely to be Jacopo's original), whereas the other manuscripts, very close to each other, represent an abridgment adapted to the need of the abbacus school.[28]

Let us therefore look first at the Vatican manuscript. Its algebra contains rules for all "cases" (simplified equation types) until the fourth degree which are either homogeneous or reducible to second-degree equations by division, and one of the three possible biquadratic equations. For the six equations of the first and the second degree, one or several examples are given – ten in total.

Theory of proportions is not used here, not even its simplest level.[29] However, the problem statements are of some interest. Five are sham commercial problems,[30] two number problems belong to classical types already found in al-Khwārizmī's *Algebra*. Two, however [ed. Høyrup 2007a: 307, 309], have a dress which appears not to be known from any earlier source:[31]

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for the Florence manuscript, but this is merely the date stated in the colophon, common to all three manuscripts. Since this manuscript is close to the Trivulziana 90 (see imminently) but with more errors, a date later in the 15th century is plausible.

27 The edition of the Riccardiana manuscript (as implicitly included in the critical edition with the Trivulziana manuscript) is a re-edition of Annalisa Simi's transcription [1995].

28 In a brief inserted note in his [2008: 313], Van Egmond claims that the Vatican algebra belongs to a family which he names after Benedetto da Firenze. He takes it for granted that the undated Florence manuscript is actually from 1307, refers to the Milan manuscript as "several later copies of it", and overlooks that verbatim repetition of the Vatican text in the *Trattato dell'alcibra amuchabile* from c. 1365 (with one improvement showing the model of the Vatical algebra to be earlier) excludes any date after 1365. His claim can be safely disregarded – as can much of his construction of "families", built exclusively on the appearance and order of equation *types,* with no regard for formulations, choice of examples, incipient symbolism, and almost none for terminology.

29 Yet it *could* have served. In one problem, the partnership serves instead to establish the equation, in another one dealing with composite interest the rule of three is used.

30 One of these [ed. Høyrup 2007a: 314*f*] deals with composite gain; it does not coincide with any of those found in the *Liber abbaci*, but it also leads to a problem of the second degree. In Jacopo's variant, the gain in the first travel (12 £) and the total possession after the second (54 £) are known. The problem of course involves a continued proportion, $\frac{C+12}{C} : \frac{54}{C+12}$ (*C* being the initial capital), but Jacopo deals with it by means of the rule of three – identified only as *la regola* – integrated in algebraic reasoning.

31 It *may* be related, but then only distantly, to Fibonacci's repeated reference to numbers for which $^1/_p\, n_1 = {}^1/_q\, n_2$ (two examples above, pp. 416 and 418).

> find me two numbers that are in proportion [*propositione*] as is 2 of 3 and when each (of them) is multiplied by itself, and one multiplication is detracted from the other, 20 remains

and

> Find me 2 numbers that are in proportion [*propositione*] as is 4 of 9. And when one is multiplied against the other, it makes as much as when they are joined together.

At first, these may look intricate, but at slightly closer inspection they are nothing but more complicated ways to ask for

> a number which, when multiplied by itself and by 5, gives 20

and

> a number which, when multiplied by itself and by 36, gives as much as when it is multiplied by 13.

As we shall see presently, later writers use the same principle to show off cheaply, but for long they mostly use the formulation "the first is such a part of the second as [say] 2 is of 3". Jacopo is thus not likely to have invented the mathematical principle, but the explicit use of the notion of proportion (expressed as *propositione*) *could* a priori have been his idea; see however note 34.

However that may be, the "proportion" concept turns up again slightly later, in a sequence of problems about the manager of a warehouse (a *fondaco,* written *fondicho* etc.) whose wages are supposed to increase in continued proportion. The statements run as follows:

> Somebody stays in a for warehouse 3 years, and in the first and third year together he gets in salary 20 *fiorini.* The second year he gets 8 *fiorini.* I want to know what he received precisely the first year and the third year, each one by itself. Do thus, and let this always be in your mind, that the second year multiplied by itself will make as much as the first in the third. […].
>
> Somebody stays in a warehouse for 4 years, and in the first year he got 15 gold *fiorini.* The fourth he got 60 *fiorini.* I want to know how much he got the second year and the third at that same rate. Do thus, that you divide that which he got in the fourth year in that which he got in the first year. And you will say that what results from it is cube root. […].
>
> Somebody stays in a warehouse for 4 years. And in the first year and the fourth together he got 90 gold *fiorini.* And in the second year and the third together he got 60 gold *fiorini.* I want to know what resulted for him, each one by itself. And let them be in proportion and let the first be such part of the second as the second of the third, and as the third of the fourth. And let it always stay in your mind this, that to multiply the first year in the fourth makes as much as the second year in the third. And it makes as much to divide the fourth year in the second as the third year in the first. […]. And 40 *fiorini* he got the third year. And it is done, and you see well clearly that each of these numbers are in

> proportion. And such part is the first of the second as the second of the third, and as the third of the fourth: each is the half. [...].
>
> Somebody stays in a warehouse for 4 years. And in the first year and the third together he got gold *fiorini* 20. And in the second and the fourth year he got gold *fiorini* 30. I want to know what was due to him the first year and the second and the third and the fourth. And that the first be such part of the second as the third is of the fourth. [...] .

As we see, the geometric proportion is taken for granted, as belonging tacitly to the dress. As soon as the procedure is explained, however, the necessary fundaments of proportion theory turn up, and in the third and fourth problem we even find the word (in the shape *propositione*[32]) together with the alternative formulation "to be such a part as".

The third problem goes beyond what can be found in the *Elements,* though it is based on knowledge which had been current in Arabic scientific mathematics since al-Karajī. If *a, b, c* and *d* designate the respective yearly wages, the first step of the solution is to state that

$$a \cdot d = b \cdot c = \frac{(b+c)^3}{3(b+c)+(a+d)}$$

This certainly goes beyond Jacopo's mathematical competence. *He* cannot have invented the problems. On his own he *may* have had the idea to introduce the term *propositione* – though no scholar he was not quite without scholarly pretensions, his five-line colophon is in Latin. On the whole, however, the appearances of the word in the algebra and in this quasi-algebraic chapter are more likely to have been borrowed together with the rest of that text: it turns up nowhere else in the treatise, and seems well integrated when appearing in the *fondaco*-problems. His pretensions may then have caused him not to eliminate it.[33]

Evidently, the references to proportions in the Vatican manuscript have no counterparts in the Florence and Milan manuscripts, from which the very chapters where they should turn up are missing. None the less, the word appears a single time [ed. Høyrup 2007a: 420], namely in the counterfactual calculation "if 5 times 5 made 26, say me how much 7 times 7 would make in that same proportion [*in quela medesima proportione*]" – that is, in the sense of "rate"; the result is then stated with the words *diremo che 7 via 7 facia 50 et* $^{24}/_{25}$ *a quela medesima rasone,* "we shall say that 7 times 7 makes 50 and $^{24}/_{25}$ at this same rate". Further on, *rasone* is used in a counterfactual calculation which follows immediately, and in nine places where rates are spoken of. In the Vatican manuscript, the first counterfactual question [ed. Høyrup 2007a: 238] has *ragione* where the Milan

32 It should be noted that something which is proposed is spoken of as a *proponinento* by Jacopo [ed. Høyrup 2007a: 250, 425].

33 Given the spelling *propositione* and the general conscientious precision of the manuscript we possess the term is not likely to have been inserted in the text during the transmission process.

and Florence manuscripts have *proportione*. This single appearance of the word is probably a secondary modification reflecting a general tendency in the later 14th century to absorb bits of the terminology of university mathematics.

A number of abbacus texts written between 1307 and 1345 (all mentioned above) contain a smaller or larger amount of algebra:

- Paolo Gherardi's *Libro di ragioni* from 1328;
- The *Libro de molte ragioni d'abaco* from c. 1330;
- The *Trattato di tutta l'arte dell'abacho* from c. 1334;

Gherardi has a systematic presentation of algebraic cases – all cases until the third degree treated by Jacopo, four more cases of the third degree which cannot be reduced to quadratic equations (the solutions for which are therefore false, produced by superficial imitation of second-degree solutions), and the case "cubes equal to square root of number". Ten of these are illustrated by problems of the type asking for numbers in given ratio (invariably, when more than two numbers are involved, as *n:m* and as *m:p,* etc., avoiding thus the need for composing ratios); but in seven of them the formulation of the matter is "such part … as *m* is of *n*". Only three [ed. Arrighi 1987b: 102, 106, 107] ask for "3 numbers that are in position [*positione*] together, that is, the first of the second as 2 of 3, and the second of the third, as 3 of 4".[34]

In the *Libro de molte ragioni d'abaco* and the *Trattato di tutta l'arte dell'abacho,* problems with the "such part" formulation are found, but never the "proportion" formulation.

In 1344, a certain Dardi of Pisa wrote the first treatise in the abbacus tradition dedicated exclusively to algebra.[35] This work contains several hundred problems, a large part of which deal with two or three numbers in given ratio. Mostly these use the formula "such part … as *m* is of *n*". In one case, however ([ed. Franci 2001: 89], similarly the manuscripts; counted as no. 10 by Dardi), a two-number problem asks for "two proportional numbers in continued proportion [*proportionali in continua proportione*] so that the first is such a part of the second as 4 is of 5". More meaningful is the question ([Franci 2001: 139], similarly the manuscripts; Dardi's no. 64) for "three numbers in

[34] But still three, while one example which is shared with Jacopo has the alternative formulation. This decreases the likelihood that Jacopo should be the one who introduced the proportion formulation (and the respective *propositione/positione* suggests perhaps shared dependency on a source or environment where *proportione* was reinterpreted as one or the other, either because numbers may be *positioned* in ratio or *posited* (namely, as 2 *things* and 3 *things* if their ratio is 2:3), or because a specific ratio is *proposed* (but cf. note 32).

[35] I have consulted Vatican, Chigi M.VIII.170 from c. 1390; [Franci 2001], an edition of Siena, I.VII.17 (c. 1470); and Van Egmond's personal transcription of the Arizona manuscript written in 1429 (for access to which I am grateful).

continued proportion [*in continua proportione*], that is, that the first is of the second as the second of the third, and be such a proportion as 2 of 3" – but we notice that the final words of the question uses "proportion" as a synonym for "part". In three later problems of the same type (Dardi's nos. 67, 69 and 75), the Arizona manuscript replaces the information about the continued proportion by the phrase "that one is such a part of the other as …", apparently meant as "each … of the following one". The Vatican and Siena manuscripts have the same construction in no. 75, but state "that the first is such a part of the second as …" in no. 69, saying nothing about the ratio between the second and the third. So does the Vatican manuscript in no. 67, whereas the Siena manuscript adds "and that they are in continued proportion". It seems likely that the Arizona manuscript corresponds to the original on this point, and that Dardi has thus explained the notion of continued proportion in no. 64 (after having used it wrongly in no. 10), and afterwards just uses "one …the other" as a way to indicate a repeated ratio; Siena and Vatican at first overlook this finesse, but in no. 69 Siena sees that information is then missing; in no. 75, both copyists have discovered.

Dardi, like Jacopo, has scholarly pretensions (and much higher mathematical competence and ambitions, but that is immaterial in this connection): his preface explains [ed. Franci 2001: 37] the four Aristotelian causes (*rispetti*) of his book, in the best scholastic manner. He *may* therefore have adopted a term from Latin university mathematics, without having much use for it (and, as we see in no. 10 and no. 64, without being quite sure of its use). His treatise is thus yet another piece of evidence that *proportione* and *proportionalità* did not yet belong to the standard terminology of the abbacus ambience.

Before c. 1340, a master Biagio known later as *il Vecchio,* "the Old", wrote an abbacus treatise which has been lost, but from which a collection of algebraic problems was copied by Benedetto da Firenze for his encyclopedia (see note 9). This collection confirms the picture, and adds some shades (with the proviso that we cannot be quite sure Benedetto did not change the precise wording of his model).

Firstly, we find again a large number of problems asking for numbers or quantities in given ratio – 19 in all.[36] Only the last of them [ed. Pieraccini 1983: 126] asks for "2 numbers, or 2 quantities, which are in proportion as 5 to 7, that is, that the first quantity is to the second as 5 to 7"; all the others ask either for quantities (10 of them) or numbers (8 of them), and all use the formula "such part … as *m* is of *n*".[37] We may speculate

[36] One of them [ed. Pieraccini 1983: 18*f*] asks for three number in ratios 2:3 and 2:5, but this does not lead to a presentation or investigation of the composition of ratios: Biagio simply posits the numbers to be 2 *things*, 3 *things*, and 7½ *things* without explanation. All others, as usually, have the ratios nicely fitting together.

[37] The homonymy should not mislead us into believing that the "quantities" are continuous

that the first 18 occurrences are borrowed material, and the last one Biagio's own construction, in which he shows the applicability of the proportion concepts to this problem type (and points to the equivalence of number- and quantity-formulations).

This is not the first time Biagio refers to "proportions". In a problem about a loan with compound interest over three years [ed. Pieraccini 1983: 67–69] he introduces the notion of a continued proportion and uses the product rule to establish the equation. Later on, in Jacopo's fourth *fondaco* problem (still told about the manager of a *fondaco,* with the data 40 and 60) [ed. Pieraccini 1983: 89–91], he first shows that the product rule $ad = bc$ gives a tautology, and then that the rule $ac = b^2$ yields an equation.[38] In an indeterminate problem about three monies with unknown metal content [ed. Pieraccini 1983: 109*f*] he postulates that the quantities are in continued proportion with ratio 2:1, and thereby gets a single determinate equation. Finally, in a more intricate problem [ed. Pieraccini 1983: 119–121] about composite gain – given difference between the interest rates and given ratio between the total interests of the first and the second year – this ratio is at first defined as being "as 2 to 3", but when it is used later we are told that "the proportion of the interest of the first year and that of the second is as 2 to 3". The last instance (perhaps also the second-last) sounds as if the idiom of proportions fell natural for Biagio; the formulations of the final problem about "2 numbers, or 2 quantities" may then indicate that he was aware that his public was less familiar with it. However that may be, 7 occurrences of the words *proportione* and *proportionalità* in a text of some 30000 words must be characterized as a modest intrusion in Biagio's language.

A final algebraic treatise, written after our limit 1345 but throwing light on the early epoch, is a *Trattato dell'alcibra amuchabile* from c. 1365 [ed. Simi 1994]. It consists of three parts

- rules for calculation with signs, square roots and binomials consisting of number and square root;
- a list of algebraic "cases", provided in part with examples;
- and a collection of problems.

Only the second of these concerns us at present [cf. Høyrup 2007a: 160,163]. It contains all of Jacopo's cases including his examples almost verbatim. This segment of the second part almost certainly descends from Jacopo's text, and it is therefore no wonder that the examples with numbers in given ratio speak of "proportion", just like Jacopo. More interesting is that it also presents cases and examples which are in Gherardi's algebra but not in Jacopo's, moreover in a version which appears to antedate Gherardi's – seven

magnitudes – lengths, areas, volumes, durations, weights – as they would have been in contemporary Aristotelian university discourse. None of the authors I treat before Pacioli uses the term in this way.

[38] Jacopo, in contrast, had only provided a rule for determining the solution.

examples in total, all constructed around numbers in given ratio(s). Four of these ask for numbers *in proporzione,* only three use the "such part" formulation. Of Gherardi's counterparts, 6 are of the latter type, only one asks for numbers *in positione;* this latter question corresponds to a "proportion"-formulation in the *Trattato.* It thus looks (but the statistics is not sufficient to allow any certainty) that Gherardi had a tendency to use "such part" even when his source (we may presume) had *propositione* or *positione;* this augments the likelihood that Jacopo did not introduce the "proportion" language on his own but took it over from his source.

5 – Antonio de' Mazzinghi

Antonio de' Mazzinghi (probably c. 1355 to 1385/86, see [Ulivi 1996: 109–115]) is praised highly for his algebraic competence in three encyclopedias from the 15th-century,[39] which are also our only sources for his mathematics. The largest extract is his *Fioretti* [ed. Arrighi 1967a].

This is an outstanding text, which fully confirms the praises heaped upon him. That is not what concerns us here, but it is good to know as a background to what follows.

The *Fioretti* do not contain a single problem of the kind asking for numbers or quantities in given ratio. We may guess that Antonio found it below his mathematical dignity to stoop to using such cheap tricks; alternatively, he may not have found them fitting for a collection of "blooms".

Two problems have a formal similarity with the cheap type [ed. Arrighi 1967a: 46–51], asking indeed for numbers in ratio – but this ratio is not given numerically but as that between two other numbers fulfilling algebraic conditions. Written in letter symbols, the respective structures are

$$ab=(a\text{-}b)^2, \quad \frac{c}{d}:\frac{a}{b}, \quad 19=c+d, \quad c\cdot d=c^2+d^2$$

and

$$a^2+b^2=60, \quad \frac{c}{d}:\frac{a}{b}, \quad c\cdot d=10, \quad c^2+d^2=ab$$

The first is the one where Antonio famously has to invent a trick that enables him to calculate with two unknowns.[40]

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[39] Benedetto's *Praticha d'arismetricha,* see above, note 39, Vatican, Ottobon. lat. 3307; and Florence, Bibl. Naz., Palatino 573.

[40] If for example 10 has to be divided into two parts, it was often found convenient to take these as 5–*thing* and 5+*thing.* As we have seen, an unspecified number is regularly also spoken of as a "quantity" Antonio combines the two ideas, taking a to be "a *thing* minus a *quantity*", b to be "a *thing* plus a *quantity*". The intellectual jump involved in this seems to have gone almost unnoticed at the time and to have inspired little imitation, maybe because the use of habitual words made Antonio's readers (including perhaps Benedetto, but see imminently) overlook that something (potentially) important had occurred – exactly as had happened to Fibonacci's similar trick (using

In problems about compound interest, Antonio points out with greater clarity than any predecessor [ed. Arrighi 1967a: 36] that interest *a chapo d’anno,* “[making up accounts] at the end of year” (that is, compound interest) “proceeds in continued proportionality”, not only with correct calculations up to five years grounded explicitly in the product rule (thus no longer the rule of three) but also with the finding of equivalent rates of interest if accounts are made up every 8 or every 9 months. Belonging to the same field of theoretical interest is the problem [ed. Arrighi 1967a: 69] of finding a five-term continued proportion beginning with 16 and ending with 81.

A large number of problems ask for three or four numbers in continued proportion which fulfil other algebraic conditions of the first or the second degree. In symbolic abbreviation and with the numeration of Arrighi’s edition (taken from the manuscript) they are:

#1	$19=a+b+c$, $a\cdot(b+c)+b\cdot(c+a)+c\cdot(a+b)=228$
#2	$a\cdot(b+c)+b\cdot(c+a)+c\cdot(a+b)=888$, $a^2+b^2+c^2=481$
#3	$9\,^1/_2=a+b+c$, $a^2+b^2+c^2=33\,1/4$
#4	$19=a+b+c$, $3a+4b+5c=81$
#5	$a+c=21$, $b+d=39$
#8	$a\cdot b\cdot c\cdot d=2916$, $a+b=17\,1/2$
#23	$a+b+c=14$, $a\cdot b\cdot c=64$
#25	$a^2+b^2+c^2=84$, $^{20}/_a+^{20}/_b+^{20}/_c=125$
#26	$10=a+b+c$, $3a+4b=5c$
#29[41]	$c-a=50$, $d-b=80$

Repeatedly, as can be expected, the solutions make use of the product rule. A couple of times, however, Antonio appeals to more advanced properties of proportions or

res and *causa* for the two unknowns) in the *Flos* [ed. Boncompagni 1862: 236].

Benedetto does use *quantity* as an algebraic unknown in his *Tractato d’abbaco* [ed. Arrighi 1974: 153, 168, 181] (Arrighi ascribes the treatise to Pier Maria Calandri), namely in solutions by means of *modo retto/repto/recto*, first-degree algebra designated *regula recta* by Fibonacci, who calls the unknown *res* [ed. Boncompagni 1857: 191 and *passim*].

The *quantità* also turns up in this function in his *Trattato de praticha d’arismetrica* – in the manuscript Siena, Biblioteca Comunale degli Intronati, L.IV.21 (Benedetto’s original, cf. below, note 46), fol. 263^v even together with a second unknown *borsa,* [the unknown contents of] a purse. This casual naming might perhaps be taken to suggest that Benedetto had assimilated the possibility of two unknowns to such a degree that he could implement it on the rare occasions where he would need it without even thinking of it as a borrowing from Antonio.

41 Actually, #29 starts with a problem $10=a+b$, $a^2+b^2+\sqrt{a}+\sqrt{b}=86$, and a position $a=5-t, b=5+t$. When this has been reduced to

$$\sqrt{5-t}+\sqrt{5+t}=36-2t^2$$

Antonio comments “I do not like it, and therefore I do not complete it” – and goes on with the problem about three numbers in continued proportion.

continued proportions. In #25, he finds it "rather clear and obvious" (*è cosa assai chiara e manifesta*) that if a, b, and c are in continued proportion, then the same can be said about ${}^{20}/_{a}$, ${}^{20}/_{b}$ and ${}^{20}/_{c}$ [ed. Arrighi 1967a: 54]. In #29, the *disjuncta* mode is described and used [ed. Arrighi 1967a: 63].

The genre as such was not new, neither in general nor to abbacus mathematics. Abū Kāmil [ed., trans. Levey 1966: 186; Sesiano 1993: 405; Chalhoub 2004: 148] has a problem about numbers in continued proportion with the structure

$$10 = a+b+c, \quad a^2+b^2=c^2,$$

and there are two examples in the third part of the above-mentioned *Trattato dell'alcibra amuchabile* [ed. Simi 1994: 39] – in symbolic abbreviation

$$10 = a+b+c, \quad a \cdot b = 4, \quad b \cdot c = 8$$

and

$$10 = a+b+c, \quad a^2+b^2+c^2 = 40$$

The former is overdetermined and impossible, and the solution which is proposed is wrong. The latter, we observe, has the structure of Antonio's #3. Antonio's #5, on its part, has the same structure as Jacopo's fourth *fondaco* problem, the one which was also solved by Biagio. Antonio solves it in the same algebraic way as Biagio, omitting however Biagio's pedagogical blind alley. I know of no evidence allowing to decide whether the number genre as found in the *Trattato dell'alcibra amuchabile* has the same ultimate origin as the *fondaco* genre, or Antonio fused the two.

What seems fairly certain is that the present type of number problems has no strong links to Fibonacci's division of 10 into four unequal parts in proportion [ed. Boncompagni 1857: 170]; Antonio certainly knew and appreciated Fibonacci,[42] but nothing suggests the same for the compiler of the *Trattato dell'alcibra amuchabile* or his source. Moreover, Fibonacci speaks of *any,* not a continued proportion (and uses the example $\frac{3}{7} : \frac{6}{14}$); afterwards he shows how to construct a sequence of any length of numbers in continued proportion, but now without constraint on their sum.

Towards the end of the *Fioretti* comes a section "Mirabile dictum" [ed. Arrighi 1967a: 81–87], showing how to divide a number (say, N[43]) into parts (say, a, b, c, d and e) in such a way that

$${}^{N}/_{a} + {}^{N}/_{b} + {}^{N}/_{c} + {}^{N}/_{d} + {}^{N}/_{e} = N.$$

This section is analyzed in [Bartolozzi & Franci 1990: 10*f*]. Since it was certainly due to Antonio himself and had little further impact in the abbacus tradition which I know

42 Quotation in Ottobon. lat. 3307, ed. [Arrighi 2004/1968: 221].

43 Antonio builds his solution up around the core ($\sqrt{6}-\sqrt{2}$, $\sqrt{6}+\sqrt{2}$), but then states that any couple of binomials $\sqrt{b} \pm \sqrt{a}$ serves if $b{:}a$ is a multiple. Unfortunately, as pointed out by Bartolozzi & Franci [1990: 10 n. 16], Antonio's condition is insufficient.

of (apart from being copied by an obviously impressed Benedetto and being used by Pacioli, see below), I shall not discuss it any further.

Antonio was familiar with Book 15, Part 1 of the *Liber abbaci* – Palatino 573 from the late 1450s quotes his *Gran trattato* for presupposing “that the proportions from the first part of the 15th chapter [of the *Liber abbaci*] be clear to you” [Arrighi 2004/1967: 190]. But this familiarity left no trace in the *Fioretti.* Whether Antonio thought of the connection to the Boethian means cannot be decided with certainty, but since those who read him have not noticed it, it is unlikely.

6 – Late 14th century otherwise

Like Fibonacci, Antonio knew theoretical mathematics well enough to adopt it creatively into his abbacus heritage. He was exceptional, and in consequence an exception; to what extent would his near-contemporaries make use of the notion of “proportion”, in any of its possible senses? Two examples will have to suffice.

The first is Giovanni de’ Danti d’Arezzo’s *Tractato de algorisimo* [ed. Arrighi 1987a] from 1370. This is a decent but not sophisticated abbacus book, containing no systematic presentation of algebra but a short passage about the arithmetic of square roots and a few algebraic problems [ed. Arrighi 1987a: 52–57, 65–69]. Giovanni’s remoteness from any scholarly mathematical environment is illustrated by his explanation [ed. Arrighi 1987a: 53] of the existence of surds: God does not want that anything but himself be perfect.

The word “proportion” (in any of the spellings we have encountered) is as absent from this treatise as from the non-Fibonacci parts of the *Livero,* from the *Liber habaci,* and from the non-algebraic parts of Jacopo’s *Tractatus* and Gherardi’s *Libro di ragioni. Propositione* is found often, but it means “a question which is proposed”.

There are seven problems asking for numbers in given ratio; all three use the “such part” formulation. For once, the same formulation is also found in a non-algebraic business problem [ed. Arrighi 1987a: 34]: A loan, on which the interest in the first year is such a part of that in the second year as 3 is of 4. Since nothing similar is found in treatises from the first half of the century, this type is likely to be an offset from the analogous pure-number problems.

The second is a *Trattato d’algibra,* constituting the last fifth of a larger abbacus treatise (*Tratato sopra l’arte della arismetricha*), according to internal evidence written in the 1390s – thus after Antonio’s death, but apparently in the tradition after Biagio (or his source). Only the algebra has been published, namely in [Franci & Pancanti 1988]. My discussion is restricted to this published part, I have not seen the manuscript.

In this algebra, the word *proporzione* turns up in two different contexts – in the theoretical introduction, and in the problems.

The theoretical introduction [ed. Franci & Pancanti 1988: 3–6] is an investigation of the sequence of algebraic powers. This introduction is both interesting and puzzling –

see [Høyrup 2008: 30–32]. What concerns us here, however, is merely that the sequence of powers is seen to be in continued proportion, which is used to show that *censo* times *censo* is the same as *thing* times *cube*, and *censo* times *cube* as much as *thing* times *censo di censo.*[44]

Proportions also turn up in four problem types: In problems about compound interest over three or four years (very similar to what Biagio does); in the *fondaco* problem also solved by Biagio;[45] in two problems about the division of ten in three parts in continued proportion – one analogous to Antonio's #3 and the second problem cited from the *Trattato dell'alcibra amuchabile,*

$$10 = a+b+c, \quad a^2+b^2+c^2=70,$$

and one similar to Antonio's #4,

$$10 = a+b+c, \quad 3a+4b+5c = 35\,;$$

and finally in a problem about three quantities of money (in the sense of coinable metal) in continued proportion, structurally identical with the problem shared with Antonio and the *Trattato dell'alcibra amuchabile*. There are also numerous problems about two, three or four numbers in given ratio, all in "such part" formulation. In an inverse variant of the well-digging problem from *Libro de molte ragioni d'abaco* nothing is said about the toil being in correspondence (*aproportionata* or otherwise) with the depth, we only get the solution by means of triangular numbers.

This treatise is mathematically very sophisticated. None the less, as we see, the use of proportion theory (or the very recourse to the terminology) expands only slightly beyond what was known at least since Biagio: proportions fully displace the rule of three in problems about compound interest, and they enter the explanation of the sequence of algebraic powers.

7 – The mid-15th-century "abbacus encyclopediae"

Around 1460, three extensive works of encyclopedic character were produced in the Florentine abbacus environment, already listed together in note 39:

– Benedetto da Firenze's *Praticha d'arismetricha* (existing in many copies, see [Van Egmond 1980: 356]), described in [Arrighi 2004/1965];[46]

[44] According to Palatino 573 [Arrighi 2004/1967: 191], Antonio appears to have made the same observation in his *Gran trattato*.

[45] The solution of this *fondaco* problem [Franci & Pancanti 1988: 80–82] runs almost exactly as Biagio's, but there is one telling difference. Biagio takes the wage of the first year to be two *things*, whereas the present author chooses 2 *censi.* He does not know, however, that this word translates Arabic *māl*, not only (namely in algebra) the square of the *thing* but also an amount of money (a capital, a dowry, etc.). In the end he therefore feels obliged to find the *thing* from the *censo* – only to square it again. This implies that the (direct or indirect) source cannot be Biagio's text; it must be traced to an ambience where the original meaning of the *censo/māl* was still alive.

[46] Inspection of the marginal computations in the manuscript Siena, Biblioteca Comunale degli

– Palatino 573, described in [Arrighi 2004/1967];
– Ottobon. lat. 3307, described in [Arrighi 2004/1968].

All of them contain material which was foreign to the abbacus tradition, and extensive extracts from the writings of (mostly) *named* abbacus predecessors (quite unusual in the abbacus environment). They are indeed our only source for Biagio's and Antonio's mathematics, but also contain long translated extracts from the *Liber abbaci.* They *may* possibly have had a model in Antonio's lost *Gran trattato* (see above).

Benedetto's work is divided into sixteen books. Three of these have to do specifically with proportions, and build for this on "foreign" material. Book II about "the nature and properties of numbers" (*la natura e propietà de' numeri*) is a presentation of speculative arithmetic in the Boethian tradition. It also offers an exposition and explanation of the way ratios were labelled in this tradition [ed. Arrighi 1967b: 324*f*]: multiple, submultiple, superparticular, superpartiens, sesquialtera, sesquitertia, etc. But it does not use the word *proportione* (nor *ragione*) but speaks of "a number which is referred to another number" (*numero che è riferito ad altro numero*). Book V is stated to deal with "the nature of numbers and proportional quantities" (*la nature de' numeri e quantità proportionali*) – see [Bartolozzi & Franci 1990: 12–14]. Its first part builds on the Campanus version of *Elements* V–IX and on Campanus's *De proportione et proportionalitate* on the composition of ratios; the second part concerns metrological conversions. The first part of Book XI presents *Elements* II (including the division in extreme and mean ratio); the second part is a translation of Book XV, Part I of the *Liber abbaci.*

How much does this general mathematical erudition influence what Benedetto did *within* abbacus mathematics? We may look at his own algebra, contained in Book XIII.

Firstly, it is said more clearly [ed. Salomone 1982: 20] than in the above-mentioned Florentine *Tratato sopra l'arte della arismetricha* that the algebraic powers (*termini dell'algebra*) are in geometric proportion; on the other hand, Benedetto does not *use* this observation for anything. The associative law for multiplication may have been too obvious for him, he sees no need to do so; alternatively, this should be said about Antonio, whom Benedetto seems to follow, cf. note 44.

Secondly, the word *proportione* turns up once [ed. Salomone 1982: 40] inside one of the many problems about numbers given in ratio, which however are all defined in terms of "such part".

Intronati, L.IV.21 shows that some of them (for instance the complicated problem with unknowns *quantità* and *borsa* mentioned in note 40) were written first and the main text in whatever space was left over on the page. This shows that this manuscript was Benedetto's working copy, and that the date given in the colophon (1463) is that of the manuscript. Fortunately, all partial editions of Benedetto's text are based on this manuscript.

Similar marginal computations in the other two manuscripts show that even they are their authors' working copies.

Palatino 573 was written by a former student of one Domenico d'Agostino *vaiaio* ("the tanner" or "the furrier" – whether this profession was indeed his or that of an ancestor is not to be decided). Its author says that he uses Benedetto's homonymous treatise, written "already some time ago" (for which reason the present manuscript should probably be dated around 1470[47]) as a model, adding and removing matters as needed [ed. Arrighi 2004/1967: 168]. It falls in eleven parts, subdivided in chapters. I shall discuss the relevant aspects on the basis of the extracts in [Arrighi 2004/1967: 168–194] (introductions to parts and chapters) and the description and quotations in [Bartolozzi & Franci 1988: 14–16].

Chapter II.8 [Arrighi 2004/1967: 176; Bartolozzi & Franci 1990: 15], about "the way to express as part, and, first, the definition", starts by quoting Boethius's, Euclid's and Jordanus's definitions of a ratio (*proportione*) as a relation between two numbers or quantities, and goes on with the traditional names (*doppia, sexquialtera,* etc.). This is not unproblematic, according to the definition a ratio is *not* a (possibly broken) number, as is the "part" he wishes to express. The author glosses over the difficulty by regarding it merely as a questions of language (thus reminiscent of certain discussions within contemporary historiography of mathematics): "we in the schools do not use such terms [*vocaboli*] but say instead […] that 8 is $^2/_3$ of 12 and 12 is $^3/_2$ of 8". The author also points to the necessity that the two magnitudes in a ratio be of the same kind, without noticing that this should create difficulties when, later, the concept is used to explain the rule of three. This is symptomatic of the whole project (as shared with Benedetto): abbacus mathematics is put into the framework of scholarly (in Fibonacci's word, "magisterial") mathematics, but the author reinterprets concepts as needed, and does not care much about the contradictions that may arise.

Part III [Arrighi 2004/1967: 176–178; Bartolozzi & Franci 1990: 15*f*] is similar in its aim. The introduction announces that its first chapter shall deal with "the 4 proportional quantities or numbers, in the vernacular called rule of three things". The chapter itself starts by defining the *proportionalità* as the equality of two ratios and explaining that such *proportionalità* may be continued or not continued, going on to present the product rule and using it to determine one term in a continued proportion from the two others. Chapter III.2 applies the rule to commercial examples, and ends by saying that

> it is true that many who want to shows this rule have said that one multiplies the quantity that one wants to know by the one which is not similar, and divides in the other quantity. And they actually say the truth. Because when you multiply a quantity by another one which is not similar, it is as multiplying the first by the fourth or the second by the third.

[47] Van Egmond [1980: 124] dates it on the basis of watermarks to c. 1460, but 1470 is still compatible with these. ⟦The compiler only refers to the predecessor as "B", but that is exactly the way Benedetto refers to himself in initial lines of the autograph.⟧

Nothing, as we see, is said about the impossibility to speak properly of ratios between dissimilar quantities; the author obviously thinks of nothing but the measuring numbers in the already established units of the statement of the problem.

Chapter III.3 translates the second part of Chapter XII of the *Liber abbaci* (discussed above).

Chapter V.3 [Arrighi 2004/1967: 181] introduces problems about numbers in given ratio by giving once more the names of ratios. This time, however, it identifies ratios with numbers in abbacus manner ("5 to 16 are $^5/_{16}$ because from 5 divided by 16 comes $^5/_{16}$").

Part IX [Arrighi 2004/1967: 190*f*; Bartolozzi & Franci 1990: 16] translates Chapter XV, Part 1 of the *Liber abbaci.* The introduction refers to objections against the relevance of this topic for algebra ("many strain themselves to prove…"). In defense of this relevance it cites Paolo dell'Abbaco's (otherwise unknown) *trattato delle quantità chontinue,* Antonio's *Gran trattato,* and the oral injunctions of his own teacher, the *vaiaio*. No argument beyond these appeals to authority is offered (nor could any probably be). The link to Boethius's presentation of the ten means once more goes unnoticed.

Part X [Arrighi 2004/1967: 191–194] is dedicated to algebra. The introduction states that in order to restore the almost lost *Maumetto arabicho* (that is, al-Khwārizmī), the presentation will be based on him.[48] Chapter 1 starts by quoting Fibonacci for the explanation that *algebra almuchabale* means "restoration and opposition, because the parts are opposed to each other, as you will see in the examples" – a misquotation, as we know, caused perhaps by al-Khwārizmī's text, perhaps (and rather) by earlier abbacus writers.[49] It then goes on with al-Khwārizmī's text in Gerard's translation.

After this pious homage to tradition in Humanist style the author feels the need to be modern, and starts by explaining the algebraic powers as a continued proportion, following (as he says) Antonio's *trattato* (probably the *Gran trattato* of which he has already spoken).

A large number of problems follow, in part taken from named predecessors.

Ottobon. lat. 3307 was written by another former student of the *vaiaio,* perhaps slightly before 1463 (at least there is no evidence whatsoever that its author knew about Benedetto's *Trattato*). It is divided similarly to Palatino 573. Beyond the chapter headings as quoted in [Arrighi 2004/1968] I used a photocopy from a microfilm of the final algebraic part, which already allows me to say (I shall omit the documentation) that this

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48 Similarly, Benedetto [ed. Salomone 1982: 20] presents not Fibonacci's but al-Khwārizmī's geometric proofs "because more ancient".

49 A third possibility is Guglielmo de Lunis's lost translation of (probably a revised version of) al-Khwārizmī's algebra, whose introduction (quoted for instance by Benedetto [ed. Salomone 1982: 1*f*]) has this interpretation of opposition; cf. also above, p. 419.

third encyclopedia is of a markedly lower quality than the other two, but not to derive much of relevance for the topic of proportions.

Maybe, however, there is not much of relevance at all: the algebraic powers (for which this author has no general term like Benedetto's *termini dell'algebra*) are *not* explained to be in continued proportion; further, the 33 folios of problems contain 2 questions (fols 336^{r}, 342^{v}) about 4 respectively 3 numbers in continued proportion, and none about numbers in given ratio.

We should be aware that these three encyclopediae all come from a specific strand in the abbacus tradition, centred upon a specific group of Florentine abbacus schools and tracing its origins to Antonio, Paolo dell'Abbaco and Biagio. Its interest in precursors – outside those belonging to the strand itself al-Khwārizmī and Fibonacci – was not shared generally. All the more telling is it that their copying of Fibonacci's sections on proportions did not really influence what they did themselves in abbacus style, apart from the formulation of the rule of three as a rule about four quantities in proportion. The interests and orientation of abbacus mathematics proper, we may perhaps conclude, left no space for that.

8 – Luca Pacioli

Even Luca Pacioli's *Summa de Arithmetica Geometria Proportioni et Proportionalita* [1494; 1523[50]], divided into nine *distinctiones,*[51] might be characterized as a kind of encyclopedia, *not* belonging to the strand discussed in the previous section. A symptom of this different affiliation is that Pacioli feels no need to give specific references for his borrowings – Pacioli instead has a general acknowledgment in the initial unfoliated *Sommario* that most of his volume has been taken from Euclid, Boethius, Fibonacci, Jordanus, Blasius of Parma, Sacrobosco and Prosdocimo de' Beldomandi. Actually, most of it probably comes from less prestigious, anonymous abbacus sources – whatever the citation strategy in the encyclopedias, it was always a strategy.

Pacioli's title might make us predict that ratios and proportions play a greater role and are more integrated than in the preceding manuscript sources. The list of his confessed sources could make us expect the same. Actually, "proportions and proportionalities" are mainly dealt with in the Sixth Distinction (at great length, fols 67^{v}–98^{v}), but they do serve elsewhere.[52]

50 In general, the second edition is faithful to the first word for word and line for line. There are a few corrections (and different misprints), but the main difference is that a number of abbreviations are expanded in the second edition.

51 I consider only the first, arithmetical part (1–224) and disregard the geometry (76 fols, 8 *distinctiones*).

52 I only got access to Fenny Smith's work on "Proportion in the *Summa de arithmetica, geometria,*

The topic is taken up for the first time in connection with the rule of three.[53] Initially, this rule is presented in terms of the similar and dissimilar, and in a different but equally "a-theoretical" way (fol. 57^r – ⟦cf. article I.5⟧).[54] After some examples, however, comes an explanation (fol. 57^v) *unde regula predicta procedat,* "where the said rule comes from", referring to *Elements* V. Actually, this explanation goes beyond what is needed for the purpose, as Pacioli points out, namely because he wants the reader to "better understand the fundaments" of the rule given. He starts by stating that the force of the rule of three "proceeds from the mutual proportionality of quantities, be they continuous or be they discrete, that is, be they numbers or be they measures, and be the proportionalities continued or not continued [*incontinua*]", with three respectively four terms. After pointing out that "in all the calculations of the trading and business world, 4 numbers always occur, of which 3 are always known and the fourth unknown" and repeating the need for three respectively four terms Pacioli makes a more interesting point: that in continued proportions all terms must be of the same nature because (except the extremes) all stand both as antecedent and as consequent, while the non-continued proportions require only pairwise identical nature. His explanation of these natures comes from the Aristotelian tradition – they are numbers, lines, surfaces and bodies. He then goes on with the product rule, with explanation of a schematic representation, and with examples of what to do if either of the four terms is unknown.

9 – The Sixth Distinction

The Sixth Distinction is described by Bartolozzi & Franci [1990: 17–27], for which reason I shall elaborate on such things only as go beyond their work. It is subdivided into six *tractati,* the first of which (fols 67^v–72^v) deals with "proportions", argued initially to be necessary not only by means of a list of glorious *names* – Euclid, the Stoics, the Platonists, the Peripatetics, Boethius, Jordanus, etc. – but also with reference to the prestigious uses of the concept: in Archimedes' *De mensura circuli,*[55] in law, in medicine (namely in composite drugs and the determination of diets), in mechanical

proportione et proportionalità of Luca Pacioli" [1998] after having finished the work. There is some overlap, but on the whole rather little, not least because our approaches to the text as well as our emphasis differ.

53 Continued proportions are mentioned but not really treated on fol. 37^v, during the treatment of progressions.

54 The second edition [1523] has this folio number misprinted as 64.

55 It may be taken note of that Pacioli follows the abbacus tradition and not the Archimedean treatise in the value of these ratios; abbacus geometry had always taken the quotient between perimeter and diameter to be exactly $^{22}/_{7}$ (not 22:7, since it was not interested in ratios), and that between the areas and the circumscript square to be $^{11}/_{14}$. Pacioli's reading of Archimedes's treatise must (at best) have been superficial.

artifices, in the painter's mixing of colours and in the canonical proportions of the human body,[56] in rhetoric, in architecture, in carpentry, in music etc. Only afterwards (fol. 69^r) come the "definitions of the various proportions", said to be preceded in Euclid by the definition of parts "as we did in the definition of fractions" (namely fol. 48^r). A ratio can, with Plato and Boethius, be determined for any two magnitudes of the same kind – but not, as "in a certain abuse of common speech", between the sharpness of a voice and that of a knife.

After this very general "definition" comes the subdivision into geometrical, arithmetical, and harmonic proportion, the first of which is supposed at this point to be applicable to continuous quantities only, the second to discrete as well as continuous quantities,[57] the third to sound and song. As can also be seen from the examples, Pacioli primarily links the three types to the disciplines from which they have their name, even though he does explain later on that the arithmetical proportion has to do with "excesses or differences" and says on fol. 75^v that this linking is what "certain blunt minds" (*alcuni roçi*) think. The fact that a harmonic proportion has to involve three terms leads to a digression (provided one can distinguish digressions from the rest in Pacioli's almost Borgesian prose) about the sloppy habit to say *proportione* where the precise word would be *proportionalità,* and about the subdivision of such proportions into continued and discontinued.[58]

The presumed observation that only geometrical proportions can be between rational as well as irrational quantities (*viz* because arithmetic *as a discipline* considers only rational magnitudes) leads to a discussion of commensurability and incommensurability, with a reference to *Elements* X. Since Pacioli's example is the diagonal in a square with side 10, this is superfluous sophistication, and in fact he goes on with an (unreferenced) borrowing from the scholastic theory of ratios, namely when speaking of the ratio diameter:side as *meççadoppia,* "half of double", explaining (with reference to *Elements* VII–VIII) that this ratio duplicated is the double ratio.

Follows a long presentation (fols 71^r–72^v) of the Boethian subdivisions of the category of "rational proportions" – now obviously only geometrical, but that goes unmentioned: equal, major, minor, multiplex, simple and multiplex superparticular and superpartient, submultiplex, etc. In the end comes the wonderful admission that "these terms which serve to denominate these many kinds of proportions serve (for you, practitioner [*a te pratico*])

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56 In this connection there is a laudatory reference to *maestro Pietro de li Franceschi nostro conterraneo del Borgo san Sepolchro* and his *De prospectiva pingendi*.

57 Pacioli even gives an argument for this claim: discrete quantities can only enter in rational ratios, continuous indifferently in rational and irrational ratios.

Everywhere else Pacioli evidently takes "geometric proportions", that is, ratios, between numbers. *Interdum dormit Homerus* – and lesser spirits too.

58 Pacioli's term has changed since fol. 57^v, now he uses *discontinua* instead on *incontinua.*

no other purpose than speaking solemnly [*proferire*] about the species you have found". Fol. 82^{r} presents the Boethian categories in a scheme.

The second treatise (fols 72^{v}–76^{r}) takes up *proportionalità* for good, defining these (with *Elements* V) as similitude of ratios.[59] Once more we get the geometrical, arithmetical and harmonic proportions, this time with numerical examples for the former two,[60] the harmonic proportion being left explicitly aside. Those still considered may be continued or discontinued; the need for similarity of kind is repeated. For continued proportions of both remaining types, the necessity of equal kind for all members is pointed out.

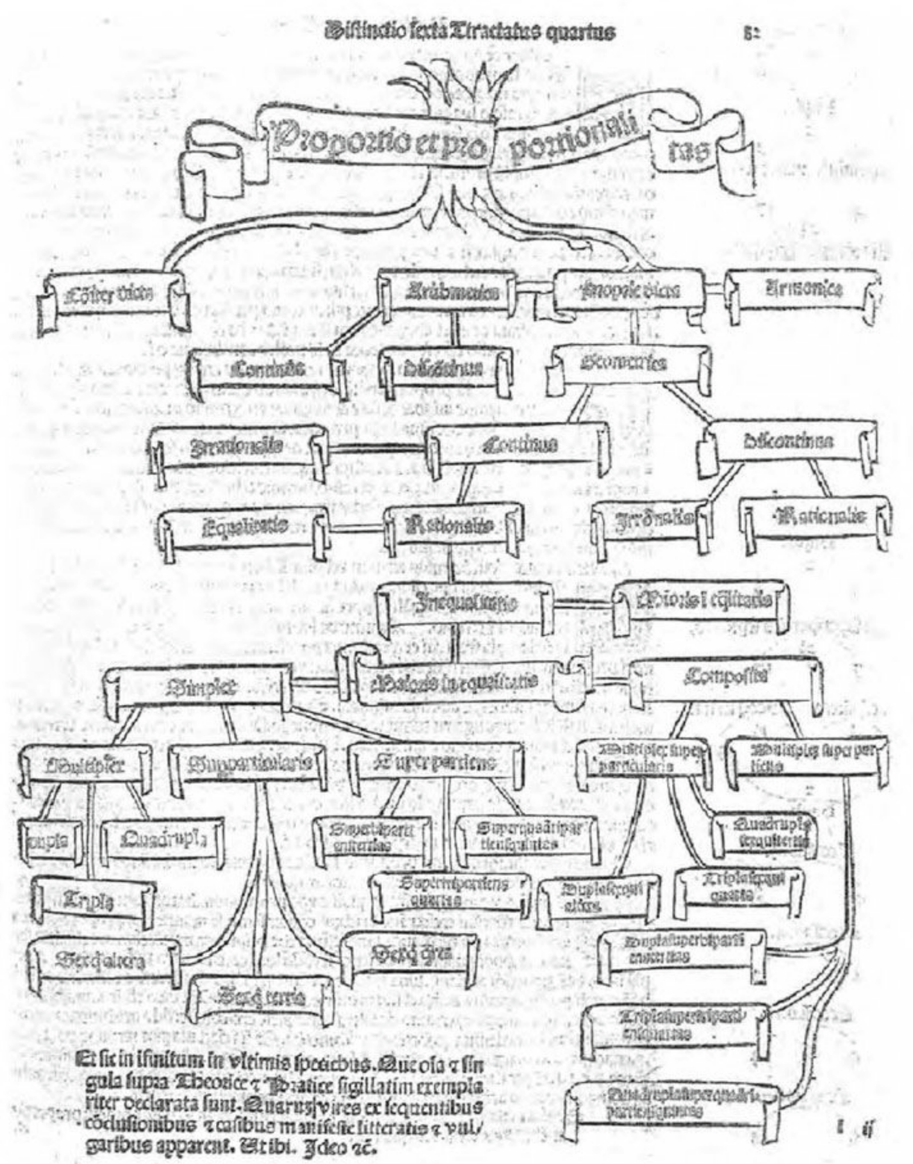
After a discussion of *disproportionality* (fol. 74^{r}) – the first "proportion" being either larger or smaller than the second – Pacioli deals with the six ways to come to grips with proportions (*de sex specibus sive modis arguendi proportionalitatum*), repeating Campanus's seven modes (see 412) and coming down to six by conflating *e contrario* with *e conversa*. Pacioli says afterwards (fol. 75^{v}) that he now deals with geometric proportions only, at which point he also asks how much of it holds for arithmetical proportions. He shows the *permutatim* mode to be valid, and then generalizes that it should hold for all – obviously without calculating, since only the *ex aequa* mode is true.

The third treatise (fols 76^{r}–80^{r}) begins by explaining the *denominations* of ratios, (not to be mixed up with the Boethian *names*), the number resulting from the division of one term by the other, in agreement with the terminology of Jordanus and Campanus in their treatises about proportions [ed. Busard 1971: 205, 213; Busard 2005: 230]. After the dismissive remark about the utility of the Boethian terminology in the first treatise (followed up here with references to *phylosophi*), Pacioli thus chooses not to do as Palatino 573, which identified ratios with numbers (or replaces them with numbers); what he does is equivalent, but in the dress of established theory. The cost (which the loquacious Pacioli may not have seen as a cost) is that what Palatino 573 does in a couple of lines now needs two dense pages (fols 76^{r}–77^{r}) to be explained.

Procured with the denomination concept and in agreement with the Campanus *Elements* VII [ed. Busard 2005: 230], Pacioli can return (fol. 77^{r}–77^{v}) to the question of whether one ratio (between numbers, which he does not say) is equal to, greater than or smaller than another one. He uses the occasion to show how this can be done also for the Boethian names, translating them into denominating numbers. He can also take up the composition of ratios (fols 77^{v}–78^{r}), "without comparison much more difficult" than the operations

[59] Bartolozzi & Franci [1990: 19] reproach Pacioli that this similitude is meaningless without its definition via equimultiples, forgetting that this is not only absent from Pacioli's *Summa* but also from the Campanus *Elements*.

[60] For geometric proportionality, the example is that 6 is to 3 as 4 to 2 – no problem with numbers, cf. note 56 and the preceding text.



Pacioli's schematic presentation of the Boethian categories [1494: 82r]

on integers, fractions and roots, and (once more) necessary for instance for the physician in his preparation of composite drugs. First he deals with continued proportions (fol. 78^r), where we see that Pacioli spontaneously tends to forget the distinction between the ratio and its denomination: in order to find the ratio between the first and the third term “it is sufficient to multiply [that between the first and the second term] by itself, or its denomination by itself, and it will make the denomination of the proportion between the first and the third”. The same tendency underlies an explanatory observation on Campanus, “by duplicated [ratio] Campanus understands (as true is) multiplied by itself” and in the corresponding reference to the “multiplication of the double [ratio] by itself” and in the general claim that “as multiplying a proportion by itself makes a third proportion, thus to multiply the denomination of the said proportion by itself will make the denomination of that third proportion”. The composition of unequal ratios comes briefly on fol. 79^v. Now Pacioli is more faithful to his theoretical base, speaks of “joining” the ratios 2:1, 6:2 and 24:6; the way is of course to multiply the denominations 2, 3 and 4.

Next follows (fols 79^v–80^r) the problem, how to divide a given ratio in several ratios from which it is composed.[61] It is correctly said, and demonstrated by examples, that this can be done in many ways, by insertion of intermediate terms ad libitum.[62] Finally (fol. 80^r) Pacioli teaches how to determine one term of a ratio if the denomination and the other term is known (or the other chosen freely, if none is fixed).

The fourth treatise (fols 80^r–81^v) is an attempted *Algorismus proportionum,* based once again on Witelo. It teaches how to add (that is, compose) and subtract ratios and how to “multiply” and “divide” ratios. “Multiplication”, however, is simply composition of several not necessarily equal ratios, and “division” is the splitting of a ratio into several not necessarily equal ratios (the examples for both use unequal ratios). Oresme’s work is clearly no inspiration.

The fifth treatise (fols 832^v–84^r) examines what happens to ratios and arithmetical proportions if they are changed in various ways (examples in the margin combine denominations and Boethian names).[63] Namely that

– a ratio grows if the major term is augmented (all ratios are supposed to have the major term first) or the minor diminished;

[61] The title asks for several equal ratios, but that does not correspond to what actually follows. In [1523: 79^v] the word “equal” has justly been removed – the publisher Paganinus de Paganino may have had the assistance of somebody who understood the matter, or based himself on a copy with corrections inserted. The latter seems plausible; in the first line of fol. 81^v, an erroneous $^2/_3$ is not corrected into $1^2/_3$.

[62] Pacioli refers for this to Witelo’s *Perspectiva,* which he has already said on fol. 79^r to have consulted years ago.

[63] [Bartolozzi & Franci 1990: 23] offers a translation into modern mathematical symbols.

– the same happens if to both terms something is added, to the major something larger than itself, to the minor something smaller than itself;
– if between the extreme terms of a ratio one or several others are inserted, then any ratio between any two intermediate terms or one extreme and an intermediate term is smaller than the original ratio;
– the increase of both major terms or both minor terms or the decrease or increase of all four terms in an arithmetical proportion by the same amount conserves the proportionality;
– a ratio does not change if both terms increase *geometrice* – further explained as increase by the same part;
– a ratio diminishes if to both terms the same absolute (*arithmetice*) amount is added, and it increases if the same absolute amount is subtracted from both;
– if both terms of a ratio are increased geometrically, then their "arithmetical proportion" (that is, their difference) increases;
– if both are diminished geometrically, then their "arithmetical proportion" decreases;
– if both terms of a ratio are equal, increasing or decreasing both arithmetically equally is the same as increasing them geometrically equally, and their ratio is conserved.

Three corollaries follow which are related to the Peripatetic theory of motion.

According to its title, the sixth treatise (fols 84^r–98^v) deals with the "seven marvels [*mirabiles*] from the proportions between two quantities". Actually, it *begins* with seven "marvels" involving two quantities and then considers others which concern three or more. The first marvel is that

> any two quantities you want in any proportion joined together, and then the sum divided by each of the said quantities; the results then joined together, and then the sum of the said results equally divided by the each of the said results; and again these latter two results joined together, will always be the sum of the first two results, and it never fails.

In symbols,[64]

$$\frac{\frac{a+b}{a}+\frac{a+b}{b}}{\frac{a+b}{a}}+\frac{\frac{a+b}{a}+\frac{a+b}{b}}{\frac{a+b}{b}}=\frac{a+b}{a}+\frac{a+b}{b}$$

I shall not go through all seven marvels (all are rendered in symbols in [Bartolozzi & Franci 1990: 23–24][65]), but two are noteworthy – in symbols, respectively[66]

[64] The fraction lines stand for the operation which Pacioli speaks of as "division"; *denom* in (5) stands for "denomination of" the ensuing "proportion".

[65] There is a (mathematical as well as translational) error in the fourth, which should be

(4) $$\frac{\frac{a+b}{a}+\frac{a+b}{b}}{\frac{a+b}{a}}=\frac{a+b}{b} \text{ and } \frac{\frac{a+b}{a}+\frac{a+b}{b}}{\frac{a+b}{b}}=\frac{a+b}{a}$$

$$\frac{a+b}{a} \times \frac{a+b}{b} = \frac{a+b}{a} + \frac{a+b}{b}$$

and

$$\frac{a+b}{a} + \frac{a+b}{b} = 2 + denom(a:b) + denom(b:a)$$

The marvels seem to have to do to with the connection between problems about the splitting of 10 into two parts a and b, where ${}^{a}/_{b}+{}^{b}/_{a}$ respectively ${}^{10}/_{a}+{}^{10}/_{b}$ is given. Such problems are known since the beginning of the algebra tradition,[67] and they had also been taken up by Jordanus in *De numeris datis*.[68] Even though I do not remember having seen Pacioli’s rules in earlier sources, I therefore suspect him to have borrowed at least some of them.

After the seven, as told, others marvels follow (fol. 85^{r-v}) regarding three, four or five numbers in continued proportions, first of which is that if three numbers are in continued proportion, then division of their sum by the single numbers produces another continued proportion. This was (for an arbitrary dividend) what Antonio considered as “rather clear and obvious” (text after note 41) and in fact a theorem which is useful for certain of the problems about the splitting of a number into a sum of numbers in continuous proportion. We may take it for granted that Pacioli took it from the tradition – perhaps indeed directly or indirectly from Antonio, since he goes on (fols 85^{v}–86^{r}) to apply the rules to binomials in the way Antonio had done in his “Mirabile dictum” (above, note 43 and surrounding text). As pointed out by Bartolozzi & Franci [1990: 24], Pacioli generalizes Antonio’s method further than Antonio himself had done (asking only for a rational ratio *b:a*) without controlling – and errs (or so it seems – the text is not fully clear as to how many conditions Pacioli wants to fulfil).

Next (fols 86^{v}–87^{v}) come a number of rules about three, four or more numbers in continued or (occasionally) non-continued proportion. Most, as Pacioli states, follow from *Elements* VI.15–16 and VII.20 (our VI.16–17 and VII.19 – the product rule for three or

The authors have overlooked that the equality between the first and second results are said to be *econverso*. As Pacioli points out, the first marvel follows from this, as do the second versions of (3) and (5), not given by Bartolozzi & Franci, in which the right-hand sides of (4) are replaced by the left-hand sides.

66 In both cases Pacioli also points out that the rules hold for the “second results” as well.

67 See [Rosen 1831: 44–46; Rashed 2007: 167–165] (al-Khwārizmī), [Levey 1966: 94–102, cf. 132–140; Sesiano 1993: 365–369, cf. 382–388; Chalhoub 2004: 58–65, 103–109] (Abū Kāmil), and [Woepcke 1853: 91*f*] (al-Karajī). All three give general rules for the behaviour of the quotients, e.g., ${}^{a}/_{b} \cdot {}^{b}/_{a} = 1$ and $({}^{a}/_{b} + {}^{b}/_{a}) \cdot ab = a^2 + b^2$.

68 I.20 and I.21α, [ed. Hughes 1981: 64].

four segments or numbers in proportion/continued proportion): how, if two (or, an overdetermined case, three) neighbouring quantities in a continued proportion are known, to find the remaining one(s).

Slightly more intricate are the cases where the first and the last of four or five quantities in continued proportion are known. In the case of four quantities, this coincides mathematically with Jacopo's second *fondaco* problem, but whereas Jacopo merely prescribes the extraction of the cube root of the ratio between the fourth and the first quantity without explaining why, Pacioli uses algebra, without which he finds it difficult to solve the problem. In the case of five quantities, the middle quantity is found first from the product rule.[69]

Between these two cases, Pacioli gives the abstract analogue of Jacopo's third *fondaco* problem. Without explanation Pacioli gives the same rule as Jacopo (see text after note 32); he certainly does not know how it comes about (if so, the algebraic solution of the preceding problem shows that he would have explained). However, the last step of his procedure (how to find two numbers from their sum and their product) suggest that Jacopo is not his direct or indirect source: it contains a hint of an underlying geometric procedure (a reference to operation with two *different* halves of a quantity) which is absent from Jacopo's text, and which neither Pacioli not any internediate abbacus writer is likely to have introduced on his own.[70]

Pacioli now (fol. 88ʳ) supplies a number of "keys", likened (nothing less!) to the two spiritual keys of gold and silver by which "in our Catholic Militant Church the first shepherd Saint Peter" opens and closes the doors of Paradise and Hell for us.

The keys – fifteen in total – are theorems (not labelled so by Pacioli), in part near- or full repetitions of what he has already explained before or easy corollaries of familiar stuff, in part new to the book and not easily guessed without symbolic manipulation. All are illustrated by numerical examples. I list them in symbolic translation, indicating the beginning of new pages:

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69 In generic terms, Pacioli says that the same method can be used for "6, 7, 8, etc." terms, but he abstains (maybe wisely) from implementing this insight – fol 182ʳ he speaks of the sixth root as the "cube root of the cube root" and of the seventh root as the "root of the root of the cube root". Possibly, these composite expressions indicate that Pacioli believed they could be found by stepwise calculation. This is not quite certain, however: as we have seen above, Fibonacci speaks in the *Liber abbaci* [ed. Boncompagni 1857: 400] of the quintupled proportion as "cube of the square, or square of the cube" and of the sextupled ratio as the "cube of the cube", but his numerical examples (32:1 as the quintuple of 2:1, 729:1 as the sextuple of 3:1) show he was not misled.

70 When solving in the second part of the *Summa* the corresponding geometric problem, Pacioli [1494: II, fol. 18ʳ] merely refers to the contents of *Elements* II.5, as does his ultimate source (Fibonacci's *Pratica geometrie* [ed. Boncompagni 1862: 63] – ⟦Fibonacci, however, only quotes the words of the theorem and does not mention Euclid by name⟧). Similarly also (with explicit citation of Euclid's proposition) in the arithmetical part, fol. 93ᵛ.

(1)(88r) If $a{:}b{:}c{:}d$, then $\frac{b+c}{a+b+c+d} : \frac{b}{a+c}$.

(2) If $a{:}b{:}c{:}d$, then $\frac{a+b}{c+d} : \frac{a}{c}$.

(3) If $a{:}b{:}c{:}d$, then $\frac{a+c}{b+d} : \frac{a}{b}$.

(4) If $a{:}b{:}c{:}d$ and $S=a+b+c+d$, then $^S/_a{:}\,^S/_b{:}\,^S/_c{:}\,^S/_d$; with three members, this was the first three-number "marvel" on fol. 85^r.

(5) If $\frac{a}{b} : \frac{c}{d}$ then $ad = bc$; the product rule, amply used before.

(6)(88v) If $\frac{a}{b} : \frac{c}{d}$ and if $c^2+d^2 = a{\cdot}b$, then $\sqrt{(a^2 \;\; + b^2)(cd)}$ has the same value. Actually, given only the proportion, $(a^2+b^2){\cdot}cd = ab{\cdot}(c^2+d^2)$.

(7) If $\frac{a}{b} : \frac{c}{d}$, then $([a{\cdot}b]{\cdot}c){\cdot}d=(a{\cdot}d){\cdot}(b{\cdot}c)$; evidently, this does not depend on the proportionality.

(8) If $a{:}b{:}c{:}d$, then $(a+b+c+d)^2=a{\cdot}(b+c+d)+b{\cdot}(a+c+d)+d{\cdot}(a+b+c)+c{\cdot}(a+b+d)+a^2+b^2+c^2+d^2$; this time, Pacioli himself points out that the rule does not depend on the proportionality.

(9) If $a{:}b{:}c$, then $(a{\cdot}b){\cdot}c = b^3$.

(10) If $a{:}b{:}c$, and if, for some quantity Q, $^Q/_a+{}^Q/_b+{}^Q/_c=a+b+c$, then $b = \sqrt{Q}$.

(11)(89r) If $a{:}b{:}c$, then $^{(a\cdot b)\cdot c}/_a=b{\cdot}c$, $^{(a\cdot b)\cdot c}/_b=a{\cdot}c$, $^{(a\cdot b)\cdot c}/_c=a{\cdot}b$, and $^{(a\cdot b)\cdot c}/_{a\cdot b}=c$, $^{(a\cdot b)\cdot c}/_{a\cdot c}=b$, $^{(a\cdot b)\cdot c}/_{b\cdot c}=a$; Pacioli points out that this does not depend on the proportionality.

(12) If $a{:}b{:}c$ and further $\frac{a}{b} : \frac{p}{q}$, then $p{\cdot}(b+c) = q{\cdot}(a+b)$.

(13) If $a{:}b{:}c$, then $2\cdot(a{\cdot}c+b{\cdot}[a+c])=a(b+c)+b(a+c)+c(a+b)$. With references to *Elements* II.2 and the formulations "in other words" in *Elements* VI and IX Pacioli points out that this does not depend on the proportionality.

(14)(89v) If $a{:}b{:}c$, then $^{a(b+c)+b(a+c)+c(a+b)}/_{2\cdot(a+b+c)}=b$.

(15) If $a{:}b{:}c$, then $\frac{a^2}{b^2} : \frac{a}{c}$.

(15) is the last "key". Under the heading "to find mean proportionals between two quantities", two sophisticated counterfactual calculations follow (fol. 89^v) which I guess are Pacioli's own invention: If 2 is the arithmetical respectively geometric mean between 5 and 11, what is then the corresponding mean between 7 and 13? In both cases, the true means between 5 and 11 and 7 and 13 are found (8 and 10 respectively $\sqrt{55}$ and $\sqrt{91}$), and the rule of three is applied. In the arithmetical case, a proof is performed, consisting in corresponding proportional change of the limits, after which the true means between these limits are shown to coincide with what was found before; in the geometrical case, a similar proof is sketched but not performed.

The “second case” under the same heading is a traditional question “Three is (too) little and 4 is (too) much”. The “just or due” amount is said to be $\sqrt{12}$, the geometric mean; this – not the arithmetical mean – is then stated to be what is used in all commercial matters (*in omnibus mercantiis*). Primarily, this probably extrapolates from the observation that the rule of three is based on geometric *proportionality.* But Pacioli may also think of the use of the geometric mean in certain mathematical *problems* in commercial disguise.

In any case, such a problem, about three pearls, follows as the “third case”. The first pearl weighs 1 carat and is worth 200 *ducati,* the second weighs 2 carats and is worth 1000 *ducati,* the third weighs 3 carats. What is its just price?

Pacioli posits a fourth pearl with weight 4 carats. To the weights 1:2:4 in continued proportion must correspond prices in continued proportion, i.e., 200:1000:5000. Therefore the price of the 4-carat pearl must be 5000 *ducati.* 3 carats being the (arithmetical) mean between 2 and 4, the price of the 3-carat pearl must be $\sqrt{(1000 \cdot 5000)}$.

A fourth case is also about justice. The Holy Father, Innocent VIII, orders that 10000 *ducati* be distributed justly between the citizens of Perugia for service rendered. This gives rise to a long discourse (more than 500 words) about Aristotle’s two kinds of justice from the [Nicomachean] *Ethics* V.2–5 [Barnes 1984: II, 1784–1789]: “commutative”[71], applicable to commercial exchange, and distributive. Both, according to Pacioli, “can, broadly speaking, be understood in two ways, geometrically and arithmetically, though, strictly and properly speaking, the maximal distributive sort can only be geometrical”.[72] After the digression into ethical theory it is then explained that the money is justly distributed if made in geometric proportion to the “quality” (*bontà*) of each.

The sixth distinction ends (fols 90^{v}–98^{r}) with 35 problems[73] and an epilogue (fol. 98^{r-v}). The final two have nothing to do with proportions – #34 is “Bachet’s weight problem”, and #35 belongs to the same family; parallels in the wording suggest that they are borrowed from the *Liber abbaci* [ed. Boncompagni 1857: 297*f*]. In all the others, “proportions” play a role.

First come 23 problems about three numbers in continued proportion. In seven of them, a number (19, 19, 14, 10, “a number”,[74] 10, 10) is split into such constituents; towards the end of the sequence, four are dressed as dealing with economic life.[75] In

71 Nowadays normally translated “rectificatory”, but Pacioli follows his fellow friar Thomas Aquinas (*Summa theologiae* 1^{a} q. 21 a. 1 s 1 co, see [*Corpus thomisticum*]), whom he cites.

72 This point comes from Aristotle, whose Chapter 3 also contains as discourse on proportion theory. Mathematical proportions (represented by lines and letters) are used further in Chapters 4 and 5.

73 Pacioli also counts until 35, but has two #18, skips #19 and #28 and has two #29.

74 This problem (#15) is indeterminate. Afterwards, the number is chosen to be 10, whereby it is made determinate.

75 #18^{bis} deals with a gambler’s gains, where the product rule is explained once again, suggesting

#1–6, specified "keys" are used as a first step in the procedure, which in these and the other cases often makes use of algebra or (in #5, #6 and #18) of *Elements* II.[76]

Next follows a sequence of ten problems about four magnitudes in continued proportion, none of them in concrete dress. Once again, the first ones make use of specified "keys" (#24–27 – but also #31–32). Most interesting are probably #31–33: #31 and #33 are pure-number versions of Jacopo's third and fourth *fondaco* problems, #32 of a similar problem where the sums of the wages for the first two and for the last two years are given. In #31, key (1) is used to reduce the problem; then the second number is taken as the *thing* and found by second-degree algebra to be $12^1/_2+\sqrt{7^{37}/_{84}}$ – at which point Pacioli cautiously leaves it to the reader to continue.[77] Since his present method does not lead easily[78] to the formula used by Jacopo and by Pacioli in the first presentation of the abstract problem just before the "keys" (above, text before note 70), Pacioli appears not to have noticed the connection.

The use of the "keys" in problem reductions leaves little doubt that these *new* theorems about the behaviour of proportions were created as tools for the solution of problems – but apparently only problems formulated in terms of proportions or proportionality, for whose initial reduction they served. Pacioli's way to add observations about (8), (11) and (13) strongly suggests that the basic set was not his own. It is likely to have been created

perhaps the text to be borrowed (but Pacioli is too fond of repeating to make the inference certain); #21, which "was proposed to me in Florence in 1480, the 22nd of June", deals with a purchase of saffron, cinnamon and mastic, and #22–23 with alloys.

[76] Algebra is thus *used* by Pacioli well before he presents it systematically. Often, this algebra is quite complex. In #4, for instance, Pacioli has to operate with two unknowns in the same way as Antonio, that is, with "a *thing* less a quantity" and "a *thing* plus a quantity". The problem in which this is used is not the same as the one where Antonio introduces it in the *Fioretti,* nor with the one from the *Flos* where Fibonacci employs it, cf. note 40.

[77] The solution is correct, but corresponds to a decreasing sequence, which is certainly not what Pacioli intended; in order to have an increasing sequence, he should have chosen the other root of the equation, $12^1/_2+\sqrt{7^{37}/_{84}}$. Since Pacioli did not discover, he cannot have finished the calculations.

When applying later the same method to an analogous wage problem with rational solutions, Pacioli makes the complete calculation and chooses the correct solution – see presently.

[78] Of course it *can* lead to it, but only if one is able to express the double root (the second and the third number, respectively) as

$$\frac{P}{2}\pm\sqrt{\frac{P^2}{4}-\frac{P^3}{3P+Q}}$$

(P being the sum of the second and the third number, Q that of the first and the fourth). The product of these is indeed

$$\frac{P^3}{3P+Q}$$

as required; but this will have been far too complicated for Pacioli.

during the fifteenth century and *seems to reflect a more intimate integration between algebra and proportions than other sources would make us expect*.

10 – Further "proportions" in Pacioli's *Summa*

Proportionality turns up in (at least) three other contexts in the *Summa* – in the general presentation of algebra, and in two sets of problems.

Fol. 143^{r} lists a sequence of 30 algebraic powers (*dignità*) in two different terminologies and observes that the reader may go on *proceeding proportionally* "as long as you want". On fol. 145^{v}, the same insight (which as we know was not new) is hinted at in the statement that all solvable cases are *proportionati* to the six basic cases.[79] It becomes more explicit (and somewhat more innovative) on fols 149^{v}–150^{r}, after a short list of select possible and impossible cases. In order to find out to which basic case a given equation reduces, one shall locate the *dignità* in the ordered sequence and reduce[80] geometrically equally to the lowest possible degree by counting downwards. However, if the *intervalli* between the three powers in a three-term equation (the only equations Pacioli considers) are not equal, it has "so far not been possible to form general rules because of their disproportionality".

On fols 186^{r}–187^{v}, a number of problems deal with gain (occasionally loss) "at the same rate" in two or more travels. Mostly, the proportionality leads to the application of the rule of three, but once, in an alternative ("and more beautiful"/*pulchrius*) solution (fol. 186^{r}), proportionality is mentioned explicitly, and the product rule applied. This evidently gives the same calculations as the rule of three; the aesthetic advantage is solely in the use of "magisterial" terminology – "speaking solemnly", in Pacioli's earlier words.

Finally, fol. 194^{r} brings six problems about the wages of a servant, in two of which the wage is supposed to increase "at the same rate" each year (four years in total). In the first of them the wage of the first year is 10, that of the last year is 60; apart from a change of the wage of the first year, this coincides with Jacopo's second *fondaco* problem. This has to be done "according to what I showed you in the proportions, and I shall say no more, except that there are four proportional numbers, and the first is 10, and the last is 60. I ask for the means" – which are then stated to be $\sqrt[3]{6000}$ and $\sqrt[3]{36000}$.

The second coincides with Jacopo's third *fondaco* problem, even in its choice of parameters. For the solution, Pacioli refers to the "first key" and to what he has already taught. This time, the numbers are convenient, and Pacioli makes the complete calculation,

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79 This, certainly, is not true *stricto sensu* if we consider as solvable, e.g., the case "*cubes* equal to number"; but Pacioli's target is the proliferation of false solutions to non-homogeneous higher–degree equations.

80 The verb is *schizzare,* which mostly refers to the reduction of a fraction through division of numerator and denominator by the same divisor.

finding the second number to be 30–√100 and the third to be √100+30 (an order which suggest he has not used the double solution but subtracted from 60).

There may be other scattered references to the concept of proportionality in the work. All in all, however, “proportions and proportionalities” are mainly treated in the sixth distinction, which is indeed extensive and profound enough to justify the appearance of the terms in the title of the work; to this distinction are also moved traditional abbacus problem types about numbers in proportion, abstract as well as in commercial dress. Outside the sixth distinction, “proportions” play as modest a role as in most abbacus treatises.

11 – Summing up

In this way, Pacioli’s *opus magnum* suggests the general summary we may draw up. Abbacus mathematics, based in practical arithmetic, was always centred around problems of simple (direct or inverse) proportionality; to this came a strand of algebraic thought with hight prestige due both to its efficiency and to its character of “theoretical level of practical arithmetic”.[81] Initially, neither the language nor the theory of proportions had anything to do in either; gradually, but hardly to a larger extent than its penetration in daily discourse, would the proportion language pop up. Problems might also be formulated in terms of quantities or numbers in proportion. *Theory* beyond the product rule remained outside.

To this, only writers with “magisterial” pretensions – Fibonacci, Antonio, Benedetto, the author of Palatino 573, Pacioli – and the shadowy inventor of Pacioli’s “keys” constitute exceptions. Apart from the inventor of the keys, who to some extent made *new* theory, what they offer in terms of theory and technicalities beyond the product rules are isolated chapters, in some cases just copied from Fibonacci (and never understanding more about these than Fibonacci himself). They are there rather by pious duty than by mathematical necessity.

As regards the Boethian terminology for ratios, the situation is even more blatant. Since late Carolingian times, this categorization had been the almost sacred core of the mathematics of Latin schools and universities. Benedetto and Palatino 573 introduce them,

81 Two quotations may suffice to illustrate this prestige. Palatino 573 [ed. Arrighi 2004/1967: 191] opens the part on algebra (as we remember, this encyclopedia falls in eleven “parts”) with the words “every part would be in vain if this [part] was left out; because […] this is the one that gives solution to all cases”. Pacioli [1494: 144^r] observes in the corresponding place that we have now “arrived with the help of God to the much desired place: that is, to the mother of all the cases popularly called the *regola della cosa* or *Arte magiore,* that is, *pratica speculativa,* otherwise called *algebra & almucabala* in the Arabic or Chaldean tongue”. The words *pratica speculativa* (used again about algebra in Pacioli’s *Divina proportione* I, cap. iv [1509: 3^v]) mean exactly “theoretical [level of] practical arithmetic”.

but the latter dismisses them immediately, and the former makes no use of them. Only Pacioli employs this "solemn speech" rather consistently when explaining the composition of ratios in Distinction 6, 4th Treatise. The general tendency is to speak "at best" (as judged from the perspective of the schools) of the denominations of ratios, "at worst" to come close to identifying the ratio with the quotient (as when Pacioli identifies composition and multiplication of ratios).

All in all, abbacus mathematics is much more modern on this account (and on several others) than scholarly mathematics of its epoch. As scholars eventually digested the abbacus heritage, they took over norms, not only technical algebraic knowledge.

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	Pappos	Nicomachos	*Liber abbaci*
$\frac{R-Q}{Q-P} : \frac{R}{R}$ (arithmet.)	P1	N1	
$\frac{R-Q}{Q-P} : \frac{R}{Q}$ or $\frac{R-Q}{Q-P} : \frac{Q}{P}$	P2	N2	27–29
$\frac{R-Q}{Q-P} : \frac{R}{P}$	P3	N3	7–9
$\frac{R-Q}{Q-P} : \frac{P}{R}$	P4	N4 (but inverted)	10–12 (inverted)
$\frac{R-Q}{Q-P} : \frac{P}{Q}$	P5	N5 (but inverted)	34–36 (inverted)
$\frac{R-Q}{Q-P} : \frac{Q}{R}$	P6	N6 (but inverted)	20–22 (inverted)
$\frac{R-P}{Q-P} : \frac{R}{P}$	absent	N7	16–18
$\frac{R-P}{R-Q} : \frac{R}{P}$	P9	N8	13–15
$\frac{R-P}{Q-P} : \frac{Q}{P}$	P10	N9	30–32
$\frac{R-P}{R-Q} : \frac{Q}{P}$	P7	N10	37–38
$\frac{R-P}{R-Q} : \frac{R}{Q}$	P8	absent	23–25
$\frac{R}{Q} : \frac{R-P}{Q-P}$	absent	absent	26

Means dealt with by Pappos and Nicomachos and in the *Liber abbaci*

Chapter 17 (Article I.16) Archimedes – Knowledge and Lore from Latin Antiquity to the Outgoing European Renaissance

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

J. Høyrup, *Selected Essays on Pre- and Early Modern Mathematical Practice*,
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Abstract

With Apuleius and Augustine as the only partial exceptions, Latin Antiquity did not know Archimedes as a mathematician but only as an ingenious engineer and astronomer, serving his city and killed by fatal distraction when in the end the city was taken by ruse. The Latin Middle Ages forgot even much of that, and when Archimedean mathematics was translated in the 12th and 13th centuries, almost no integration with the traditional image of the person took place.

Petrarca knew the civically useful engineer and the astrologer (!); no other 14th-century Humanist seems to know about Archimedes in any role. In the 15th century, however, "higher artisans" with Humanist connections or education took interest in Archimedes the technician and started identifying with him. In mid-century, a new translation of most works from the Greek was made by Jacopo Cremonensis, and Regiomontanus and a few other mathematicians began resurrecting the image of the geometer, yet without emulating him in their own work.

Giorgio Valla's posthumous *De expetendis et fugiendis rebus* from 1501 marks a watershed. Valla drew knowledge of the person as well as his works from Proclus and Pappus, thus integrating person and works. Over the century, a number of editions also appeared, the *editio princeps* in 1544, and mathematical work following in the footsteps of Archimedes was made by Maurolico, Commandino and others.

The Northern Renaissance only discovered Archimedes in the 1530s, and for long only superficially. The first to express a (purely ideological) high appreciation was Ramus in 1569, and the first to make creative use of his mathematics was Viète in the 1590s.

Contents

MARSHALL CLAGETT
and
MARIE-DOMINIQUE CHENU
in memoriam

Since four centuries, Archimedes is known among those interested in ancient Greek mathematics – not only those competent in the field – as one of its greatest names. Witness, for present times, the overwhelming interest in the "Archimedes palimpsest".

But it was not always so, neither in the ancient Latin world nor in the Latin Middle Ages or the European Renaissance. Much of the time, Archimedes was certainly an important name among literati. The foundation for his fame, however, varied as much as the situation of the literati themselves.

In the following, I shall trace the fate of this Protean "Archimedes" in the Latin world (excluding thus Greek Antiquity as well as Byzantium and the Islamic world), from Cicero's times until the end of the Renaissance period around 1600.[1]

From Cicero to Cassiodorus

The earliest Roman writer whom we know to have dealt with Archimedes (and the one who spoke more often of him than any other) was Cicero.

Most famous is his account in *Tusculanae disputationes* V.xxiii of how he found Archimedes's tomb marked by a column carrying a sphere and a cylinder, going on with a less oft quoted praise of the felicity of the philosopher and the mathematician as compared to that of the tyrant Dionysios. Also in *Tusculanae disputationes* (I.xxv) Archimedes's *sphere*, a mechanical model of the planetary system and its motions, is praised as the product of a divine mind. The same sphere is spoken of in *De natura deorum* II.xxv; in *De re publica* I.xiv this model is said to have been generally considered Archimedes's glory, and *Academica* II.xxxvi lets him prove by geometric diagrams that

[1] For Archimedean *mathematics* during the medieval and Renaissance periods, Marshall Clagett's *Archimedes in the Middle Ages* has evidently been an all-important resource. The *Archimedes figure* of the Renaissance has been dealt with from compatible but different perspectives in [Laird 1991] and [Høyrup 1992].

The writing of the paper was spurred by a request to write an encyclopedia article on the topic for "a work of tertiary literature [containing] digested knowledge [but] neither research literature (primary literature) nor review articles summarizing original papers (secondary literature). Content therefore consists of established information in the particular field". As I found out, established published information was too patchy to allow the writing of such an article. The paper is the outcome of the ensuing pursuit of an over-all picture.

the sun is much larger than the earth. *De finibus* V.xix praises him for being so occupied with his diagrams in the sand that he did not notice Syracuse was taken. *De oratore* III.xxxiii speaks of Euclid and Archimedes as the two cultivators of geometry, while *Actio in Verrem II,* IV.lviii, refers in passing and *hors propos* to Marcellus's admiration for Archimedes. *Oratio pro Cluentio* xxxii says about the multiplication 16×40000 = 640000 that Archimedes could have done no better. Finally, two letters to Atticus (XII.4, XIII.28) refer to an intricate diplomatic problem as a "problem for Archimedes", indicating that Archimedes's unspecific ingenuity was proverbial at least in Cicero's circle at the time.

In Augustean times, Livy (*Ab urbe condita* XXIV.xxxiv, XXV.xxxi) mentions Archimedes's astronomical fame in passing and then describes his war machines in detail, and finally tells how he was killed while drawing figures. Ovid, *Fasti* VI.277 mentions the sphere "made by Syracusan art"[2] without mentioning Archimedes by name. A generation later, Valerius Maximus (*Facta et dicta memorabilia* VIII.vii.7) speaks generically about the efficiency of Archimedes's war machines, about Marcellus's admiration for his genius, and about Archimedes's death. Vitruvius (*De architectura* I.i) speaks about technical manuals written by Ctesibios, Archimedes and others, which however one cannot understand without having learned natural philosophy; later on in the same chapter he speaks of mechanics writings by Aristarch, Philolaos, Apollonios, Archimedes and others. The introduction to book IX tells the anecdote of Hieron's crown and Archimedes's exposure of the fraud. The Elder Pliny lists Archimedes as one of his many sources for the cosmology of book II of his *Historia naturalis* but does not cite him in the text; in VII.xxxvi.125 he calls Marcellus in as witness of Archimedes's knowledge of the sciences of geometry and machines. In the later first century CE Quintilian adds to a discussion of the cosmological insights provided by geometry (I.x) that he will not go into the details of tactics nor speak about Archimedes's single-handed defense of Syracuse. In the same epoch or slightly later, Florus (*Epitome* I.xxii.33) mentions Archimedes's ultimately failing defense of Syracuse. In the mid-second century Apuleius's *Apologia* [ed. Nisard 1865: 212] ascribes to Archimedes a large treatise explaining rainbows and other optical phenomena, adding that he is most famous for his study of convex and concave mirrors in spite of his admirable subtlety in geometry in general.[3] Probably in the early third century, Solinus's *De mirabilibus mundi* V.13 mentions Archimedes's knowledge of stars and machines, with no more details.

In the fourth century, Ammianus Marcellinus (XXVI.i.8) lists Meton, Euctemon, Hipparchos and Archimedes as the most distinguished students of the stars; Firmicus Maternus (*Matheseos libri VIII* VI.xxx.26 [ed. Kroll & Skutsch 1897: II, 148]) refers

[2] As all translations in the following where no translator is indicated, this one is due to the author.

[3] Such a work has not survived, but also Theon of Alexandria and others refer to it, so it is likely to have existed – cf. [Heiberg 1972: II, 550].

briefly to Archimedes's ingenious sphere and the efficacy of his machines; finally, Claudius Claudianus (shorter poems, LI) disparages Archimedes sphere as a poor imitation of the divine creation.

Finally, in the early fifth century, Macrobius (*Commentarii in Somnium Scipionis* I.xix) enrolls Archimedes and the Chaldaeans as supporting Cicero's opinion about the order of the spheres of the planets. Roughly contemporary is probably Martianus Capella telling about Plato and Archimedes rotating golden spheres (*De nuptiis* II.213). Probably somewhere in the fifth century, ps.-Priscian, *Carmen de ponderibus* [ed. Hultsch 1864: II, 95–97] tell the story of the crown.

This overview so far disregarded the (even more meagre) patristic references. In the *Patrologia latina* we find the following:

Tertullian, *De anima* [PL 2, col. 669] refers to Archimedes's wonderful hydraulic organ. Lactantius, *Divinarum institutionum* II [PL 6, col. 297] seems to borrow from what Cicero writes in *Tusculanae disputationes* I.xxv about the sphere. Orosius, *Historia* IV.xvii [PL 31, col. 896] speaks about Archimedes's machines and their efficiency in defending Syracuse; the words suggest use of Valerius Maximus, but the death story is omitted. Augustine, in *De utilitate credendi* [PL 42, col. 74][4] asks rhetorically who would take Epicuros as his guide to Archimedes's geometrical writings – "against which he spoke with much tenacity, in my opinion without understanding them".[5]

While this remark suggests that Augustine (alone among Latin authors, at most together with Apuleius!) understood not only that Archimedes made geometry but also that proofs and not mere drawings or results were essential, Claudianus Mamertus [PL 53, col. 782] only refers to his use of the *radius,* in parallel to Orpheus's use of the plectrum (etc.).[6] Similarly, Cassiodorus's *Institutiones* II.vi.3 only mentions Archimedes along with Euclid and Apollonios "and other authors" as Greek writers about geometry [PL 70: col. 1213]. His *Epistola* XLV [PL 69. col. 539, also PL 63, col. 564] states that Boethius translated "the mechanician Archimedes".

4 My translation, as other translations with no identified translator in the following.

5 Epicuros being older than Archimedes, Augustine presumably refers to Epicurean objections to the foundations of theoretical geometry which Archimedes shared, perhaps more specifically to Eudoxean theory – cf. [Sedley 1976] and [Cambiano 1999: 587–590] (the passage in Cicero, *Academica* II.xxx, about Polyaenos the geometer converted to Epicureanism and then claiming the whole of geometry to be false, cannot be Augustine's source).

6 The passage is also found in a letter to Claudianus from Sidonius Apollinaris (*Epistulae* IV.iii.5), which can hence be supposed to be Claudianus's direct source

The Latin Middle Ages

From the Middle Ages proper, the *Patrologia latina* only offers two references. Paulus Winfridus (or Diaconus) [PL 95, col. 784] repeats a couple of lines from Orosius about Archimedes and the efficiency of his machines in the defense of Syracuse in his *Historia miscella;* in an *Epistola de duplici solis eclipis anno 810,* Dungalus Reclusus [PL 105, col. 450] borrows what Macrobius tells about the order of the heavenly spheres as described by Cicero and in agreement with Archimedes and the Chaldaeans. In total, four lines, out of the some 11–12 million lines in those volumes of the *Patrologia latina* that cover the period 550–1200.

The 12th century gave access to some genuine Archimedean works, and to works drawing on Archimedes. Two translations of the *Measurement of the circle* were made – one possibly by Plato of Tivoli, the other in any case by Gerard of Cremona. The latter circulated well among mathematically active university scholars – witness the production of at least nine revised versions from the 12th through the 14th century [Clagett 1964]. Archimedean material – explicitly ascribed to Archimedes – was also contained in the *Verba filiorum* of the Banū Mūsā [ed. Clagett 1964: 264], similarly translated by Gerard. The well-circulated *De curvis superficiebus* compiled in the late 12th or earlier 13th century by John of Tynemouth (also responsible for the so-called Adelard-III version of the *Elements* [ed. Busard 2001]) contained material in Archimedean style though not in direct translation; Roger Bacon ascribes it to Archimedes in the *Communia mathematica* [ed. Steele 1940: 44] while he never refers to the genuine works (as we shall see, others were to make the same ascription until the 16th century).

In 1269, William of Moerbeke made an almost complete translation of the Archimedean corpus from the Greek [ed. Clagett 1976] – among the works known today, only *The Sandreckoner* and *On Method* are lacking.[7] Nobody but Witelo appears to have used it in the 13th century, but in the 14th it was drawn upon by Jean de Murs, Oresme, Henry of Hesse and Albert of Saxony [Clagett 1978: 3–144] – all linked to Paris university.

Outside the restricted circle of these five – Witelo, Jean, Oresme, Henry and Albert – only Gerard's translation of the *Measurement of the Circle* had repercussions. Around 1250, Vincent of Beauvais combined in *Speculum historiale* V.XLIII [1624:149]) quotations from Orosius (the machines and the defense of Syracuse) and Valerius Maximus (the death story) with a reference to Archimedes's *Measurement of the Circle,* "of which Aristotle says that it can be but is not known".[8] In the 1340s, Walter Burley added to this in

[7] *On Method,* as well known, is a recent discovery – details in [Heiberg 1907].

[8] The reference is probably to *Categories* $7^{b}31$–33, maybe in interaction with Boethius's commentary to the work [PL 64, col. 231]).

De vita et moribus philosophorum [1487: b.ii^{v}] a long *verbatim* borrowing from Valerius Maximus. But that is as far as the integration of the Latin Archimedes with the author of mathematical writings went in 13th-14th-century scholastic culture. On the whole, the Arabo-Latin as well as Moerbeke's Greco-Latin translations circulated in complete isolation from interest in the person – and information about the person was drawn from very few sources: Orosius, Macrobius, and Valerius Maximus.[9]

Early Italian Humanism

As a rule, 14th-century Humanists were interested in neither the works nor the person. One was, however, though only in the person: Petrarca – often regarded as the most eminent of early Humanists, and therefore worth looking at even if an exception.

To judge from Petrarca's letters, Archimedes was not central even to him – he is not mentioned at all in the *Familiari,* nor in the *Extravagantes;* only two passages in the *Senili* refer to him, both from 1362. One [ed., trans. Fracassetti 1869: I, 46] speaks about his overpowering love of studies, on a par with that of Cato, Varro, Livius Drusus, Appius Claudius, Homer, Socrates, Isocrates, Sophocles, and eight more named classical writers and philosophers; the other (*ibid.* p. 63) lists him together with Ptolemy and Firmicus Maternus as representatives of the class of famous astrologers (in the modern sense of that word, to judge from the context).

Yet Petrarca produced two biographical notices. In *De viris illustribus vitae* (1338), within the biography of Marcellus [ed. trans. Razzolini 1874: I, 280–282], he narrates in his own words Valerius Maximus's stories about Archimedes's powerful war machines and his death. This gives Petrarca the possibility to insert his own observations, twists and additions: that even though Firmicus Maternus disparages Archimedes as a mechanic, he was formidable both as an astrologer and a mechanic; and that the figures he was drawing were astrological or geometrical; finally, that Cicero found Archimedes's grave. In *Rerum memorandarum libri* I.23 (1343) [ed. Billanovich 1943: 22–24], an arrival in Syracuse offers the occasion to expand Livy's single line about Archimedes's unique observation of the heavens and the stars into a general praise of Archimedes's study of nature and of his sphere. This sphere is praised because it allows us to understand the celestial motions not only by the mind but also, apparently better, by our eyes – rather un-Platonic, but Petrarca does not know. Even more commendable, according to the ascending rhetorical curve, were Archimedes's terrestrial mechanical feats; his fatal distraction when Syracuse was taken, on the other hand, is slightly censured as "immoder-

[9] It may not have helped integration that the figure known from Latin authors was *Archimedes,* while the author of the translation from the Arabic was designated *Archimenides.* However, Moerbeke seems to have been just as convinced as Vincent of Beauvais that the two were identical – see [Clagett 1976: 66].

ate" – once again, Platonizing celebration of the pure intellect is wholly foreign to Petrarca's perspective. All in all, the value scale is that of Cicero and the Roman elite of his time, and Archimedes is *almost* perfect when measured thus. At least, as we see, Archimedes was part of the legitimate ancient heritage, respected because of his sophisticated machines, his planetary model as well as those that could serve efficiently in war. Even Petrarca's aversion for astrology only induces him to compare modern astrologers unfavourably to Ptolemy, Archimedes and Firmicus Maternus, not to denigrate this aspect of his "Archimedes".

A final reference to Archimedes, in *De suis ipsius et multorum ignorantia* (1367) [ed. Capelli 1906: 47] says nothing about Archimedes himself, it only denounces the opinion that Archimedes's construction of the model of the heavens should be more admirable than nature's production of the original – apparently an echo of Claudianus.

The 15th century

Other 14th-century Humanists seem not to have been interested in the Archimedes figure at all. That changed to some extent in the 15th century, as "higher artisans" (architects, painters, military engineers, etc.) and their ambience began to have intercourse with Humanists. The engineer Mariano Taccola (1382 to c. 1453) was proud to be known as the "Archimedes of Siena" [Prager and Scaglia 1972: 17 and *passim*]; he was no mathematician, and never mentions Euclid nor Archimedes in his writings; but he was an accomplished designer of military and other engines and also a good artist. Filippo Brunelleschi, with similar credentials, enjoyed the similar honour to be considered a "second Archimedes" [Laird 1991: 633].

Those who awarded such honours must have had an idea of what the name "Archimedes" stood for – at the overlap between, on one hand, what information about Archimedes was available, on the other, what Taccola and Brunelleschi were famous for. That overlap encompasses neither mathematical theory nor pictorial arts – the "Archimedes" of the day was a skilled engineer and architect, a designer of machines. We may imagine he was also thought of as a patriot defending his city and his prince by means of his supreme skills, but there is no evidence for that.

The most likely main source for the admiration of Archimedes among the higher artisans is Vitruvius, so to say a colleague. Leon Battista Alberti – the earliest outstanding Humanist who was also active as a higher artisan, and Brunelleschi's partner in the creation of perspective technique – takes over Vitruvius's version of the crown story in the *Ludi matematici* [ed., trans. Williams, March & Wassel 2010: 66]. However, his *De re aedificatoria* shows that increasing familiarity with Plutarch's biography of Marcellus was also influential – in VI.vi [Alberti 1541: 83v] he cites Plutarch for the story of Archimedes moving a loaded ship on the ground and for his promise to be able to move this world if he could only go to another one.

As informative as borrowings is the way they are filtered. Medieval and early Renaissance writers could not go beyond the Latin sources with which they were familiar, and that may explain the paucity of what they tell; Alberti, however, distances himself from part of what he reads, and omits an essential part of Plutarch's Platonizing message. Firstly, Plutarch's insistence that Archimedes considered mechanical work and even mechanical treatises unworthy (*Marcellus* xvii.3–4) is not argued against, it is by-passed in silence – after all, it would have undermined Alberti's own undertaking. Neoplatonism and esoteric Platonism remain as unthinkable as they had been for Petrarca. Secondly, in *De re aedificatoria* IX.x [1541: 245^{r}] Alberti declares not to intend to be Zeuxis in painting, Nicomachos in the manipulation of numbers, or Archimedes when dealing with angles and lines; as in the treatises he has written about painting and drawing (that is, *Elementi di pittura* and *Elementa picturae*) he will stick to the basic principles and that which gives honour and fame to the architect. Admitting here that Archimedes had been the supreme geometer (which he could read in Plutarch, *Marcellus* xvii.4–7), Vitruvius distances himself from this aspect of the Hero.

Even among Humanists with no direct links to architecture and engineering there seems to have been some interest in Archimedes, probably because of what the Latin sources tell about him. In 1423–24, rumours circulated that Rinuccio da Castiglioni had brought back from Byzantium an unknown Archimedes manuscript; Rinuccio confirmed to have a manuscript by Archimedes *De instrumentis bellicis et aquaticis* [Rose 1975: 31] – suspicious as an Archimedean title, to be sure, but in line with commonplace Latin lore, which can only reinforce our doubts. In any case, nobody ever saw the manuscript in question (then or in later centuries), and some 25 years passed before more happened among Humanists. Around 1450, however, Jacopo da San Cassiano Cremonensis made a fairly complete translation (dealt with in depth in [d'Alessandro & Napolitani 2012]). Of the works translated by Moerbeke, *On Floating bodies* is missing in the new translation and from the manuscript used by Jacopo, while the *Sandreckoner*, absent from Moerbeke's Greek manuscript, is translated by Jacopo. None, of course, knew *On Method*. Whether Jacopo's translation was made while he was still in Mantua or after his move to the Papal court is not clear, but in any case the undertaking agrees well with Nicholas V's broader translation programme. This translation is the basis for Regiomontanus's judgment (in a lecture "explaining briefly the mathematical sciences and their utility", held in Padua in 1463/1464 [ed. Schmeidler 1972: 45]) that Euclid was

> followed by Archimedes, citizen of Syracuse, and by Apollonios of Perga, customarily called the Divine because of the height of his genius, of whom it is not easy to say whether one is to be preferred to the other. While namely Apollonios described the elements of conics in eight books, which have never been put into Latin, the first rank appears to belong to Archimedes the Sicilian by the variety of publications, which under Pope Nicholas V were rendered in Latin by a certain Jacobus of Cremona.

This translation was also the one Regiomontanus intended to print [ed. Schmeidler 1972: 533].

Both Jacopo and Regiomontanus were connected to Bessarion's circle, though at different moments. Bessarion himself was interested enough to get a copy for his library, and Cusanus expressed his gratitude to the Pope for having put the manuscript at his disposition (which, however, did not affect his idiosyncratic approach to mathematics much, see [Clagett 1978: 297–319]). Apart from that, echoes among Humanists cannot be discerned during the first decades. It is noteworthy, moreover, that Jacopo himself had a thorough training in university philosophy; Regiomontanus, on his part, was an accomplished university astronomer, already familiar with the medieval direct and indirect Archimedean tradition before being introduced to Humanism by Bessarion [Clagett 1978: 343–354]. Both therefore had a substantial foundation allowing them to understand Archimedes as an awe-inspiring geometer. Humanists with no similar background could only take such Latin sources on faith as claimed without specifying that he was ingenious – among other more important things also as a geometer; at most they could read Plutarch's generic praise. Even Regiomontanus, however, did not go beyond recognition – his own mathematics is not marked by Archimedean inspiration (after all, his primary interests were astronomy and astrology, as also obvious from the rhetoric of the Padua lecture if read in its entirety).

Among writers in the vicinity of the abbacus tradition (not fully separate from the Italian university environment, some abbacus teachers also taught mathematics and astronomy at university) and the higher artisanate, there was a certain impact. Piero della Francesca, who had access to Jacopo's translation (and even copied at least part of it [d'Alessandro & Napolitani 2012: 84*f*]), has some Archimedean namesdropping in *De quinque corporibus regularibus* for matters that already appear but without such reference in his earlier *Trattato d'abbaco* [Clagett 1978: 396–398]. There are apparently no references in his works to Archimedes beyond that. Luca Pacioli, too, must have seen Jacopo's translation [Clagett 1978: 448, 460], but mostly draws on the direct and indirect medieval traditions for Archimedes's mathematics, giving only quite imprecise references; on the other hand, in the *Summa de arithmetica* … [1494] he refers in the unpaginated dedication to Duke Guidobaldo to "the great Syracusan geometer Archimedes" who, with "his machines and mechanical inventions kept Syracuse safe for long". The passage is repeated almost verbatim in his *Divina proportione* [1509: b ii^r] – now addressed to Ludovico Sforza of Milan and characterizing Archimedes as a "noble ingenious geometer and most worthy architect" and pointing out that he defended his *patria*. In the same work Pacioli gives further imprecise references to Archimedes's results.

The case of Leonardo da Vinci is somewhat similar but not identical. He appears to draw on recent as well as medieval Archimedean mathematics [Clagett 1978: 478]. He understands that Archimedes "never squared any figure with curved sides" but "only squared the circle minus the smallest portion that the intellect can conceive, that is the

smallest point visible" [trans. Richter 1883: II, 446]. Also insightful at least as to the conditions of his own epoch, Leonardo believes Marcellus wanted to find Archimedes in order to make use of his services – but he appears to draw on sources from approximate memory, believing that it was Cato, not Cicero who found Archimedes's tomb, and that the latter defended some Spanish city in its wars with the English (*ibid*., pp. 446, 451).

One late-15th-century pure-breed Humanist manifested interest in Archimedes the mathematician: Giorgio Valla. He bought the manuscript from which Moerbeke had made his translation, and he at least saw Jacopo's translation [Clagett 1978: 462]. He also possessed a manuscript of Proclos's commentary to *Elements* I, copied in part by himself [Rose 1975: 47]. His posthumous *De expetendis et fugiendis rebus* [1501] returns to Archimedes time and again. Regularly it refers to what *could* have been known from Plutarch and the Latin sources – but Valla draws his information from Proclos (the other way, almost every passage in Proclos's commentary referring to Archimedes is used). Sometimes Valla discusses Archimedes's mathematics (not merely quoting or using results), in which cases neither Latin sources not Plutarch would have had anything to offer. On page a I.iii^{v} we find the siege and death stories, embellished by the additions that Archimedes did not ponder flight, and that he used geometry to inspect the possibilities of defense (apparently Valla's own inventions, the latter corresponding to what court mathematicians of the epoch were doing); page I.b.i^{r} speaks of Archimedes's work on mechanics (drawn from Proclos [ed. Friedlein 1873: 41]) and page I.n.ii^{r} relates the ship and crown stories (again from Proclos [ed. Friedlein 1873: 63*f*]); page I.n.ii^{v} borrows from Proclos [ed. Friedlein 1873: 68] that Archimedes mentions Euclid. Further, passing to mathematics, on page I.n.v^{v}, Archimedes is cited for the definition of the straight line as the shortest lines between the extremities (actually the first postulate of *On the Sphere and Cylinder*) and claims it to be equivalent to Euclid's definition, which is indeed what Proclos argues [ed. Friedlein 1873: 110]; page I.i^{v} ascribes to Archimedes the description of 13 regular and semiregular bodies, information drawn from Pappos's *Collectio* V.xix [ed. Hultsch 1876: I, 352–354]. Some further references to Archimedean results could come from anywhere (often they are made in passing in an unspecific context), or they may be results of Valla's own readings of Archimedes (in particular those of vol. II). Chapter ii of Book XIII (pages u.v^{v}–x.iiii^{r}), draws upon Eutocios's commentary to *On the Sphere and Cylinder II.*

All in all, Valla's "Archimedes" builds upon the same sources as his acquaintance with Archimedean mathematics – he needs neither Plutarch nor Latin anecdotes, even though he presents much of their substance.

The 16th century

The 15th-century "Archimedes" did not disappear completely for that. In 1568, Giorgio Vasari [ed. Milanesi et al 1846: XI, 98] could still characterize the painter and architect Bartolomeo Genga as a new Archimedes because of his design of fortifications. Twenty years later, Sperone Speroni, trained in natural philosophy (by Pomponazzi, among others) was told to have been in doubt whether the famous technician had left anything in writing – thus Bernardino Baldi [ed. Narducci 1886: 401], who forgives this "alert mind of our age" because of his different profession. However, *within* the increasingly important and increasingly competent mathematical professions,[10] Archimedes the mathematician came to equal or even overshadow his mechanic namesake.

The 16th century brought the printing of Archimedean works. In [1503], the astronomer Luca Gaurico published a small volume containing Archimedes's *Quadrature of the parabola* and *Measurement of the circle* in Moerbeke's translation along with ps.-Campanus's *Tetragonismus id est circuli quadratura* (the latter provided with due critical commentary). In [1543], Tartaglia republished Gaurico's two Archimedean editions together with *On the equilibrium of Planes* and *On Floating Bodies* I, even these in Moerbeke's version. Without claiming it explicitly, Tartaglia managed to make the world believe that he had translated from the Greek himself – a belief that survived until the rediscovery of the Moerbeke manuscript in 1882 [Clagett 1978: 553]. In [1560], Tartaglia further published as Book III of *La quarta parte del general trattato de' numeri e misure* an Italian translation of *On the Sphere and Cylinder* I; it was probably first made in 1531 on the basis of a Moerbeke text but then corrected by means of Jacopo's translation before the final publication [Clagett 1978: 541].

The *editio princeps* of the Archimedean corpus was published by Thomas Gechauff Venatorius in Basel in [1544]. The Greek text was accompanied by Jacopo's translation as corrected by Regiomontanus. These two, and the new partial translation which Federico Commandino published in [1558a] and [1565a], were the main sources for later 16th and 17th-century Archimedean work [Clagett 1978: 568].

[Commandino 1558a] contained *On the Measurement of the Circle, On Spiral Lines, On the Quadrature of the Parabola, On Conoids and Spheroids,* and *The Sandreckoner;* they were translated directly from Greek manuscripts, and accompanied by a separate volume of commentaries [Commandino 1558b]. [Commandino 1565a] was a revised version of the Moerbeke translation of *On Floating Bodies* – this Archimedean treatise had not been included in the *editio princeps,* nor was it contained in available Greek manuscripts. All were much sounder than preceding translations.

[10] See [Biagioli 1989].

A further incomplete translation was made by Antonius de Albertis at some moment before 1555 [Clagett 1978: 1357–1365]. It remained in manuscript and appears to have had no influence.

Better text versions and publication in print is one aspect of the novel way the 16th century approached Archimedes. Another aspect is the *use* made of Archimedean theory. Most important in this respect is what was done by Francesco Maurolico and Commandino.

Maurolico's "Archimedean" inspiration went beyond mathematics. When unpaid Spanish auxiliary troops were marauding Sicily in 1539, so he tells, he "put aside his ruler and compass and took up arms, the example of Archimedes warning him not to be devoting himself to describing lines and circles at the time of such danger"; concretely, he assisted in the fortification of Messina [Clagett 1978: 755].

Already in 1534, he had composed his own versions of *On the Quadrature of the Parabola, On the Measurement of the Circle,* and *On the Sphere and the Cylinder,* making use of the various direct and indirect medieval traditions combined with information drawn from Valla [Clagett 1978: 773]. In *De momentis aequalibus* from 1547–48 he took up the investigation of the centres of gravity of solids – as Maurolico [1685: 156] points out, in *On the Equilibrium of Planes* Archimedes had only dealt with those of plane figures. This topic also occupied him later on, and in the 1550s or 1560s he applied Archimedean mechanics and medieval impetus theory to the ps.-Aristotelian *Mechanica.*

Commandino, too, was to publish on the centres of gravity of solids in [1565b]. Giovanni Battista Benedetti, Guidobaldo del Monte and Baldi confronted the ways mechanical questions were dealt with by Archimedes, Heron, Pappos, Aristotle, ps.-Aristotetle, Jordanus and impetus-theory – confronting also each other [Rose 1975: 154–156, 230–233, 249–253].

As could be expected, knowing the Archimedean texts intimately and using them changed the image of Archimedes among competent mathematicians. This can be illustrated by how Girolamo Cardano, Commandino and Baldi spoke about him.

In *Encomium geometriae,* read by Cardano in the Academia Platina in Milan in 1535, we find a long list of names of geometers, drawn as far as the minor figures are concerned from Proclos's recently published commentary to *Elements* I and thus ultimately from Eudemus. Then comes the observation [Cardano 1663: 443] that

> they are all defeated by Archimedes of Syracuse, almost all of whose findings we possess. A man of the highest genius, and who will have shown the circumference of the circle pretty closely, and taught by solid geometry how to interpose two lines between two others in continuous proportion. But that has been lost.

In the context of a praise of geometry, only Archimedes's mathematics finds its place. In *De subtilitate* [Cardano 1550], the situation is different. Book I refers repeatedly to Archimedes in the discussion of mechanical questions; book IV refers to a book about parabolic burning mirrors mistakenly ascribed to Archimedes and to Archimedes setting fire to Roman ships by means of burning mirrors (a story Cardano attributes to

Galen ⟦[11]⟧). Book XVI, finally, situates Archimedes first in a list of subtle minds (pp. 313*f*)

> not only because of his works which have now been published but also because of his mechanics which, as Plutarch relates in his *Life of Marcellus,* discouraged the Roman troops time and again by his inventions, and discourages us no less by Galen's testimony, in both areas not only the first but inimitable.

Archimedes is followed by Ptolemy, Aristotle, Euclid, John Scot, Swineshead, Apollonios, Archytas, al-Khwārizmī, al-Kindī, Jābir ibn Aflah, Galen and Vitruvius[12] – a striking order, given Cardano's own primary engagement in astrology, medicine and, when it comes to mathematics, algebra.

Venatorius, in the dedicatory letter introducing his edition of the Greek text, speaks at some length about the mathematics it deals with; but in the end he comes to the defense of Syracuse and to Archimedes's promise to move the earth if only he might get another globe.

Both Commandino's translation [1558a] and his commentaries [1558b] contain dedicatory letters speaking about Archimedes. The former, addressed to Ranuccio Cardinal Farnese (to whose household Commandino belonged since years), first explains that the mathematics, dealing with the intelligible only, have higher rank than metaphysics and natural philosophy: these depend on matter, and even Plato and Aristotle cannot agree about them. This higher rank is also to be ascribed to those mathematical disciplines which *contemplate* the sensible: mechanics, astronomy, optics, etc. So, nothing is more useful nor more necessary for the human race than mathematics, neither in private matters nor in public management – not only geometry, arithmetic and proportion but also mechanics and the preparation of instruments (perhaps for the sake of decorum written in Greek). This leads naturally to Archimedes – first his astronomy and sphere, then arithmetic (the *Sandreckoner*). Though Archimedes is not known for anything in music, Commandino finds it plausible that he excelled even in this discipline. That he was "a kind of God in geometry nobody sane of mind can deny". Mechanics he first practised for war, and then transferred to peace – specifically, we hear about the ship, the defense of Syracuse, about Marcellus's grief, and about Cicero finding the grave. "Much other we hear which, although it be eminently true, arouses more admiration than belief among posteriors". In contrast to all less competent predecessors, Commandino dares characterize Archimedes's "few extant writings [as] most obscure, and hardly understandable by

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11 ⟦Actually, Galen's words are ambiguous and could also refer to burning pitch. [Simms 1991] investigates the genesis of the story of the burning mirrors. Cf. also [Schneider 1968].⟧

12 For some reason, Ptolemy has disappeared from the list in [Cardano 1663: III, 307], Aristotle becoming *secundus* instead of receiving the *tertium locum.* This is the list I cited in [Høyrup 1992: 94]. Archimedes is not touched.

exertion of the greatest efforts" – but Eutocios, Regiomontanus and Maurolico, so he adds, have been able to fathom them.

The dedication of the commentaries is addressed to the Cardinal's brother-in-law, Duke Farnese. It is much shorter, and concentrates on military mathematics – thus reminding us that dedications tell us as much about the opinion the writer holds about the addressee and what interests the latter as about the writer's own thinking about the subject-matter. None the less, the former dedication seems really to reflect Commandino's own thought. Later in the commentary volume (fol. 42^{v}) we find another personal paratextual observation – namely that Maurolico is so "skilled in mathematics that in these times he can with justice be said to be another Archimedes".

Baldi had studied with Commandino, and began work on his *Vite de' matematici* after the latter's death in 1575. The longest of these is that of Pythagoras, written in 1588; according to Baldi's excuses [ed. Narducci 1887: 199], however, this is for Italian patriotic reasons. Almost as long is the biography of Archimedes (from 1595), which needs no excuse [ed. Narducci 1886: 388]:

> In all domains there have been some who, having arrived at the peak of excellence, have demonstrated how far the human intellect can advance in that direction. Without doubt Archimedes was such a man in mathematics, since the first place is due for good reasons to him.

Baldi builds as much as possible on Archimedes's own introductory letters; but he also draws on Plutarch, Cicero, Ovid, Ptolemy, Pappos, Proclos, Martianus Capella, Lactantius, Claudianus, etc., confronting them critically with each other. This allows him to present all the usual stories about Archimedes's life and his mechanical feats, and also to speak of his surviving works (still including the falsely attributed *De curvis superficiebus*) as well as lost writings known from more or less reliable references.[13] About Guidobaldo del Monte it is said because of his "most subtle demonstrations", and in particular because of his machines, that he possesses "if not the soul then at least the same genius as Archimedes" (p. 390). In conclusion (p. 453) Commandino is cited for the opinion that "that one can hardly call himself a mathematician who has not studied the works of Archimedes".

Among the mathematicians of the outgoing Renaissance, Archimedes had thus becomes primarily a mathematician creating advanced theory; but his fame as most skilled in theoretical as well as practised mechanics was not discarded. These mathematicians themselves, often working at princely courts, were engaged not only in theory but also

[13] On the other hand, while referring to Apuleius's testimony concerning catoptric writings by Archimedes, Baldi corrects Maurolico's ascription of ibn al-Haytham's treatise about that topic to Archimedes [ed. Narducci 1886: 401], ⟦pointing out that this work speaks of Archimedes in the third person, and cites the Byzantine architect and geometer Anthemios of Tralles "who lived and flourished many centuries later"⟧.

in practical service to prince and state. Their Archimedes was one of their own kind, as that of Taccola and Brunelleschi had been in the early 15th century – only their kind had changed.

Archimedes in the Northern Renaissance

All of this, from Petrarca onward, concerned Italy (with Venatorius's publication of the *editio princeps* as only exception – a partial exception, Venatorius having learned much of his mathematics in Italy).

In the Northern Renaissance, references to Archimedes's mathematics are late and rare, and those to the person even fewer. Stifel tells the crown story in the *Arithmetica integra* [1544: 267^{r}], and refers to $3^1/_7$ as the value for the ratio between the circular perimeter and diameter "whose originator is said to be Archimedes" (225^{v}). The dedicatory letter of [Scheubel 1550: II] mentions the defense of Syracuse, after which neither Archimedes nor his mathematics appears in that work. In the *Protomathesis,* Oronce Finé [1532: 59^{v}–82] gives an Archimedean squaring of the circle (followed unfortunately by an "exact" construction of his own). Other works of his contain the same, or scattered references showing that Finé has consulted [Valla 1501]. Joannes Buteo's *De quadratura circuli* from [1559] obviously also refers repeatedly to Archimedes – it consists indeed of translations with commentary of *On the Measurement of the Circle* and Eutocios's commentary, and of refutations of Finé's and other erroneous circle squarings. But Archimedes only appears as needed for this purpose.

The earliest example of Northern worshipping "Archimedism" is to be found in Petrus Ramus's *Scholae mathematicae* from [1569]. The presentation of Archimedes begins (p. 26) almost as that of Baldi:

> God has decided that there should be in each art something like a unique idea which everybody studying the discipline would propose to himself as a model – as in eloquence, Demosthenes and Cicero, and in medicine Hippocrates and Galen: thus Archimedes in mathematics.

From Ramus's own pseudo-practical orientation, from his lack of mathematical depth and from his castigation of Euclid's "Platonic error" (pp. 27*f*) one should expect him to praise first of all Archimedes the engineer. But that does not happen – first comes Archimedes's excellence as a pure mathematician, excelling in arithmetic (the *Sandreckoner*) as well as geometry. Only afterwards it is admitted that according to Plutarch Archimedes was imbued with Plato's error – and then we are told, as implicit refutation, about the crown and about the mechanical and military feats. Ramus's own mathematics was uninfluenced by Archimedes – his Archimedism was pure ideology, it did not inspire him to become an Archimedean.

The first Northern figure to approach Archimedean mathematics *as mathematics* and not only as a small collection of famous results is thus Viéte, at the very end of the 16th

century. In his *Opera mathematica* [ed. van Schooten 1646], the Archimedean treatise which plays the largest role in *On Spiral Lines,* being referred and used repeatedly in *Apollonius Gallus* and *Variorum de rebus mathematicis responsorum liber VIII.* When it fitted his mathematical intent, Viète was thus an Archimedean; however, there is no general praise in the works, neither of the person, nor of Archimedes as a mathematician; even for a mere presentation of either of these one will look in vain. In double contrast to Ramus, Viète, while sometimes Archimedean, was no Archimedist.

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Part II
Oblique Glances and Birds-Eye Views

Chapter 18 (Article II.1) Existence, Substantiality, and Counterfactuality: Observations on the Status of Mathematics According to Aristotle, Euclid, and Others

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

The article tries to answer the (arguably anachronistic) question whether ancient philosophers and mathematicians held mathematical objects or mathematical truth as a whole to *exist*. Aristotle being the philosopher who more than anybody else takes up the general question of "existence" with all its inherent ambiguities, the first and major section analyses what he has to say about mathematical objects being in some sense *substances*, that is, existing entities. The second section looks at Euclid's postulates (literally "requests"), three of which are simply false according to the standard cosmologies of the time. The likely answer of geometricians, actually offered by Cicero, is that these postulates have to be granted if geometry is to be practised – that is, so to speak, that they provide the foundation for what Wittgenstein would call a "language game". The last section looks at the question whether mathematics as a whole is a free construction or somehow determined, from the viewpoints of Pythagorean, Platonic and post-Platonic, Aristotelian and other philosophies.

Contents

In memoriam
Stig Andur Pedersen

The question of mathematical existence

In current loose parlance, the view that mathematical objects *exist* is regarded as *Platonism*. It might therefore seem obvious that this existence was the basic assumption of ancient mathematics – not least if we accept Proclos's statement in his commentary to *Elements* I [ed. Friedlein 1873: 68^{20f}, trans. Morrow 1970: 57] that Euclid "belonged to the persuasion of Plato and was at home in his philosophy".[1]

Actually, the ancient texts do not speak exactly in terms that can unambiguously be translated into "existence" (presupposing for the sake of the argument that this latter term possesses an unambiguous meaning) – at best they use forms and derivatives of the verb εἰμί,"to be", which (as pointed out in the ancient texts themselves) has a wide, and wider, range of meanings, though certainly including "being there". In order to decide the stance of the ancients with regard to the question of mathematical existence, we must therefore attempt a translation, not *de verbo ad verbum* but "conceptual network to conceptual network". We shall have to ask whether, and in which sense, something mathematical was supposed to exist by ancient Greek mathematicians and/or Greek philosophers discussing mathematics. The point of asking this question in a context which is not primarily concerned with ancient thought ⟦[2]⟧ is that the outcome may elucidate not only ambiguities that inhere in the writings of the ancients but also ambivalences in more recent debates. I shall, however, abstain from drawing such wider conclusions myself on the present occasion.

In contemporary discussions, mathematical "existence" is often meant as a negation of unconstrained constructibility – what *exists* can be *discovered* but cannot be

[1] I use three kinds of translations in the following: (i) some are borrowed from an existing translation, in which cases the translator is identified as here; (ii) others are similarly borrowed but corrected at points where I find the translation unduly free for my actual purpose or inconsistent with other translations used in my text – for these, the translator is identified in the same way, but corrected passages are put into superscript pointed brackets ⟨ ⟩ (empty ⟨ ⟩ means that I have removed a superfluous word; inserted explanations and words from the original text appear in [ ]); (iii) some shorter phrases I have finally translated myself (but obviously controlled against established translations); quotations with no identified translator belong to this category.

All important passages of borrowed translations have been checked against the Greek text (in the case of Aristotle the Loeb editions); difficult passages have also been compared with the interpretations of other translators. For reasons of space, these are referred to only in cases where their reading is discussed.

[2] ⟦The first version of the present paper was presented to the meeting "Existence in mathematics", Roskilde University, 24–25 November 2000.⟧

invented. ⟦[3]⟧ If this perspective is combined with the view that mathematics is first of all concerned with coherent structures, the question to the ancients might be whether they considered mathematics *in toto* (or at least whole mathematical domains) as somehow existing or constrained by reality (and in case, by *which* reality). This is a problem to which we shall return; however, the explicit discussions in ancient writings which come closest to the question of existence are concerned not with mathematics as such or with domains but with *the objects* of mathematics, τὰ μαθηματικά, "the mathematicals" – numbers, ratios, lines, triangles, etc.[4] Do *they* exist?

Aristotle

The surviving writings of the ancient mathematicians are not explicit on this account. This does not preclude a reading that searches for hidden presuppositions, but such a reading is most likely to yield a result if seen in the light of the more outspoken discussions of the philosophers. I shall refrain, however, from a broad coverage of this topic and concentrate on the writings of *the philosopher*, that is, Aristotle.

The central Aristotelian concept in any discussion of what *is really there* is οὐσία, "being" or, in the received translation, "substance".[5] Actually, speaking of a "concept" may be a slight overstatement. Aristotle himself points out in *De anima* II.i (412^{b}9, ed.

3 ⟦I disregard the alternative, logicist point of view that "mathematical existence is nothing other than consistency" [Ferreirós 2009: 34], as too far removed from ancient thought to enter in possibly fruitful dialogue.⟧

4 One may observe that I refrain from engaging in the discussion whether ancient proofs of construction were meant as existence proofs. To ask whether Euclid meant *Elements* I.1 to prove the existence of an equilateral triangle with a particular given side is only meaningful if we know what he and his contemporaries thought about the existence of mathematicals in general.

5 Alternatively, we might translate it "primary being" with Richard Hope [1960], or think of it as what is "really there" (in a sense which is still to be decided). The term is to be kept apart from ὑποκειμένον, "that which is beneath", used by Aristotle about existing things (substances; but also genera) as carriers of properties or *differentiae* [Bonitz 1955: 798]. Even this word is sometimes translated "substance" though more commonly "substrate". Some recent translations and publications - e.g., [Sorabji 1988] – use "subject", in agreement with the linguistic make-up of Aristotle's ontology.

To complete the confusion, translations of Plato often use "essence" for οὐσία (where οὐσίαι may be Platonic Forms); in an Aristotelian context, the "essence" of translations mostly corresponds to τὸ τί ἦν εἶναι, literally "the what it is to be [of the kind in question]" (seen in *Metaphysics* Z, 1035^{b}33, it is true, as a particular kind of form – see below, p. 486).

In order to minimize the chaos, I shall use "substance" consistently for οὐσία and "substrate" for ὑποκειμένον (and correct borrowed translations accordingly). In agreement with Hugh Tredennick's usage in his translation of the *Metaphysics* I shall capitalize Platonic separate Forms but leave Aristotle's immanent forms without capitalization. I shall follow Aristotle's shift between Idea (ἰδέα) and *Form* (εἶδος), without implying that this make any difference in meaning.

[Hett 1936: 68]) that both τὸ ἕν, "unity", and εἶναι, "to be" (from whose participle οὐσία is a nominal derivation[6]), have many senses.[7]

The chief sense of both, according to the same passage, is ἐντελέχεια, "complete reality" or, we might say, genuine existence (the same word which is used about the divine Prime Mover and about the rational soul when Aristotle discusses the possibility of its existence independent of the body irrespective of its status as a form). But perusal of the Aristotelian corpus reveals a rather wide spectrum of possible variations. According to *Metaphysics* Λ, 1070^{a}10–13,20–21, there are three kinds of substance: matter, nature (form), and their unity. However, matter (and, in consequence, even substance) constitutes a hierarchy, no single level: "utmost matter" (ὕλη ἔσχατη), as substrate receiving the qualities of warmth, cold, humid and dry ("utmost form"), gives rise to the four elements, which seen in this perspective are substances; these, on their part, also function as matter which, united in proper proportion (λόγος– another level of form[8]) produce bone, flesh etc., a higher level of substance. But even flesh etc., growing naturally together (συμφύσω) are matter for a still higher level of substance, namely the head and other bodily parts. These then constitute the matter for a "substance in the strictest sense", the living person (Socrates or Callias).

Elsewhere we find the term "material substance" (οὐσία ὑλική) in what seems to be the sense of "non-utmost matter".[9] This agrees without much difficulty with the passage that was just discussed. Not directly in harmony with it, nor however in blunt contradiction, is the usage of the *Categories* (trans. Cook in [Cook & Tredennick 1938]), according to which species and genera (the "universals") are "secondary substances" (δεύτεραι οὐσίαι), the individuals being "primary" (πρῶται οὐσίαι) – individuals being "best spoken of" as substances, and among the universals species more properly than genera (2^{b}7–8,15–18). In contrast, *Metaphysics* Z, 1038^{b}9*ff* argues (echoed by I,1053^{b}17) that the universal (τὸ καθόλου) cannot be a substance. For this reason (1039^{a}24*ff*), even Ideas cannot be (separable) substances, at least if species are constituted from a genus

6 The feminine participle is οὖσα, whence οὐσια; the corresponding neuter is ὄν, which we shall encounter repeatedly in nominalized function as τὸ ὄν.

7 The same ambiguity of the notion of "being" is pointed out in *Metaphysics* I, 1053^{b}25, K, 1064^{b}15, and N, 1089^{a}7–15 (in the former of these passages coupled again to "unity", in the latter used as an argument against Platonism); it can thus be regarded as a key point, not as an accidental or rhetorically motivated observation. Cf. also *Metaphysics* Z, 1028^{a}10.

Less specific but more polemic is a passage in *Metaphysics* A (992^{b}19–21, trans. [Tredennick 1933: I, 79]): "In general, to investigate the elements of existing things [τὰ ὄντα, plural of τὸ ὄν] without distinguishing the various senses in which things are said to exist is a hopeless task".

8 This specification is borrowed from the kindred passage in *De anima* I, 410^{a}2 – cf. also *Metaphysics* N, 1092^{b}18–22.

9 *Metaphysics* H, 1044^{a}15; Θ, 1049^{a}36; and M, 1077^{a}36.

(functioning as substrate) and determined by the imposition of differentiae – but this whole discussion of the status of universals and forms is spurred by the preceding observation ($1029^{a}27$*ff*) that matter is even less a substance – characterized, as stated here (and again in Λ, $1070^{b}36$–$1071^{a}1$), by separability and individuality – than both the form and the combination of form and matter.

All passages discussed so far regard sensible and movable "primary" substances and the matter and form that cause them to exist. In this domain, as we see, the term is supposed to cover primarily individual entities, and matter (at whatever level), form and universals only deserve it in restricted or secondary ways. Substances of this kind may be characterized mathematically (the oft-mentioned bronze sphere is an obvious instance – *Metaphysics* $1070^{a}3$–4), but the mathematical characterization in itself is never discussed as a sensible substance. In *Metaphysics* H $1043^{b}34$ it is said explicitly that *if* really numbers are substances, then at least not as claimed by "some" (probably Pythagoreans) as aggregates of (possibly sensible) monads. Below we shall meet a reference to "sensible circles" which are told *not* to be those of mathematics.

Another category of substance can neither be perceived by the senses nor moved – it is immovable and "intelligible" (νοητός), it can only be reached through the intellect, the νοῦς. In *Metaphysics* Λ, $1069^{a}34$–36 we are told that this separate category is postulated by some, either comprising both Forms and mathematicals (as distinct subcategories or as one indistinct class) or consisting of mathematicals alone. Later in book Λ, however, only the Prime Mover (and, possibly, the movers of the celestial spheres) are counted as intelligible substances – Aristotle evidently does not share the views presented a bit earlier. Book Z, $1028^{b}20$–27 [trans. Tredennick 1933: I, 315] is more explicit:

> Thus Plato posited the Forms and the 〈mathematicals〉 as two kinds of substance, and as a third the substance of sensible bodies; and Speusippos assumed still more kinds of substances, starting with "the One," and positing principles for each kind: one for numbers, another for magnitudes, and then another for the soul. In this way he multiplies the kind of substance. Some [the followers of Xenocrates, successor to Speusippos as head of the Academy] again hold that the Forms and numbers have the same nature, and that other things – lines and planes – are dependent upon them; and so on back to the substance of 〈the heaven〉 and sensible things.

Even to Aristotle, however, the mathematicals are, or are sometimes, intelligible (though not substances). In *Metaphysics* Z, $1035^{b}31$–$1036^{a}12$ [trans. Tredennick 1933: I, 361–363] we find the following reflections:[10]

> A part, then, may be part of the form (by form I mean essence), or of the concrete whole composed of form and matter, or of the matter itself. But only the parts of the form are

[10] "Essence" corresponds everywhere in the quotation to τὸ τί ἦν εἶναι, cf. note 5; "formula" corresponds to λόγος, cf. p. 485.

> parts of the formula, and the formula refers to the universal; for "circle" is the same as "essence of circle", and "soul" the same as "essence of soul". But when we come to the concrete thing, *e.g.* *this* circle – which is a particular individual, either sensible or intelligible (by intelligible circles I mean those of mathematics, and by "sensible" those which are of bronze or wood) – of these individuals there is no definition; we apprehend them by intelligence [μετὰ νοήσεως] or perception and when they have passed from the sphere of actuality [ἐκ τῆς ἐντελεχείας] it is uncertain whether they exist [εἰσὶν] or not, but they are always spoken of and apprehended by the universal formula. ⟨For⟩ the matter is in itself unknowable [ἄγνωστος]. Some matter is sensible and some intelligible; sensible, such as bronze and wood and all movable matter; intelligible, that which is present in sensible things not *qua* sensible, ⟨such as⟩ the ⟨mathematicals⟩.

Mathematical circles are thus two different things: the word may refer to the universal defined by a formula or λόγος, for instance the one which is quoted in Plato's (or ps-Plato's) *Seventh Letter* [trans. Bury 1929: 533], "that which is everywhere equidistant from the extremities to the centre"; or it may designate a particular specimen, distinct (it seems) from the actual (sensible) drawing but still coming into actual existence and passing away from it (together with the drawing, we must presume) – we may think of one of the two distinct circles that serve in the construction of *Elements* I.1.[11] The final passage "For the matter … mathematicals" could be taken to refer to the problem that the existence of distinct individuals belonging to the same species presupposes (according to Aristotle's normal ontology as reflected in the very term "concrete" – σύνολος, "all together" etc.) that they originate by the imposition of form upon matter; in this reading, Aristotle concludes that the matter (ὕλη) of the distinct circles is unknowable (ἄγνωστος) but intelligible.[12] Alternatively, the individual mathematicals

[11] That the plurality of mathematical circles occurring in the same propositions is meant is also the interpretation of David Bostock [1994: 156] (it seems difficult to find any other). Such particular specimens are of course localizable, and to assume that Aristotle should refer to them might seem to contradict the assertion made in N, 1092^a19–20 – *viz* that mathematicals (including in particular mathematical solids) have no position; but the context (a discussion of generating principles) shows that here the category of mathematicals which coincide with their essence is meant. In K, 1061^a36], indeed, where the practice of geometrical science is dealt with, its objects have at least relative position [θέσις].

[12] This is how the passage is interpreted by Ian Mueller [1971: 163]. Further on (pp. 166*f*), Mueller mentions (accepting the view) that an ancient commentary ascribed to Alexander of Aphrodisias understands the matter in question to be mere extension – cf. also [Heath 1949: 224]. Since *Physics* IV, 209^a21 says explicitly that place (τόπος) is not the matter of anything, the idea is not without problems – in particular because it has just been explained (208^b27–28) that the vacuum which certain thinkers accept is "place deprived of bodies", which shows us that Aristotle's "place" encompasses what we would call "space". To this comes, as pointed out by Thomas Anderson [1969a: 22 n. 59], the difficulty that numbers can hardly be supposed to have extension as their matter (instead, as we shall see in note 29, the observation that numbers and geometrical shapes are always numbers and shapes of *something*, leads him to suggest that the intelligible matter may

themselves are regarded as "the matter", which is "present in" (ὑπάρχουσα) sensible objects (the term for presence having strong connotations of "belonging properly to" and of subordination, which indeed fits matter better than form). The latter reading is strongly supported by the parallel "such as bronze … such as the mathematicals" (οἷον χαλκός … οἷον τὰ μαθηματικά) and by the choice of grammatical case.[13] It is also confirmed by the context – the passage sums up a preceding discussion in which it is stated among other things [trans. Tredennick 1933: I, 355–359] that

> [1035^{a}9] This is why the formula of the circle does not contain that of the segments, whereas the formula of the syllable does contain that of the letters; for the letters are parts of the formula of the form [of the syllable]; they are not matter; but the segments are parts in the sense of matter in which the form is induced. [… 1035^{a}17] For even if the line is divided and resolved into its halves, or if the man is resolved into bones and ⟨sinews⟩ and flesh, it does not follow that they are composed of these as part of their essence but as their matter; and these are parts of the concrete whole, but not of the form, or that to which the formula refers. [… 1035^{a}26] All things which are concrete combinations of form and matter (*e.g*. "the snub" or the bronze circle) can be resolved into form and matter, and the matter is a part of them. [… 1035^{a}32] For this reason the clay statue can be resolved into clay, and the sphere into bronze, and Callias into flesh and bones, and the circle too into segments, because it is something which is combined with matter. For we use the

be indeterminate, unspecified substance).

To modern ears it may sound strange that something is intelligible but unknowable, but the two terms belong at different levels. The former epithet refers to an ontological dichotomy – indeed, "sensible matter" cannot be reached by the senses. The latter, "unknowable", refers to the shared characteristic of everything not submitted to measure and order (thus not least utmost matter); its use can be elucidated by a passage from the *Rhetoric* (1408^{b}21–28, trans. [Freese 1926: 383]) about the form of prose composition: it should not be metrical, nor however without rhythm, "for that which is unlimited (ἄπειρον) is unpleasant and unknowable. Now all things are limited by number, and the number belonging to the form of diction is rhythm, of which the metres are the divisions".

[13] At least three other passages from the *Metaphysics* refer to mathematicals as matter, one obliquely, the others explicitly but in different senses. The first is M, 1078^{a}29–31, where we find that the geometricians are right in claiming that what they discuss is something real (ὄντα); being (τὸ ὄν), indeed, is of two kinds – either entelechy, "full actuality" (which the mathematicals have just been argued not to be) or material (ὑλικῶς). The second is N, 1092^{b}17–18, where it is said in connection with a discussion of the composition of flesh and bone from the elements in a specific ratio (see p. 485) that "clearly, numbers are neither substance nor the cause of forms; the ratio, indeed, is the substance, and the number matter". The third is H, 1045^{a}34–36 [trans. Tredennick 1933: I, 425], "some matter is intelligible and some sensible, and part of the formula is always matter and part actuality; *e.g*., the circle is a plane figure". The last passage clearly corresponds to the view of a genus as a substrate and a kind of quasi-matter which, when receiving *differentiae specificae* functioning as quasi-form, brings forth the more substantial species (*Metaphysics* Δ, 1016^{a}24–32; Z, 1039^{a}24–27), and thus speaks of the universal circle; the ratio, in contrast, appears as a parallel to the individual man, and must thus be seen as an individual ratio between particular numbers (which on their part correspond to the bodily parts).

> same name for the absolute [ἁπλόος] circle and for the particular ⟨ones⟩ [ἕκαστα], since there is no special name for the particular ⟨ones⟩.
>
> [… 1935^{b}7] But the formula of the right angle is not divisible into the formula of an acute angle, but vice versa; since in defining the acute angle we use the right angle, because "the acute angle is less than a right angle." It is the same with the circle and the semicircle; for the semicircle is defined by means of the circle. And the finger is defined by means of the whole body; for a finger is a particular kind of part of a man. Thus such parts as are material, and into which the whole is resolved as into matter, are posterior to the whole […].

Semicircles, line segments and acute angles are certainly mathematicals (and the text points out explicitly not to speak exclusively of the sensible circles etc.). One sense in which mathematicals can be matter is thus as components of other mathematicals (this does not preclude that it may also be possible in other senses, but Aristotle does not tell how). That the reflections are really concerned with mathematicals and not with sensible shapes alone – but with *individual*, not universal mathematicals – is confirmed by a later passage from the book, namely 1036^{b}33-1037^{a}5 [trans. Tredennick 1933: I, 367–369]:[14]

> And with respect to ⟨the mathematicals⟩, why are the formulae of the parts not parts of the formulae of the whole; *e.g.*, why are the formulae of the semicircles not parts of the formulae for the circle? for they are not sensible. Probably this makes no difference; because there will be matter even of some things which are not sensible. Indeed there will be matter in some sense in everything which is not essence or form considered independently, but a particular thing. Thus the semicircles will be parts not of the universal circle but of the particular circles, as we said before [namely in the passage quoted above] – for some matter is sensible, and some intelligible.

Metaphysics M, 1077^{a}33–36 asks in what sense lines (referred to as the primary generators of mathematicals) can be substances.[15] Not by being "some kind of form or shape" (εἶδος καὶ μορφή τις) like the soul, nor as matter, as the body is; indeed, "it certainly does not appear that [anything] can be composed of lines or planes or points", which would be the case if lines were a kind of material substance. Superficially read, the passage looks like a rejection of the view of mathematicals as intelligible matter. Aristotle's own inference, however, is that lines cannot be "material substance" for (potentially animate, and thus sensible) *bodies*, as are flesh and bone for the living being. When summarizing, he concludes (1077^{b}12, trans. [Tredennick 1933: II, 187])

> *(a)* that ⟨(the mathematicals)⟩ are not more substantial than corporeal objects; *(b)* that they

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[14] Probably displaced and originally belonging just after the passages discussed here – see [Bostock 1994: 166].

[15] This follows shortly after a cross-reference to the discussion of the impossible independent existence of mathematicals in book B to which we shall turn imminently. The recurrent cross-referencing between books A, B, Z and M ensures that it is legitimate to combine the evidence they offer.

> are not prior in point of existence [τῷ εἶναι] to sensible things, but only in formula; and *(c)* that they cannot in any way exist in separation.

This is thus a rejection of Pythagorean and related derivations of sensible reality from the mathematicals, and has no bearing on the status of mathematicals as intelligible matter for other intelligible entities (mathematicals, or whatever else might be imagined).

A discussion in book B is part of the same confrontation. Already in book A, $987^b15–18$ [trans. Tredennick 1933: I, 45] we find that the mathematicals not only constitute a separate category along with forms and sensible things for Plato, but that they stand between (μεταξύ) these, differing "from sensible things in being eternal and immutable, and from the Forms in that there are many similar 〈of them〉, whereas each Form is itself unique". In book B, 997^b13-998^a19 [trans. Tredennick 1933: I, 113–117] Aristotle makes it clear that he finds the notion absurd:

> […] if anyone posits Intermediates distinct from Forms and sensible things, he will have many difficulties; because obviously not only will there be lines apart from both Ideal [τ' αὐτὰς] and sensible lines, but it will be the same with each of the other classes. Thus since astronomy is one of 〈these〉 [the mathematical sciences], there will have to be a heaven besides the sensible heaven, and a sun and moon, and all the other heavenly bodies. But how are we to believe this. Nor is it reasonable that the heaven should be immovable; but that it should move is utterly impossible.[16] It is the same with the objects of optics and the mathematical theory of harmony; these too, cannot exist apart from sensible objects. Because if there are intermediate objects of sense and sensations, clearly there will also be animals intermediate between the Ideal animals and the perishable animals.
>
> One might also raise the question with respect to what kind of objects we are to look for these sciences. For if we are to take it that the only difference between mensuration and geometry is that the one is concerned with things which we can perceive and the other with things which we cannot, clearly there will be a science parallel to medicine (and to each of the other sciences), intermediate between Ideal medicine and the medicine which we know. Yet how is this possible? for then there would be 〈certain〉 healthy things apart from those which are sensible and from the Ideally healthy. Nor, at the same time, is it true that mensuration is concerned with sensible and perishable magnitudes; for then it would perish as they do. Nor, again, can astronomy be concerned with sensible magnitudes or with this heaven of ours; for as sensible lines are not like those of which the geometrician speaks (since there is nothing which is straight and curved in that sense; the [sensible] circle touches the ruler not at a point but [along a line] as Protagoras used to say in refuting the geometricians), so the paths and orbits of our heaven are not like those which astronomy discusses, nor have the 〈points〉 [σημεῖα] of the astronomer the same nature [φύσις] as the stars.
>
> Some, however, say that these so-called Intermediates between Forms and sensible objects do exist: not indeed separately from the sensibles, but in them. It would take too long to consider in detail all the impossible consequences of this theory, but it will be sufficient to observe the following. On this view it is not logical that only this should

16 Namely because movement belongs solely with the sensible, and thus not with a merely intelligible intermediate heaven.

> be so; clearly it would be possible for the Forms also to be in sensible things; for the same argument applies to both. Further, it follows necessarily that two solids must occupy the same space; and that ⟨they⟩ [the intermediates] cannot be immovable, being present in sensible things, which move. And in general, what is the object of assuming that Intermediates exist, but only in sensible things? The same absurdities as before will result: there will be a heaven besides the sensible one, only not apart from it, but in the same place; which is still more impossible.

Various observations beyond the rejection of Intermediates can be made regarding this passage. Firstly, that much of the argument is built around "the more physical of the mathematical sciences" (τὰ φυσικώτερα τῶν μαθημάτον– *Physics* II, 194ᵃ8): astronomy, optics and harmonics. Since Aristotle does ascribe to these a particular status among the *mathemata*, he might have discarded them as not directly relevant, which he fails to do. Secondly, the parallel to medicine strengthens the impression that the relation between the mathematical sciences (and thus also the mathematicals) and the sensible world is understood to be fundamental. Thirdly, that Aristotle feels entitled to assume that the presence of intelligible mathematicals *in* sensibles assumed by some of those against whom he argues is meant spatially. *Perhaps* we should understand this "presence in" as an identification of the intelligible sphere with the actual surface of the bronze sphere. That such an identification is in any case *not* accepted by Aristotle is clear from the endorsement of what Protagoras says about the actual comportment of lines and circles understood as external limits of sensible rulers and wheels. The relation between mathematicals and sensibles, though indubitably pivotal, is no such simple connection. Nor is it a Platonic "participation" (μέθεξις), since this is said in book A, 987ᵇ13–14 to be nothing but a formulation in other words of the Pythagorean view of reality as an imitation (μίμησις) of numbers[17] (deprived moreover of clear meaning), and in 991ᵃ22–23 to be "empty talk and poetical metaphors".

These scattered observations should allow us to approach the main investigation of the possible existence of mathematicals as unchangeable and eternal substance, which is undertaken in *Metaphysics* M, chapters I–III – the chapters on which Edward Hussey's fairly recent discussion [1992] of Aristotle's view of the mathematicals concentrates. Many points made in these chapters repeat what we have already discussed (and a few were

[17] In N, 1090ᵃ21–24 [trans. Tredennick 1933: II, 277] we find the more familiar version of the Pythagorean doctrine – namely that the Pythagoreans,

> observing that many ⟨affections⟩ (πάθη) of numbers apply to sensible bodies, assumed that real things are numbers; not that numbers exist separately, but that real things are composed of numbers. But why? Because the ⟨affections⟩ of numbers are to be found in a musical scale, in the heavens, and in many other connections.

Since the account in book A is specific and refers to a precise terminology("μίμησις"), we may take it to render something which at least some Pythagoreans would sometimes say, if not necessarily to express a generally held Pythagorean view of the nature of number and material reality.

already quoted) – thus the (cross-referenced) dismissal of the Platonist and similar theses about the status of Forms and mathematicals as independent substances either separate from sensibles or to be found "in" them.

Before discussing what is said we should take note of what is *not* said. Even in these chapters, the mathematicals are never stated to be substances;[18] however large the range of categories that on one or the other occasion are designated thus (be it in a secondary or lesser sense, as with universals, forms and matter), this range is not large enough to encompass numbers, ratios and circles. This is made explicit (concerning the special case of lines) in chapter II, 1077^{a}32–36 [trans. Tredennick 1933: II, 185–187] – (cf. p. 489):

> […] body [σῶμα] is a kind of substance, since it already in some sense possesses completeness (τέλειος); but in what sense are lines substances? Neither as being a kind of form or shape, as perhaps the soul is, nor as being body, like the body; for it does not appear that anything can be composed of either lines or of planes or of points, whereas if they were a kind of material substance it would be apparent that things can be so composed.

The refusal to classify the mathematicals as substances even in the most diluted sense already locates them at the same level as the affections (πάθη) of substances proper, that is, of entities that have full and separate existence. And the text indeed goes on as follows (1077^{b}1–17, trans. [Tredennick 1933: II, 187]):

> Let it be granted that [lines etc.] are prior in formula; yet not everything which is prior in formula is also prior in substantiality. Things are prior in substantiality which when separated have a superior power of existence; things are prior in formula from whose formulae the formulae of other things are compounded. And these ⟨do not apply at one time⟩ [οὐχ ἅμα ὑπάρχει]. For if ⟨there are no affections⟩ apart from substances, such as "moving" or "white," ⟨ ⟩ "white" will be prior in formula to "white man," but not in substantiality; for it cannot exist in separation, but always exists conjointly with the concrete whole [ἅμα τῷ συνόλῳ ἐστίν] by which I mean "white man." Thus it is obvious that neither is the result of ⟨removal⟩ [19] prior, nor the result of adding ⟨ ⟩ [πρόσθεσις[20]] posterior – for the expression "white man" is the result of adding ⟨ ⟩ to "white".

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[18] The only exception I have found is *Metaphysics* M, 1092^{b}13, where *the ratio* – the least substantial of Aristotle's mathematicals, we might say – is said to be the οὐσία and number its matter; but this is probably too particular to allow any conclusion beyond the ensuing refusal of substantial status for number.

[19] ' Αφαίρεσις, "taking away". This is the term that is normally translated "abstraction". In modern language, however, the main sense of "abstraction" is that which remains when certain features have been removed (mentally). In order not to read "darkness" where "light" is meant (removal of light leaves darkness!) the translation must be literal.

[20] Literally "putting unto", the opposite of "taking away".

> Thus we have sufficiently shown *(a)* that ⟨(the mathematicals)⟩ are not more substantial than corporeal objects;[21] *(b)* that they are not prior in point of existence to sensible things but only in formula; and *(c)* that they cannot in any way exist in separation. And since we have seen that they cannot exist in sensible things, it is clear that either they do not exist at all, or they exist only in a certain way, and therefore not absolutely [ἁπλῶς] for "exist" has several senses.

This does not assert that there is no difference between mathematicals and properties like colour, and probably it is not meant to convey that message; but if the argument is to possess any validity it must presuppose that they belong to ontologically comparable categories.

Chapter III gets closer to positive assertions about the mathematicals. Its beginning reads thus (1077^b18–1078^a9, trans. [Tredennick 1933: II, 187–191]):

> The general propositions in mathematics are not concerned with objects which exist separately apart from magnitudes and numbers; they are concerned with magnitudes and numbers, but not with them as possessing magnitude or being divisible.[22] It is clearly possible that in the same way propositions and logical proofs may apply to sensible magnitudes; not *qua* sensible, but *qua* ⟨being such⟩ [ᾗ τοιαδί]. For just as there can be many propositions about things merely *qua* movable, without any reference to the ⟨suchness⟩ [τῶν τοιούτων] of each one or their ⟨accidents⟩ [τῶν συμβεβηκότων], and it does not necessarily follow from this either that there is something movable which exists in separation from sensible things or that there is a distinct movable nature in sensible things; so too there will be propositions and sciences which apply to movable things, not *qua* movable but *qua* corporeal only; and again *qua* planes only and *qua* lines only, and *qua* divisible, and *qua* indivisible but having position, and *qua* indivisible only. Therefore since it is true to say in a general sense not only that things which are separable but that things which are inseparable exist, *e.g.*, that movable things exist, it is also true to say in a general sense that ⟨the mathematicals⟩ exist, and ⟨indeed such as they [the mathematicians] say⟩. And just as it is true to say generally of the other sciences that they deal with a particular subject – not with that which is accidental to it (*e.g.* not with "white" if "the healthy" is white, and the subject of the science is "the healthy"), but with that which is the subject

21 The whole passage is indeed directed against the Pythagorean, Platonic and related persuasions.

22 As examples of such "general propositions" we may first of all think of Euclid's "common notions"; but *Posterior Analytics* I (74^a18–25, trans. [Tredennick & Forster 1960: 51]) offers a more sophisticated instance, which is indeed taken by Heath [1949: 223] as the clearest example of the category:

> […] the law that *proportionals alternate* might be supposed to apply to numbers *qua* numbers, and similarly to lines, solids and periods of time; as indeed it used to be demonstrated of these subjects separately. It could, of course, have been proved of them all by a single demonstration, but since there was no single term to denote the common quality of numbers, lengths, time and solids, and they differ in species [εἶδος]from one another, they were treated separately; but now the law is proved universally; for the property did not belong to them *qua* lines or *qua* numbers, but *qua* possessing this special quality which they are assumed to possess universally.

> of the particular science; with the healthy if it treats of things *qua* healthy, and with man if *qua* man – so this is also true of geometry. If the things of which it treats are accidentally sensible, although it does not treat of them *qua* sensible, it does not follow that the mathematical sciences treat of sensible things – nor, on the other hand, that they treat of other things which exist independently apart from these.
>
> Many ⟨accidents⟩ are essential [καθ' ἁυτὰ] properties of things possessing a particular characteristic; *e.g.*, there are ⟨affections⟩ [πάθη] peculiar to an animal *qua* female or *qua* male, although there is no such thing as female or male in separation from animals.[23] Hence there are also [affections] which are peculiar to things merely *qua* lines or planes.

This is remarkable not only for stating the legitimacy of an investigation which leaves out certain characteristics as irrelevant and concentrates on those which are the concern of the science in question *as if* these could occur in isolation but also for a remarkable symmetry between types of characteristics. In the "textbook version" of Aristotle's ontology, genuine substances come about when form is impressed upon matter; this provides these substances with necessary attributes. Besides these, they may receive other attributes which, however, are *accidental*; this is the basis for the distinction between explanations based on *causes*, answers to questions "why", and a mathematical description à la Archimedes or Ptolemy. In the present passage, as we see, it is supposed to depend on the perspective of the investigation – whether a natural object is examined by physics or geometry – which characteristics are accidental and which not. When contemplated by a geometer, the sensibility of a bronze sphere – its most fundamental characteristic according to the standard ontology – becomes an "accident".[24]

After a passage treating of the exactness of various types of knowledge (to which we shall return on p. 499), Aristotle closes the deliberations about the removal of that which is accidental according to the perspective of the science in question with these words (1078^a14–31, trans. [Tredennick 1933: II, 191–193]):

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[23] Richard Hope [1960: 275] understands this period differently:

> So there are many traits that things have because they are what they are: a living being has the attributes of being female or male, yet there is no female or male being separate from living beings.

Read in this way (which seen in isolation seems to agree better with the Greek text), the point would thus be analogous to the observation that bodies may have different shapes, but being bodies they need to have *some* shape. However, the following period (which Hope translates in the same vein as Tredennick, "there are peculiar attributes that things have when taken only as lengths or as planes") agrees better with Tredennick's reading, whose main idea is also followed by Julia Annas [1976: 95].

[24] As a matter of fact, the same dependence on the perspective of what is accidental is asserted in book A, 980^b20. Here it is accidental that Socrates is a man if the observation is made by a physician. The incipient shift of the distinction necessary/accidental from ontology to epistemology is thus no mere side-effect of the need to rationalize the existence of mathematics in *Metaphysics* M.

> The same principle applies to both harmonics and optics, for neither of these sciences studies objects *qua* sight or *qua* sound, but *qua* lines and numbers; yet the latter are affections peculiar to the former. The same is also true of mechanics.
>
> Thus if we regard objects independently of their ⟨accidents⟩ and investigate any aspect of them as so regarded, we shall not be guilty of any error on this account, any more than when we draw a diagram on the ground and say that a line is a foot long when it is not; because the error is not in the premises. The best way to conduct an investigation in every case is to take that which does not exist in separation and consider it separately; which is just what the arithmetician or the geometrician does. For man, *qua* man, is one indivisible thing;[25] and the arithmetician assumes man to be one indivisible thing,[26] and then considers whether there is any ⟨accident⟩ of man *qua* indivisible. And the geometrician considers man neither *qua* man nor *qua* indivisible, but *qua* something solid [στερεόν]. For clearly ⟨what⟩ would have belonged to "man" even if man were somehow not indivisible can belong to man irrespectively of his humanity or indivisibility. Hence for this reason the geometricians are right in what they maintain, and treat of ⟨being things⟩ [ὄντα]; ⟨and being things⟩ exist. For ⟨being is twofold⟩, either complete reality or matterlike.[27]

A similar point of view is expressed in more detail in the same terminology in book K, 1061^{a}29*ff* [trans. Tredennick 1933: II, 67–69]:

> […] the mathematician makes a study of ⟨removals⟩ (for in his investigations he first ⟨removes⟩ everything that is sensible, such as weight and lightness, hardness and its contrary, and also heat and cold and other sensible contrarieties, leaving only quantity and continuity – sometimes in one, sometimes in two and sometimes in three dimensions – and their affections *qua* quantitative and continuous, and does not study them with respect to any other thing; and in some cases investigates the relative positions of things and the properties of these, and in others their commensurability or incommensurability, and in others their ratios; yet nevertheless we hold that there is one and the same science of all these things, viz. geometry […].

Two passages from other works should be drawn in at this point. One is *Posterior Analytics* I, 74^{a}39–74^{b}4 [trans. Tredennick & Forster 1960: 53]. In connection with the question how to attain universal knowledge when we have so far only achieved knowledge of particular cases (cf. note 22), Aristotle observes that

> the property of having angles equal to the sum of two right angles will apply to "bronze isosceles triangle"; and it will still apply when "bronze" and "isosceles" are removed

– but not if "triangle" is removed. We notice that here the matter "bronze" and the

[25] If divided (mentally or by a butcher's saw), indeed, man is no longer man but a plurality of bodily parts (either conceptually or in reality). Cf. p. 485.

[26] Namely when identifying a collection of n persons with the number n. We remember the (Pythagorean and later) definition of number as a "collection of units".

[27] Cf. note 13.

geometrical property "isosceles" are treated in full parallel, in agreement with the "matterlike" existence of mathematical properties and with the discussion of mathematicals as "intelligible matter" in *Metaphysics* Z.

The other is *Physics* II, 193^{b}24–36 [trans. Wicksteed & Cornford 1929:1,119], which is often mentioned as a parallel to the discussion of abstraction or removal in *Metaphysics* M. Here we read that

> we have next to consider how the mathematician differs from the ⟨natural philosopher⟩ [φυσικός]; for natural bodies have surfaces and occupy spaces, have lengths and present points, all which are subjects of mathematical study. And then there is the connected question whether astronomy is a separate science ⟨or part of natural philosophy⟩; for if the student of nature is concerned to know what the sun and moon are, it were strange if he could avoid ⟨their accidents⟩; especially as we find that the writers on nature have, as a fact, discoursed on the shape of the moon and sun and ⟨on whether the earth and the cosmos⟩ is spherical or otherwise.
>
> ⟨The mathematician, then, also deals with these [i.e., surfaces, spaces, lengths, points], but not⟩ *qua* boundaries of natural bodies, nor ⟨does he consider the accidents indicated as accidents of such substances⟩. Therefore he separates them; for they are ⟨separable according to thought from motions, and it makes no difference, nor does separation lead to any falsity⟩.

As we should probably expect in a work dealing centrally with natural philosophy ("physics"), the attributes that belong to "nature" and those regarded by mathematics are treated on a less equal footing here than in *Metaphysics* M – appurtenance according to nature seems to be ontological, whereas the mathematicians' abstraction is epistemological, only performed "according to thought" (τῇ νοήσει)– and it is no longer produced by "removal" but by "separation"[28] From the absolute perspective of this work, the properties regarded by the mathematician – in geometry, the shape – remain "accidents". Here, geometrical shapes remain properties of physical bodies, properties that can be isolated from these by a process of mental separation, and they have no genuinely independent existence.

We must conclude that this is the general view of Aristotle regarding the status of the mathematicals: geometrical shape is a property of bodies, as number is a property of collections of units; even ratio is a ratio between numbers of something or between quantities, and thus a property of a relation between such concrete collections or quantities – as is indeed stated in *Metaphysics* N, 1092^{b}18–23. The only vacillation is between the asymmetrical position of the *Physics* (which may also be that of the early *Metaphysics* N, though that is not sure) and the symmetrical stance of *Metaphysics* M:

[28] This difference of vocabulary is hardly random: a proper elimination of characteristics that are regarded as essential might sound too strange. Instead, the *Physics* (which according to cross-references is likely to be earlier than the *Metaphysics*-M passages) makes use of a terminology closer to the discussion of the Platonic stance.

(i) the *Physics* distinguishes between an ontologically based suppression of the accidents of shape which may be undertaken by the natural philosopher, and the mental elimination of “physical” properties undertaken by the mathematician; (ii) *Metaphysics* M, in contrast, grounds removal (abstraction proper) neither on an ontological distinction between natural necessity and accidents of shape or number nor on individual thought, but on the distinctive view or approach *of the investigating science* – somehow intermediate between absolute ontological necessity and (legitimate but still arbitrary) personal choice.

In terms of the distinction of *Metaphysics* Z, 1035^{b}33–1036^{a}12 (above, p. 486), this understanding of the mathematical as properties of substances would first of all concern the individual circles and their kin, which would be properties of the drawn individual circles. The circle in the sense of λόγος or “what it is to be a circle” would then be a universal, similar to the “secondary substances” of the *Categories*.

This seems a reassuring conclusion,[29] and apart from the distinction between the different approaches to the nature of the abstraction process it is not very different from what earlier workers have found and stop at.[30] But second thoughts will show us that it is insufficient, as can be seen from this passage from *Metaphysics* B (cf. fuller quotation on p. 490):

> Nor, again, can astronomy be concerned with sensible magnitudes or with this heaven of ours; for as sensible lines are not like those of which the geometrician speaks (since there is nothing which is straight and curved in that sense; the [sensible] circle touches the ruler not at a point but [along a line] as Protagoras used to say in refuting the geometricians), so the paths and orbits of our heaven are not like those which astronomy discusses, nor have the points of the astronomer the same nature as the stars.

If the circular and the straight are properties of the boundaries of physical bodies, merely regarded in (one or the other kind of) abstraction from the physical properties of these bodies, how can that which a circle *regarded mathematically* has in common with a touching straight line be a mere point and that which they have in common *without removal* be a whole line segment?

[29] Or perhaps not fully reassuring, since it can be argued to entail that the “abstraction of the mathematician ends up not with pure quantity but with quantified substance” [Anderson 1969a: 17]. Therefore, as the sensible circle has the sensible bronze circle as its substrate, the mathematical circle (at least the individual circle) must have unspecified physical substance as its substrate and, in this sense, its matter. This would be a parallel to the view of a genus as quasi-matter and the *differentiae specificae* as quasi-form (cf. note 13), and would therefore present us with no difficulties if only Aristotle had ever hinted at something of the kind. Unfortunately he never does but leaves it to Thomas Aquinas to make the point – see [Anderson 1969b]. Cf. below, note 32.

[30] Annas [1976: 29–33] even observes that the view of *Metaphysics* M is independent of the “psychological account” of the *Physics*, but being trapped by a modern understanding of her translation “abstraction” she fails to grasp the nature of the difference.

The dilemma is not restricted to mathematics; it has a close parallel in the difficulty which we encounter if we interpret an Aristotelian "nature" through the modern notion of a natural law. We may look at this passage from the *Politics* (1253^{a}2–5, trans. [Rackham 1932: 9]):

> […] therefore it is clear that the city-state is ⟨by nature⟩, and that man is by nature a political animal, and a man that is citiless by nature and not merely by fortune is ⟨truly either a petty man or beyond measure⟩.

An Aristotelian nature is, indeed, a final cause, a "cause in the sense of 'that for which' ": that after which the entity for which it is a cause strives. It is not necessarily realized in the particular case. This is explained in *Physics* II (199^{a}7–199^{b}26, trans. [Charlton 1970: 40–42]):

> […] The "or something", then, is present in things which are and come to be due to nature.
>
> Again, where there is an end, the successive things which go before are done for it. As things are done ⟨by art⟩ (πράσσω), so they are by nature such as to be, and as they are by nature such as to be, so they are done ⟨by art⟩, if there is no impediment. Things are done ⟨by art⟩ for something. Therefore they are by nature such as to be for something. Thus if a house were one of the things which come to be due to nature, it would come to be just as it now does by the agency of art; and if things which are due to nature came to be not only due to nature but also due to art, they would come to be just as they are by nature. The one, then, is for the other. In general, art either imitates the works of nature or completes that which nature is unable to bring to completion. If, then, that which is in accordance with art is for something, clearly so is that which is in accordance with nature. […].
>
> […] And since nature is twofold, nature as matter and nature as form, and the latter is an end, and everything else is for the end, the cause ⟨in the sense of "that for which"⟩ must be the latter.
>
> Mistakes occur even in that which is in accordance with art. Men who possess the art of writing have written incorrectly, doctors have administered the wrong medicine. So clearly the same is possible also in that which is in accordance with nature. If it sometimes happens over things which are in accordance with art, that that which goes right is for something, and that which goes wrong is attempted for something but miscarries, it may be the same with things which are natural, and monsters may be ⟨failures⟩ [αμάρτημα] at that which is for something. […]
>
> […]
>
> […] A thing is due to nature, if it arrives, by a continuous process of change, starting from some principle in itself, at some end. Each principle gives rise, not to the same thing in all cases, nor to any chance thing, but always to something proceeding towards the same thing, if there is no impediment. […] but when a certain thing comes to be always or for the most part, it is not a concurrent happening, nor the outcome of luck. Now with that which is natural it is always thus if there is no impediment.

Slightly later (200^{a}15*f*) it is then stated that the "necessary appears in mathematics and in the things which come to be in accordance with nature, in a parallel fashion". It should thus be legitimate to think of the mathematical properties of things in parallel to those properties that constitute their "nature": not something which is necessarily so but

as something which is so to the extent "there is no impediment".[31] For further illustration, we may turn to the passage from *Metaphysics* M which was omitted between two quotations on p. 494 (1078^{a}9–13, trans. [Tredennick 1933: II, 191]):

> And in proportion as the things which we are considering are prior in formula and simpler, they admit of greater exactness (ακριβής); for simplicity implies exactness. Hence we find greater exactness where there is no magnitude, and the greatest exactness where there is no motion; or if motion is involved, where it is primary, because this is the simplest kind; and the simplest kind of primary motion is uniform motion.

Since it has just been stated (1077^{b}1, above, p. 492) "that [lines etc.] are prior in formula" without being thereby "prior in substantiality", it appears that mathematical properties of substances may indeed surpass "in exactness" those substances of which they are properties, just as these may be surpassed by their forms in perfection. Other sciences should study *the nature* of their object, not those shortcomings of individual specimens that are produced by accidental impediments that cripple this nature; in the same way,

31 This analogy between form proper and mathematical characterization is probably the explanation of what Hussey [1992: 125 n.40] regards as a "puzzling and isolated passage", namely *Posterior* Analytics I, 79^{a}6–10 [trans. Tredennick & Forster 1960: 91]:

> Of this kind [viz, studied by more than one science] are all objects which, while having a separate substantial existence, yet exhibit certain specific forms. For the mathematical sciences are concerned with forms; they do not confine their demonstrations to a particular substrate. Even if geometrical problems ⟨treat of⟩ a particular substrate, at least they do ⟨not do so *qua* treating of a substrate⟩.

Obviously, since the pertinence of "more than one science" is a consequence of the possession of the "specific forms" referred to, these "forms" cannot be the nature that give the objects in question their "substantial existence"; they (and thus the "forms" with which the mathematical sciences are concerned) must be something which shares fundamental characteristics of forms without being forms in the ontological sense – they certainly cannot be "substantial forms". The passage should be read in the light of *Metaphysics* M rather than in continuation of *Physics* II.

It may sound odd that the mathematicals are spoken of here as forms and apparently elsewhere as matter. Since they are neither form nor matter in the fundamental sense, however, the contradiction is not very different from what befalls universals: at times they are determined by form in the sense of essence or formula (*Metaphysics* Z, 1035^{b}31, quoted above, p. 486), at times they function as some kind of matter (above, note 13).

⟦According to a passage in *De caelo* II.14 (297b 21–23, trans. [Guthrie 1939: 251),

> either then [the earth] *is* spherical, or at least it is natural for it to be so [ἢ οὖν ἐστὶ σφαιροειδές, ἢ φ ύσει γε σφαιροειδές], and we must describe each thing by that which is its natual goal or its permanent state, not by any enforced or unnatural characteristics [τοιοῦτον εἶναι ὃ φύσει βούλεται εἶναι καὶ ὅ ὑπάρχει, ἀλλα μὴ ὅ βίᾳ καὶ παρὰ φὺσιν]"

This passage, unfortunately overlooked when the article was written, fully confirms the seemingly controversial thesis that mathematicals, though properties of existing individual things, are *aims* for these things just as forms are – aims which may surpass in exactness what is actually attained because of intervening impediments.⟧

mathematics should deal with the mathematicals in their exactness (which condition is the *sine qua non* for their being intelligible), not impeded by the shortcomings of the sensible substances of which they are properties.[32]

We may regard this as an implied acceptance of the separate existence of ideal mathematicals; but we must recognize that this was not how Aristotle saw things, nor apparently the readers of his epoch.

Euclid's postulates: counterfactual existence claims?

The preceding scrutiny of Aristotle has shed some indirect light on the Pythagorean and Platonic views of the nature of the mathematicals. We might go further in this direction and look at later writings belonging to the Platonic and neo-Pythagorean currents – beginning with Nicomachos, according to whose *Introduction to Arithmetic* (I.VI, trans. [Bertier 1978: 59*f*]) that number which orders everything by nature in the cosmos is able to do so because it is founded upon "the number pre-existing in the thought [διάνοια] of the creator God", and going on with other writers at various levels. One reason for not proceeding in this direction (apart from limitations of space) is that this esoteric number and its associates have little to do with the mathematicals that were treated in mathematics; as Aristotle observes in *Metaphysics* N, 1090^{b}27–35 [trans. Tredennick 1933: II, 281] about the particular "Ideal numbers", "no mathematical theorem applies to them, unless one tries to interfere with the principles of mathematics and invent particular theories of one's own"; further, those who invented "two kinds of number, the Ideal and the mathematical as well, neither have explained nor can explain in any way how mathematical number will exist and of what it will be composed".

If we are interested in understanding ancient *mathematics*, it may therefore be more illuminating to confront the results of the preceding with the *Elements* – that ancient mathematical treatise which comes closest to the model for scientific work which Aristotle delineates in the *Posterior Analytics*. The point on which we shall focus is the set of postulates, "requests" (ed. [Heiberg 1883: I, 8], trans. [Heath 1926: 154]):

[1] ⟨It is requested⟩ to draw a straight line from any point to any point,
[2] ⟨and⟩ to produce a finite straight line continuously in a straight line,
[3] ⟨and⟩ to describe a circle with any centre and distance,
[4] ⟨and⟩ that all right angles are equal to one another,
[5] ⟨and⟩ that, if a straight line falling on two straight lines make the interior angles on the same side less than two right angles, the two straight lines, if produced indefinitely, meet on that side on which are the angles less than the two right angles.

32 We notice that this eliminates Anderson's dilemma as described in note 29. It also shows the nullity of Arthur Madigan's protest [1999: 60]: "Does Aristotle think, then, that geometry is an exact science because its objects are exactly as they are defined to be, but that they are exactly as they are defined to be because that is how geometers think of them?".

There is no doubt that definitions were familiar in mathematics already in Plato's and Aristotle's times; at least one of Euclid's common notions is also quoted by Aristotle as an example of his category of axioms. It is much less certain how far back the postulates should be dated, and how they relate to Aristotle's notions of hypotheses and postulates.[33] I shall therefore leave the question of chronology and inventor aside and just argue from the postulates as they are found in the text.[34]

A feature which as far as I have noticed has never been emphasized is that postulates 2–3 and 5 are counterfactual according to ancient standard cosmologies – including Aristotle's.[35] If the cosmos is a sphere with a finite radius, it will still be possible to connect any two points with a straight line, since all points will have to fall within the sphere. But a line going from one pole of the firmament to the other cannot be produced; if we chose a point with a small distance δ from the firmament as our centre and $2R–2\delta$ as radius (R being the radius of the firmament and cosmos), then our attempted circle will be a short arc and no "figure" (something *enclosed* by a boundary or boundaries – def. 14) – or, if we take the firmament as part of the boundary, then two circles of the kind will not meet, and the demonstration of prop. 1 fails. Even the fifth postulate becomes untenable – actually, what we get (presupposing the adequate metric) is one of Felix

33 See the convenient summary in [McKirahan 1992: 133–137]. To this may be added, however, that the second postulate seems to have been known to Aristotle in a formulation close to what we find in the *Elements* – *Physics* III, 207b29–31 [trans. Hardie & Gaye 1930] explains that mathematicians "do not need the infinite and do not use it 〈but〉 only that the finite straight line may be produced as far as they wish", using the same verb (περαίνω) and form as Euclid.

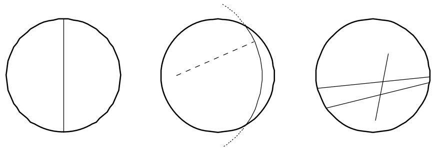
34 More precisely, found in the Heiberg text and in most of the manuscripts. Some medieval manuscript traditions include a supplementary postulate, namely that two lines cannot include a space – thus "Adelard I", ed. [Busard 1983: 32]). Others make this a common notion – thus the translation due to Gerard of Cremona [ed. Busard 1984: 3]. The former variant is referred to by Simplicios in his commentary as quoted by al-Nayrīzī [ed., trans. Besthorn & Heiberg 1893: I, 25], who adds that it is not found in old manuscripts; the latter was known to Proclos (*In primum Euclidis Elementorum librum commentarii* 196[21–23], trans. [Morrow 1970: 154]), who points out that this is no notion common to several mathematical sciences but exclusively geometric.

35 Simplicios (as quoted by al-Nayrīzī [ed., Latin trans. Besthorn & Heiberg 1893: I, 15]) comes so close that he would certainly have mentioned the cosmological problem if it had been familiar or had come to his own well-trained Aristotelian mind. In his commentary to the principles of the *Elements* he refers both to the common-sense protest that one cannot continue a line over the sea (apparently thinking on drawings made on the Rhodian or some other shore) and the philosophical objection that "the infinite does not exist", ascribing both to what is "commonly held" (the philosophical objection is of course somewhat off the point, since the postulate does not ask for this actual but only for the potentially infinite – cf. also the passage from *Physics* III quoted in note 33).

Even Sextus Empiricus fails to mention the problem in his fairly extensive and well-informed *Adversus geometras* [ed., trans. Bury 1949], even though he seems to subscribe to the standard cosmology (taking it in III.16 to be blatantly false that "the earth flies along").

Klein's models for non-Euclidean geometry. The closed cosmos cannot promise us that the prolongation, the circle and the intersection of which the three postulates speak exist however much we request them to do so.

This can be understood in one of three ways. We may assume that Euclid the mathematician rejected the prevalent cosmology of the philosophers, or followed a philosophy which held the cosmos to be without limit or to be surrounded by an infinite void; we may look at the list of postulates as part of something like a Wittgensteinian language game – that is, a network of concepts etc. inextricably linked to a *particular* practice, namely that of geometry; and we may try to make sense of it within the Platonic or Aristotelian framework.



Postulates 2–3 and 5 related to a finite cosmos.

Since we know nothing of Euclid as a person, the first possibility cannot be excluded; but this would at most explain why he wrote as he did, not why his work was so widely accepted and called forth no objections from later commentators on this account – neither from Ptolemy, who according to Proclos was critical of taking the fifth postulate as a postulate, nor from Proclos the Platonist himself, nor from Simplicios the Aristotelian or Sextus Empiricus the scepticist antagonist of teachers and doctrines. We may also wonder why none of those who argued for one or the other limitless model seems to have appealed to the geometrical postulates.[36] We may add that the most renowned infinite cosmology of the Hellenistic Age – that of Epicuros, the only one to be held by a famous philosophical school – was coupled to an understanding of geometry that was probably much too naive to appeal to anybody versed in mathematics. Euclid may have been among those who, in the words of Sextus Empiricus [trans. Bury 1949: 3] conjectured Epicuros' rejection of the liberal arts to have been

> a way of covering up his own lack of culture (for in many ways Epicurus stands convicted of ignorance, and even in ordinary converse his speech was not correct).

Somewhat more plausible is that Euclid would have agreed with those among the Stoics who believed not only in an extracosmic void but in one of infinite extension.[37]

[36] See [Sorabji 1988: 125–141] and [Grant 1981: 17–23, 105–108]. Instead, repeated appeal is made to Archytas's common-sense argument that somebody at the outer limit of a limited world should be able to stretch his arm beyond the limits unless he was impeded by something outside the limit.

[37] According to Plutarch (*De stoicorum repugnantiis*, ed. [Baldassari 1976: 127]), this was Chrysippos's opinion – apparently not shared by, e.g., Posidonios [Sorabji 1988: 129].

It should be mentioned that an analysis of the epistemological presuppositions in Euclid's *Optics* suggests the author to be closer to the Stoic than to other persuasions [Incardona 1996: 56–60].

Remain the Wittgensteinian (or, better perhaps, "naive-Wittgensteinian") interpretation, and the correlation with Aristotelian and Platonic views.

An appeal to the notion of a language game is less out of the way than it might seem at first (at least to those who know the idea best through its use in postmodernist philosophy as an expression of radical relativism). It is an old observation that several of Euclid's definitions define little if read out of context. To explain a point as "that which has no part" might identify it, for instance, with Parmenides's, Speusippos's or Plotinus's *One* or with Allah (as was indeed done by certain medieval Islamic theologians) – or with the indivisible unity of Greek arithmetic, exemplified by man seen by the arithmetician (see the above quotation from *Metaphysics* M, 1078^{a}24, p. 495). But if we already know that we are practising geometry, it becomes meaningful to explain that a dot (be it στιγμή, "a mark", be it σημεῖον, "a sign") is henceforth a dot so small that it has no parts, and that a stroke (γραμμή) is henceforth presupposed to be a length so narrow that it has no breadth at all; as Aristotle points out (*Topics* VI, 143^{b}28–29), this definition presupposes that there are lengths without as well as lengths provided with a width. We may add – this may perhaps be what Aristotle proposes in *Physics* III, 207^{b}29–33 after the apparent citation of the second postulate - that for purposes of proof, the scale of figures may always be changed so as to remain within the existing world. However, if this was Aristotle's own way to get rid of the dilemma, none of the commentators seems to have taken note of it

We may look at the postulates in the same light, and say that they provide the foundation that is indispensable if we want to play the game of geometry (that is, play it in agreement with the intuitions that are either more or less inborn or produced by a century's professional practising of the discipline).[38] Since it is doubtful whether Aristotle knew the postulates as a closely knit group, they seem indeed to have been introduced stepwise as things which geometers had to require if their game was to be played.

In the case of the fifth postulate, it is rather obvious from the sources that this is what had happened. In *Prior Analytics* II, 64^{b}34–65^{a}9 [trans. Tredennick 1938: 485–487], Aristotle discusses circular reasoning in deductive systems – proving A from B, and B from Γ, even though Γ is itself only accepted because it is a consequence of A. As an example he refers to

> those persons who […] think that they are drawing parallel lines; for they do not realize that they are making assumptions which cannot be proved unless the parallel lines exist.

The use of the present tense "do" (ποιοῦσιν)shows that this was still a flaw of geometry

[38] In Cicero's words (*Academic*a II.116, trans. [Rackham 1933: 617]), they are "first principles of mathematics which must be granted before [the geometricians] are able to advance an inch". Simplicios answers similarly to the "commonly held" objections (see note 35).

as practised at least by some of Aristotle's contemporaries. Aristotle also discloses the way out: to take as an axiom (αξιόω, 64^b38f) that which is proposed; but since he does not tell us that this is done in geometry, nor a *fortiori* how the axiom or postulate should run, we may be fairly sure that no such scheme had gained general acceptance.

A similar process seems to lie behind postulate 4. The so-called "geometric algebra" of *Elements* II.1-10 appears to be a "critical" re-elaboration of a set of geometrical riddles that had circulated among Near-Eastern surveyors since c. 2000 BCE, and was solved by them by a "naive" cut-and-paste geometry, in which the correctness of procedures could be "seen" immediately.[39] Here, as in pre-Greek surveying in general, the concept of a quantified angle did not exist. Obviously, Babylonian as well as Egyptian surveyors distinguished practically right-angled from acute and obtuse corners. All evidence at hand suggests, however, that their understanding of the matter can be summed up in a pun: for them, a "right angle" was the opposite of a "wrong angle", that is, an angle whose legs do not determine an area.

Elements II.1–10 is a theoretical "critique" of the procedures and solutions in the sense that they put them on a theoretically secure foundation, proving the equality of areas instead of "moving them around". In order to do this, the author or authors of these proofs (probably working in the outgoing fifth century BCE) had to clarify their notion of angles. According to the *Republic* (510C), the concepts of the right, the obtuse and the acute angle were at first taken as self-evident and above discussion (and perhaps in no need of definition); but in *Metaphysics* Z, 1035^b8-9 and M, 1084^b7 we see that Aristotle already knew something like the definitions given in the *Elements* (quoting in the former passage the definition of the acute from the right angle in words that are almost the same as Euclid's, see above, p. 489). That the equality of all right angles is not guaranteed by the definition is likely to be a secondary discovery, certainly not yet known to Plato and probably not to Aristotle; but at some point between Plato and Euclid the fallacy was noticed and induced somebody to require explicitly that all right angles be equal, since the game could not be played otherwise.

A "Wittgensteinian" interpretation thus makes sense, and does not require that we ascribe to Euclid and his contemporaries any anachronistic familiarity with the *Philosophical Investigations* – only the stance which was formulated some centuries later by Cicero and Simplicios, see note 38. All that is needed is indeed that acceptance of the intellectual autonomy of a particular profession or scientific practice of which Aristotle's view of separate sciences with each its own principles is one rationalization, and the language-game formulation another. The discrepancy between what Euclid really does and what is

39 The best known reflection of this "surveyors' proto-algebra" is the Old Babylonian school algebra, whose geometric character is argued in [Høyrup 2002]. The relation to the geometry of *Elements* II is examined in [Høyrup 2001] ⟦= article I.3⟧. There is no reason to repeat the extensive argument here.

prescribed by Aristotle is sufficiently large to warrant a rather a-philosophical interpretation of the Euclidean text – Euclid, in other words, may have regarded the conflict between the postulates and cosmology as a problem for cosmologists alone. In terms of "existence", such a view would be compatible with a view close to what is regarded today as "naive Platonism" in didactical discussion – *of course* triangles and numbers exist (if anybody should get the quaint idea to ask), and *of course* they are ideal and not to be identified with the diagrams we draw and the collections of material objects which we amass.

Though compatible with the "Wittgensteinian" explanation of the use of apparently counterfactual postulates, however, this a-philosophical stance does not follow from it. No less compatible is the Aristotelian view as interpreted above, in particular in the symmetrical version of *Metaphysics* M. In such a view, the cosmological constraints might be seen as "impediments" which would interfere with the full unfolding of the essence of lines ("what it means to be a line"), circles, etc. The counterfactuality of the postulates would only concern the material objects of which the mathematicals are properties, and lines and circles might therefore "exist" just as truly as forms exist, counterfactuality notwithstanding – but still not exist as indubitably as substances in the proper sense.

A Platonist stance is hardly in harmony with any Wittgensteinian view, "naive" or otherwise. But it would have no greater difficulties with the conflict between cosmology and postulates. If mathematical numbers and figures derive from Ideal numbers etc., and the physical world is one or more steps further down the ladder, then the conflict would be nothing but yet another proof of the imperfection of the sensible world.

Constructibility versus existence

At an early point I promised to return to the question whether the ancients considered mathematics *in toto* as somehow existing, in the sense of not being freely constructible according to the whims of the mathematician. Even here, as in the case of the existence of single mathematicals, it will be wise (following the best Aristotelian and scholastic models) to distinguish rather than to answer with a precipitate "yes" or "no" to a question whose meaning is ambiguous. Since neither Euclid nor any other mathematician says anything of relevance to the question, we shall have to approach it through the philosophers.

From the Platonic and related points of view (including post-Platonic Pythagoreanism as represented by Nicomachos and Proclean Neoplatonism), both question and answer *are* probably unambiguous. If Ideal mathematics is divine – whether belonging in the thought of the creator god or in a less personified universal νοῦς – and mathematical mathematics derives from it, human thought or νοῦς is obviously not free to do more than discovering it. Similarly from the earlier Pythagorean stance as reported by Aristotle (see note 17 and preceding text): if material reality *imitates* numbers and the rest of mathematics is derived from arithmetic, arithmetic is clearly primary, and geometry,

harmonics and astronomy are woven into this primary fabric; if real things *are* numbers, the result is no different.

If we go to Aristotle, on the other hand, we *shall* need to distinguish. According to the view of *Metaphysics* M (which we may take to represent the fully developed Aristotelian view), it depends on the science through which we approach a given sensible object - without which neither forms nor mathematicals exist – whether its form (as understood by natural philosophy) or its mathematical properties come to the fore. Only the perspective of *mathematics appears* to create the conditions where the mathematicals become exact, where the circle has a point and nothing more in common with a tangent. In this sense, mathematics is *constructed by application of the perspective* of mathematical science. But this perspective, though built on removal of physical properties which from this stance are accidental, is not arbitrary. Even if we accept with *Physics* II that what goes on is a mental separation, there is no hint in Aristotle's formulations that this separation could have been made in a way that resulted in a different mathematics, once we have chosen to concentrate on *mathematical* properties and to disregard the rest.[40]

It is certainly possible to project part of the "doctrine of odd and even" of *Elements* IX.21–34 onto an arithmetic modulo 2. But there is, firstly, not a single word in the sources to suggest that the ancients saw this sequence of theorems as "an (alternative) arithmetic" and not simply as a part of arithmetic; secondly, some of the theorems in question are pointless if not meaningless if understood through this projection.[41] To see this as an "arithmetic constructed by the Pythagoreans", as done by Imre Toth [1998: 166*f*], is thus misleading if meant as an interpretation of ancient thought.[42]

Those who doubted that mathematics as known was compulsory seem to have rejected

40 More precisely, since mathematical sciences are plural: once we have chosen the perspective of arithmetic, only one arithmetic can result; and if we apply that of geometry, the geometry that results is equally predetermined. Cf. *Metaphysics* M as quoted on p. 495 (1078^a24–26, trans. [Tredennick 1933: II, 193]),

> the arithmetician assumes man to be one indivisible thing, and then considers whether there is any accident of man *qua* indivisible. And the geometrician considers man neither *qua* man nor *qua* indivisible, but *qua* something solid.

41 Thus proposition 30 [trans. Heath 1926: II, 417], "If an odd number measure an even number, it will also measure the half of it" – which translates into "if 1 measure 0 [which it always does], it will also measure half of it [which can be 1 as well as 0]). Even more absurd, of course, is the imposition of a concept of "0(mod. 2)" onto a thinking which does not know "0".

42 Toth's statement is made in connection with a broad argument meant to demonstrate that Plato and Aristotle were aware of the possibility to construct a consistent non-Euclidean geometry (which, as he sees it, they rejected on other grounds, thus assimilating their position to that of Saccheri and not that of Bolyai and Lobachevsky). Unfortunately for the thesis (set forth by Toth in a number of publications since 1966), the textual evidence on which it is based turns out on close scrutiny to be either misquoted or, at best, interpreted freely and out of context – see [Høyrup 2000].

theoretical mathematics wholesale, not just *this* mathematics. In the above quotations from Aristotle, we have already encountered two instances (both directed against geometry): Protagoras's appeal to the sensible line and circle, and the censure of geometers who claim that a line drawn on the ground is one foot long even if it is not.[43] In Cicero's *Academica* (a convenient compendium of post-Platonic scepticist opinions – ed. [Rackham 1933]) we find that Epicuros held all geometry to be false and the sun to be no larger than it looks (II.106 and II.82, respectively); Cicero himself (arguing as a spokesman of the scepticism of the post-Platonic Old Academy) accepts the compelling force of geometrical reasoning. Sextus Empiricus, while finding Epicuros's arguments superficial, rejects the legitimacy of the whole deductive endeavour.

All in all the ancients were thus convinced that mathematics, to the extent it was at all accepted as a separate field of theoretical study, was one and compelling (apart from its split into distinct disciplines). If this absence of free constructibility is what we really mean by asking for existence, mathematics was certainly held to exist.

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[43] Since the point is made both in *Metaphysics* M, $1078^{a}19$–20 and in *Metaphysics* N, $1089^{a}23$–26 (probably written at a considerably earlier moment), it seems likely that somebody had formulated this objection in earnest.

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Chapter 19 (Article II.2) Conceptual Divergence – Canons and Taboos – And Critique: Reflections on Explanatory Categories

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A few additions touching the substance in ⟦…⟧

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Abstract

Since the late 19th century it has been regularly discussed whether, e.g., the ancient Egyptian way to deal with fractions or the Greek exclusion of fractions and unity from the realm of numbers was a mere matter of imperfect notations or due to genuine "conceptual divergence," that is, to a mathematical mode of thought that differed from ours. After a discussion of how the notion of a "mode of thought" can be made operational through the linking of concepts to mathematical operations and practices it is argued (1) that cases of conceptual divergence exist, but (2) that the discussion of notational imperfection versus conceptual divergence is none the less too simplistic, since differences may also be due to deliberate choices and exclusions on the part of the authors of the ancient texts – for instance because such a choice helps to fence off a profession, because it expresses appurtenance to a real or imagined tradition, or as a result of a critique in the Kantian sense, that is, an elimination of expressions and forms of reasoning that are found theoretically incoherent. The argument is based throughout on historical examples.

Contents

PETER DAMEROW und WOLF WUCHERPFENNIG gewidmet,
auf Anlaß deren sechzigsten Geburtstage

Prolegomenon

In *Physics* IV.4, 212^{a}5–6 [ed., trans. Hussey 1993: 28], Aristotle states that *place* [τόπος] is "the limit of the surrounding body, at which it is in contact with that [body] which is surrounded" – in 212^{a}20 modified into "unchangeable limit of what surrounds". In IV.11, 219^{a}8–9 *time* is explained to be "either change [κίνησις] or some aspect of change" [ed., trans. Hussey 1993: 43] – a point made in almost the same words in *Metaphysics* Λ, chapter 6, 1071^{b}10. The *Physics* passage goes on to argue that since (as just shown) it "is not change, it must be some aspect of change", and concludes in 220^{a}3 that it is "the number of the motion" [ὁ τῆς φορᾶς ἀριθμός].

Nobody who has gone beyond an introductory course in the history of philosophy would get the idea that Aristotle thought so because he was unable to grasp space and time in (our) quasi-Newtonian way, as receptacles within which bodies are located but which themselves do not depend on the presence of such bodies. Regarding space he even explains (*Physics* IV.1, 208^{b}33–35) that Hesiod thought "as most people do" that place "is prior to all things, since that, without which no other thing is, but which itself is without the others, must be first. (For place does not perish when the things in it cease to be)" – and in *Categories* 6, 5^{a}6–13 [ed., trans. Ackrill 1963: 13] he himself comes disturbingly close to these "vulgar" opinions[1] about space and time:[2]

> Time also and place are of this kind [continuous quantities]. For present time joins on to both past time and future time. Place, again, is one of the continuous quantities. For the parts of a body occupy some place, and they join together at a common boundary. So the parts of the place occupied by the various parts of the body, themselves join together at the same boundary at which the parts of the body do.

Aristotle did not reject the seemingly modern views of space and time because they could not be thought in his epoch; after many pages of arguments he rejects them in the *Physics* because he cannot make philosophical sense of them: how, indeed, can "place" be established unless it be with regard to something known, and how can time be understood if we do not observe or think of something changing with time? Aristotle's solutions certainly do not coincide with those offered by Berkeley, Kant, Mach and

[1] This is how such opinions were characterized by the schoolmen, who were no less familiar with them – see [Grant 1981: 9].

[2] I leave aside as immaterial for the present discussion the question whether Aristotle's thought had developed over time or a different agenda made him speak differently.

Einstein, but on a general level the problems he tries to solve belong to the same family as theirs.[3]

The preceding remarks had to do with the history of natural philosophy. They may serve to introduce a similar problem for the historiography of mathematics. In this domain, the problem of past conceptual structures differing from ours has certainly been discussed for well over a century,[4] but the discussion has turned around a different pivot: were historical concepts really different and the historical actors unable to think or express themselves in our terms, or is everything just a question of terminology and notations? In the following I shall argue that this debate is unduly simplistic, and that more attentive reading of pre-Modern sources reveals that early mathematical writers, and not only Aristotle, might have other reasons than failing conceptual capacity or inadequate terminology to think or express themselves in ways that differ from ours. However, since mathematical writers tend to *use* their concepts or at most to define them rather than analyzing them or explaining their *raison-d'être*, we rarely have anything similar to Aristotle's many pages discussing the shortcomings of rival views to help us. We shall therefore start with some reflections on how to approach a "mathematical mode of thought".

[3] Cf. Einstein's introduction of the problem of time and contemporaneity in §1 of his treatise "Zur Elektrodynamik bewegter Körper" [Einstein 1905/1913: 28] (here, as everywhere in the following where no other translator is identified, the translation is mine):

> If we want to describe the *motion* of a material point, then we indicate the values of its coordinates as a function of time. Now it has to be kept in mind that a mathematical description of this kind only has a physical meaning if one has come to an understanding of what should be meant by "time". [. . .] When I say, for example, "That train arrives here at 7 o'clock", then the meaning is something like "The pointing of the small hand at 7 and the arrival of the train are simultaneous events".

Up to this point, the main distinction between Aristotle's reference to motion and Einstein's reflections on the meaning of time consists in the latter's specification of the kind of moving object he refers to (*viz* the hand of a clock). Serious divergence between the two only starts five lines later, when the finite speed of light is taken into account.

[4] In the later 19th century, we have Léon Rodet's attack on August Eisenlohr's and Moritz Cantor's use of modern algebraic symbolism in their interpretation of the Rhind Mathematical Papyrus (abbreviated RMP; see below), and the Zeuthen-Cantor debate [Lützen & Purkert 1994] about the (il)legitimacy of the reading of the historical record as contemporary mathematics. In more recent decades, the still cited standard example is the Unguru[1975]-Weil[1978]-Freudenthal[1977]-debate, with B. L. van der Waerden [1976] in an intermediate position not too different from that of Cantor. More examples are referred to in the following.

Tools and mode of mathematical thought

A “mode of thought” is *prima facie* as intangible as a *Zeitgeist*, and claiming that the mathematics of an ancient culture was rooted in a distinct “mode of thought” therefore does not in itself assist us much in understanding whether, why or in which respect this mathematics differed from ours – it amounts to little more than a reformulation of the same matter in more airy terms. Speaking of the mathematical “concepts” of the culture in question is somewhat less elusive, but concepts should not be identified with the mere words into which they are put. Disregarding general epistemological discussions we may start from the metaphor that a mathematical concept is *a tool*: a mental tool, but still a tool only by being a tool for operations. The shared properties and conditions of the whole network of connected mathematical concepts with participating operations then characterize the corresponding mode of thought.

This statement remains pretty abstract, but may be elucidated by an example. If we want to know (or, perhaps better, *decide*) whether, for instance, late medieval *abbacus* treatises operate with a concept of “negative numbers” it is not enough to notice that they use the word *meno*; even the observation that they state the rule that *meno via meno fa più* does not suffice.[5] As it is made manifest by the general adequacy of the translation “less” for *meno*, the rule might simply refer to a notion of “subtractive” members of a polynomial.[6] We should rather observe whether “numbers *meno*” also occur as results, or the actual use is restricted to expressions “*a* and less *b*” where *b* is not (or cannot, if roots are involved, easily be seen to be) larger than *a* (*a* as well as *b* being “non-*meno*” numbers or roots); further, whether the rule is used not only in multiplications of polynomials but also when a polynomial involving members *meno* is subtracted from another polynomial. If one of these conditions is not fulfilled, the notion of “less” is so different from our conception of “negative numbers” that it is misleading rather than illuminating to identify the two.

We could be more restrictive and refuse to speak of “negative” numbers before we have replaced the idea of two categories of numbers – normal and *meno* – by a single

5 An early published appearance is in the *Trattato dell’alcibra amuchabile* from c. 1365 [ed. Simi 1994: 17]. The unpublished occurrence in the *Aliabraa argibra* (ms. Chigiana M VIII 170, fol. 5^{v}) is linked to the example 10–2 times 10–2 and may go back to c. 1340. ⟦See article I.12.⟧

6 A “subtractive” number or member of a polynomial is an “ordinary”, that is, non-negative, member of an arithmetical expression or a polynomial whose role it is to be subtracted – as 3 has to be subtracted in the expression 5–3. As a practical category, subtractive numbers appear (together with the corresponding sign rule) in ordinary elementary school mathematics and often in introductory algebra long before negative numbers are introduced. Widespread lore notwithstanding, negative numbers are absent from Babylonian mathematics, but subtractive numbers are spoken of explicitly – see [Høyrup 1993].

category divided "in the middle" by 0; but we may also decide that the idea of two separate categories is nothing but another version of the concept. In any case, the two concepts or two versions of the concept are linked to different practices with appurtenant tools: *the two categories* to the practices of accounting and rhetorical equation algebra,[7] *the single category* to the new practices evolving around symbolic algebra, analytical geometry and *analysis infinitorum*.[8]

We may further observe that the two-category version is not simply an incomplete version of the single number line but in itself a mental tool which enabled Cardano and Bombelli to accept the only slightly more "false" categories of imaginary and complex numbers,[9] in a way which would have been barred once the single-category understanding was established, and which was only opened again by the invention of the geometric representation and the formal operation with pairs of real numbers.

A similar example is offered by the discussion whether the ancient Greeks possessed a notion of "general fractions" or merely one of repeated aliquot parts. Does $\begin{matrix}5\\16\end{matrix}$ (Diophantos, *Arithmetica* II.8, ed. [Tannery 1893: I, 93]),[10] mean $\frac{16}{5}$? Obviously yes,

[7] We may of course remember Leonardo Fibonacci's observation in the *Flos*, ed. [Boncompagni 1862: 238] that a certain problem "is insolvable, unless it is conceded that the first man has a debt", and the similar passage in his *Liber abbaci* [ed. Boncompagni 1857: 256]; but the real argument for the link is and remains the correspondence between the sets of operations on possessions and debts or incomes and expenses, booked in separate columns, and the treatment of numbers simply and *meno*.

[8] Actually, the first explicit reference to the single category which I know about (though for integers only) goes back to 1544, thus antedating symbolic algebra proper as well as analytical geometry and *analysis infinitorum*. In the *Arithmetica integra*, Michael Stifel [1544: 249r] explains that the "fictitious numbers" are "below 0, that is, below nothing". But it is also explained that this fiction is introduced because of its "supreme utility in things mathematical", a claim that is illustrated by the transformation of the subtraction (8+5) – (10+2) into (8–2) – (10–5) (both expressed in schemes, not by means of the parentheses invented by Bombelli ⟦yet only used by him for more restricted purposes, cf. article II.14⟧ – but anyhow in an early form of symbolization if this is understood as a representation that allows operation directly at the level of non-verbal representatives); on the verso of the same folio, furthermore, we find explicitly the sequence –3, –2, –1,0,1,2,3,4,5,6 counting the exponents of the geometrical progression $\frac{1}{8}$, $\frac{1}{4}$,$\frac{1}{2}$,1,2,4,8,16,32,64. This first beginning of the one-category view, like its later consolidation, is thus linked to considerations of consistency derived from inner-mathematical practice.

[9] The link is betrayed by the vocabularies they invent – for instance Bombelli's *più di meno* for $+\sqrt{-a}$ and *meno di meno* for $-\sqrt{-a}$. We might speak of "Papageno tolerance" ("Indeed, there are black birds. Why should there not be black people, too?"): the existence of one irregular category, once habit has made it acceptable, opens the mind to the acceptability of other aberrations of a similar kind even if they remain aberrations.

[10] Reviel Netz [2002] has observed that Tannery uses the stenographic symbols much more consistently in his edition than the manuscripts do, and that these latter do not use them in the same place. No manuscript, moreover, goes back to Diophantos's own epoch. Though complicated fractions

in the sense that this is the correct solution; but if we read the way such correct solutions are expressed in I.23 we see that $\frac{50}{23}$ appears as $\bar{\nu}\ \kappa\gamma^{\omega\nu}$, "50 of 23rds", and $\frac{150}{23}$ slightly later as "150 of the said part". Obviously, what *we* would express as $\frac{p}{q}$ is thought of as p times the qth part; this is different from the Pharaonic Egyptian canon, according to which each aliquot part should appear only once in a number; but it is not for that reason by necessity identical with our concept. In III.11, however, we also find that 30¼ times $\frac{77}{41}$ is $\frac{77}{1240¼}$, and that the "part denominated by" $\frac{77}{41}$ is $\frac{41}{77}$.[11] It seems reasonable to assume that Diophantos's concept was richer in operational links than a mere heaping of identical aliquot parts would suggest, and that he presupposes a similar richer concept on the part of his reader; but since he does not tell how he operates, we still cannot decide exactly how similar his "practised concept" was to ours.[12] Only in the late medieval *abbacus* treatises, where cross-multiplication and other arithmetical operations are explained in detail, also when polynomial denominators are involved, can we be sure that the concept is really close to ours.[13]

Structures of mathematical operations grow out of operations with tools in the proper sense: the manipulation of bamboo sticks on a counting board, geometrical construction on a dust abacus or paper, routines for accounting or for solving equations, etc. But they are never *identical* with the more or less structured set of operations with these tools but always contain both (*qua* abstractions) *less* and (*qua* intellectual elaborations) *more:* for instance, a diminished expense results in an increased possession, which corresponds to the rule that *meno*(*meno* α) is simply α – but no straightforward accounting operation corresponds to the rule that *meno via meno* is *più*.⟦[14]⟧ Therefore it cannot be excluded that mathematical conceptual structures that are fairly congruent with something *we* know

and reciprocals are less liable to variation in this respect than the symbols ϛ and ⋔ ("number" and "less", respectively), we should not feel too confident that the expressions appearing as shorthand symbols in the present argument were all written in that way by Diophantos himself, though some similar expressions certainly were. But the argument does not really hinge on the stenographic writings.

11 In more detail: ¼ is written $\delta^{\times}$ in agreement with the explanation given in the introduction (p. 6); the *arithmós* is found to be $\frac{77}{41}$, and the number which was posited as *arithmós*^{x} is then $\frac{41}{77}$.

12 A similar conclusion is reached by Jean Christianidis [2004: 333–335], from analysis of the more complex calculations of IV.36, in which fractions denominated by binomials are added and multiplied. In this case, as pointed out by Christianidis, Diophantos does refer to a general rule for the addition of fractions (μόρια).

13 Being unable to read the Indian texts I prefer not to include the indubitably earlier Indian fractions in this discussion.

14 ⟦The convoluted "if I spend 2 *soldi* two times less, I shall end up having 4 *soldi* more" is never seen in the sources as an argument.⟧

grow out of manipulations of tools which are quite different from those from which we are now accustomed to see them evolve. Identifying underlying tools that differ from ours does not prove that the corresponding concepts were also fundamentally different.

This is exemplified by the Cardano-Bombelli and the post-Gauss notions of imaginary and complex numbers. Another example (which goes both ways) is this: A couple of years ago a "historically interested" mathematician (I shall leave him anonymous and hence for copyright reasons not quote his words) claimed in a discussion on a web-site that the Babylonians could hardly have failed to recognize the particular character of irrational square roots because they will have seen that the equally non-finishing sexagesimal reciprocals of irregular numbers are periodical, and have to be so because of the finite number of possible remainders. He forgot that the structure on which he himself was first taught about irrational numbers – probably decimal fractions and operations with rational numbers – was not the one on which the Greeks developed their notion of magnitudes that "could not be spoken" or "were not in ratio". He failed to notice that only the existence of a distinction between rational and irrational magnitudes once developed in relation to a different set of operations makes the distinction between periodic and non-periodic decimal fractions interesting. He presupposed (without knowing to presuppose anything) that the Babylonians divided by irregular numbers in a way that leaves successive remainders (he may have been right, but that is a different question and so far undecided); and he overlooked that the sources that elucidate the question – the few listings of the reciprocals of irregular numbers – all stop before getting to the point where periodicity shows up, with the sole exceptions of the reciprocals of 59 (told to be 1 1 1) and of 1 1 (i.e., 61), told to be 59 59 (meaning 0;59,0,59) – hardly cases that invite to consider the total set of possible remainders. All in all, the partial agreement between the ancient and the modern concepts of irrationals (and between ancient and modern place value notations) has veiled that the underlying sets of operations are different, and therefore invite different further extensions.

Leaving the anonymous mathematician aside we may also note that the aim of *Elements* X is very hard to understand if one's concept of irrationals is based on decimal fractions, whereas – conversely – the Greek concept does not allow the formulation of the distinction between algebraic and transcendental irrationals (not to speak of the theorems about the different decimal-fraction convergence patterns of the two classes).

These hints and sketched arguments should suffice to illustrate, both the fertility of the claim that mathematical concepts and conceptual structures are formed in interaction with tools within a practice, and the dilemma presented by the lack of clear one-to-one correspondences between practices and mathematical conceptual structures. If we leave out the epithet "mathematical", this is of course a well-established Hegelo-Marxist point of view.[15] They should also suffice to show that "conceptual divergence" – differences

[15] However, insight into the connection between practices and conceptual structures certainly precedes

between concepts that cannot be reduced to more or less full development of the same ideal concept – is something that must be taken into account.

The latter point can hardly be considered a historiographic revolution. As mentioned above, attempts to trace the differences between foreign and familiar conceptual worlds are certainly not new within the history of mathematics. The linking of the divergence to different practices is less hackneyed[16] – the prevailing tendency has been to find the inner logic of a certain conceptual world and explain its character or limits from there (a somewhat circular argument). Moreover, claims that the mathematical concepts of other cultures differ from ours were mostly challenged by proponents of the view that mathematics is only plural from the grammatico-etymological point of view, and that differences are to be found at the level of notations, not of thought (apart from that increasing scope and sophistication of mathematical thought which nobody could and would deny). We shall encounter more examples of both views below.

However, the fruitfulness of the notion of conceptual divergence is no proof that it explains *all* differences between the ways ancient and more recent texts speak about what from a Zeuthen-Weil point of view is basically *the same* mathematics. Some examples will show that other factors have sometimes been in play.

Egyptian discussions

The historiography of Egyptian mathematics is a classical ground for fighting the battle about dissimilar concepts. The first skirmish, mentioned in passing above, was between August Eisenlohr [1877] and Moritz Cantor [1880] on one side and Léon Rodet [1881] on the other – the former two explaining the procedures of the ‘ḥ’-problems in the Rhind Mathematical Papyrus by means of symbolic first-degree algebra, the latter claiming that this betrayed the underlying thought of the Egyptian calculator and proposing (with ample references to pre-modern counterparts) the use of a single false position. This discussion went on for long – I shall only mention Eric Peet’s identification of Rodet’s reference magnitude or *bloc extractif* with a common denominator [1923: 18] and Otto Neugebauer’s arguments that this modernizing view “misunderstands the inner unity of Egyptian computation completely” [1934: 138*ff*, quotation p. 145].

both Marx and Hegel – in Goethe’s *Wahlverwandschaften* (II.8; [*Werke* VIII, 149]), the romanticist *Gehilfe* describes the relation between the changing view of nature and general material conditions and corresponding economic practice as follows:

> People who need to use their land will soon erect walls around their gardens to ensure their crop is safe. From there, a new view of things gradually emerges. Once again, the useful gets the upper hand, and in the end even the one who owns much will feel obliged to use all of it.

[16] But see the articles in [Damerow & Lefèvre (eds) 1981].

Slightly later came discussions about the particular Egyptian way to express fractional quantities – strikingly different from ours yet coherently developed and hence apparently the best candidate for a way to think about numbers that disagrees with our ways without being merely incomplete. As a typical representative of the attempts to be loyal to the Egyptian pattern of thought we may quote Alan Gardiner's *Egyptian Grammar* [1957: 196]:

> For the Egyptian the number following the word *r* had ordinal meaning […]. As being the part which completed the row into one series of the number indicated, the Egyptian *r*-fraction was necessarily a fraction with, as we should say, unity as the numerator. To the Egyptian mind it would have seemed nonsense and self-contradictory to write *r-7 4* or the like for 4/7; in any series of seven, only one part could be the seventh, namely that part which occupied the seventh place in the row of seven equal parts laid out for inspection. Nor would it have helped matters from the Egyptian point of view to have written
>
>  *r-7* (+) *r-7* (+) *r-7* (+) *r-7* ,
>
> a writing that would have likewise assumed that there could be more than one actual "seventh". Consequently, the Egyptian was reduced to expressing (e.g.) $\frac{4}{7}$ by $\frac{1}{2}$ (+) $\frac{1}{14}$.

Already Friedrich Hultsch [1895: 9] had pointed out that what we regard as fractions with a numerator greater than 1 was "nach Ägyptischer Anschauung Vielheitstheilungen oder noch nicht zu Ende geführte Divisionen". Evidently the Egyptians knew how to express 4 as measured by 7, namely as a sum of aliquot parts; paraphrasing Hultsch one might say that $\frac{4}{7}$ could well be thought, but as *a problem*, whose *solution* was $\dot{2}\;\dot{14}$.[17]

Peet and others objected that this was a question of notation, not one of *conception*; in Peet's words [1923: 16], "the argument from what the [Egyptian] was capable of expressing in symbols to what he was capable of conceiving is a *non sequitur*, and the suggestion that his notation must surely have kept pace with his conception will fall on deaf ears in the case of those acquainted with the amazing conservatism of the Egyptian mind in every branch of life".

More compelling than such general appeals to the supposedly familiar character of the Egyptian mind (though scarcely ever noticed) were perhaps Vogel's objections. He pointed [1929: 43] at two telltale slips in RMP #81 (noted by Peet [1923: 123] in the translation but not commented upon by him): in one place the scribe writes $\dot{5}$ instead of $\dot{2}\;\dot{8}$, thus betraying that something like $\frac{5}{8}$ (probably 5 times $\dot{8}$) was on his mind; in the following line, $\dot{4}\;\dot{8}$ is replaced by 3, with the implication that he was thinking of $\frac{3}{8}$. Vogel and others also pointed out that the unhesitating doublings of aliquot parts with

17 In the manner of hieratic Egyptian I use a dot over a number to indicate the corresponding "weak" number or aliquot part.

denominator $2n$ as $\dot{n}$ implies knowledge that $2\dot{n}(+)2\dot{n} = \dot{n}$, and thus presupposes some concept of $\frac{2}{p}$ that entails $\frac{2}{2n} = \dot{n}$.

In [1975], David Silberman pointed out that a late Old Kingdom use of the aliquot part notation (in fact the *only* known Old Kingdom instance of fractions beyond the "natural fractions" $\ddot{3}$, $\dot{3}$, $\dot{2}$, $\dot{4}$, "$\frac{3}{4}$" and $\dot{6}$, as far as I am informed) registers a damage to a cup as being large $\dot{5}$ $\dot{5}$ finger.[18] All in all it seems legitimate to conclude, not only that the Egyptians knew $p{\div}q$ as a problem, but also that they were able, so to speak, to manipulate this *problem* (presumably thought of as $\dot{q}$ $\dot{q}$ … $\dot{q}$) as a *representative of the solution*, that is, as a number. But this observation does not change the fact that Middle Kingdom scribes refused to use this kind of number when writing down a result. We shall return to the conclusions that may be drawn from this fact.

Babylonian mysteries

Discussions similar to those concerning the Egyptian "equations" and "fractions" are almost non-existent in connection with Babylonian mathematics. Discussions there certainly are – but they have concerned the question whether Babylonian "algebra" was really algebra or not, and if not, whether it was a collection of empirical recipes or based on arguments hidden from view.[19] I shall not pursue these topics, they are nor very relevant for the present discussion.

Without being discussed, however, statements about the particular mathematical mode of thought of the Babylonians have certainly been made.[20] One example is Abram A. Vajman's explanation [1961: 100] of the custom of subtracting an entity before it is added elsewhere, which he saw as an expression of a primal "concrete" organization of thought not yet ready for abstraction: we cannot add something before it has been made available.

In my [1990: 264] I cited Vajman's observation and explained (away) the only exception I had noticed by then. More recently, however, a fuller survey of texts made me discover that the exceptions are numerous, and that there is a pattern in their distribution.

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18 Silberman explains this as an instance of scribal ignorance, but in the context of Middle Kingdom mathematics the point is so fundamental that it would correspond to a modern accountant ignorant of the place value system (as I have pointed out at an earlier occasion).

The text in question is published in [Posener-Kriéger & de Cenival (eds) 1968], with fractions $\dot{4}$, $\dot{6}$ and $\dot{5}$ $\dot{5}$ on plates 23–25; translations can be found in [Posener-Kriéger 1976].

19 Until recently (and even today in much of the general literature), "Babylonian mathematics" was conventionally understood as an undifferentiated whole. The following regards only the mathematics of the Old Babylonian period (2000–1600 BCE), during which the overwhelming majority of known mathematical texts were produced.

20 Outside the domain of mathematical thought, similar statements have also called forth discussion. I shall restrict myself to mentioning [von Soden 1936] and [Larsen 1987].

Let me first present an example where the rule is followed: the problem solved in the text YBC 6967 [MCT, 129]. It deals with two numbers *igûm* and *igibûm* that belong together in the table of reciprocals (the names means "the reciprocal" and "its reciprocal"; for short in the following, n and $\tilde{n}$), and whose product is hence 1 or (in the actual case) 60; their difference is told to be 7. The product is spoken of as a "surface", which allows the interpretation of the procedure which is shown in the diagram. First, the excess of $\tilde{n}$ over n is bisected and moved around. This transforms the rectangle into a gnomon, which can be completed as a square by being joined to the smaller square □(3½) which it encloses. The area of the completed square is 72¼ and its sides, both vertical and horizontal, hence 8½ ; from the vertical side we now remove that part which was moved around, leaving 8½−3½ = 5 as n; putting the *same piece* back into its original place and joining it to the horizontal side of the completed square gives us $\tilde{n} = 8½ + 3½ = 12$.

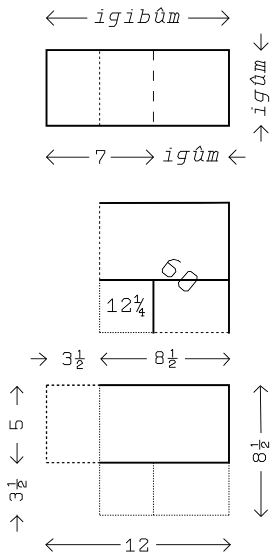

Figure 1. The procedure of YBC 6967.

Rectangle problems where the sum of the two sides and not their difference is given together with the area do not require that *the same* piece be removed and joined. Here, as in all cases where there is no inner constraint (not least when independent variations of problems are listed in sequence), the Old Babylonian texts let addition precede subtraction exactly as we do. On the other hand, rectangle problems are not the only ones where concrete meaningfulness requires subtraction to precede addition. As an example I shall mention the first-degree problem VAT 8389 #1 [MKT I, 317*f*], in which a field is divided into two partial fields. The rent per area unit for each partial field is given together with the complete area and the difference between the total rents paid from the partial fields.[21] At first the two total rents are found under the assumption that the partial fields are equally large. The amount by which these hypothetical rents differs is too small, and the next step is to find how much must be transferred from one partial field to the other in order to give the required difference; this piece is then really transferred, first removed, then joined.

Other texts, also texts treating the same type of rectangle problem as YBC 6967, do not respect the principle. Often, after having found the side of the completed square, they have the abbreviated formula "join and remove" – at times expanded into "join to one, remove from the other" – and then state the two resulting values. Moreover, it turns out

21 Vajman's primary example is a problem from the closely related text VAT 8391. As a second example he refers to YBC 6967.

that several of the texts that use the short version of the formula belong to the earliest phase of Old Babylonian "second-degree algebra", being thus close to the adoption into the scribe school of a set of mathematical riddles treating of the sides and areas of squares and rectangles originally belonging to an environment of non-scribal surveyors; in contrast, all those texts which respect concrete meaningfulness are younger.[22]

The use of the concretely absurd ellipsis thus cannot be explained as a result of a century's school routine in which once concretely meaningful operations were worn down to their arithmetical essentials. Quite reversely, it turns out to be the scribe school that *invented* concreteness, or made it a canonical rule – or rather, that certain scribe schools did so: texts from other late text groups do not respect the canon.

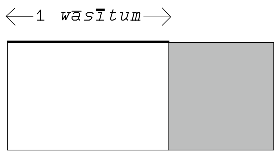

Figure 2. The square area and the side provided with a *waṣītum*.

I just referred to Old Babylonian "algebra", claiming in the same breath that it deals with geometric problems. The ideas of a Babylonian "algebra" and of geometry did non originally go together. Neugebauer claimed already in 1935 [MKT II: 63*f*] that the "nonsensical" inhomogeneous additions of sides and areas found in many texts prove that the problems are numerical and the geometric appearance an external dress; the same argument was advanced for instance by van der Waerden [1962: 71*f*]. Neither drew any consequences of the fact that the texts in question regularly use two different words for addition and distinguish between the situations where one or the other should be used (though Neugebauer appears to have been fully aware of it). One (*waṣābum*, "joining") is meant to be concretely meaningful; the other (*kamārum*, in general interpretation "to heap", "to accumulate") can be used to add together the measuring *numbers* of discordant entities – lengths and areas, areas and volumes, or men, working days and bricks produced. As a rule, problem statements formulate the addition of such discordant magnitudes as a "heaping".

When *solving* problems in which, for instance, the "heap" of a square area and the corresponding side is given, the texts may then employ various devices in order to make the sum concretely and not only numerically meaningful and permit a geometrical procedure analogous to the one that was shown in Figure 1. One major text (BM 13901, in [MKT III, 1–5]) refers to an entity called "*wāṣītum* 1", derived from a verb meaning

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[22] For brevity and in agreement with established tradition I use the term "algebra" about the solution of problems about square or rectangular areas and sides. It is immaterial for the present discussion whether and in which sense this usage is justified; if "algebra" began with Emmy Noether, as maintained by some mathematicians, then of course there was no "Babylonian algebra".

For the relative dating of the Old Babylonian texts I refer to [Høyrup 2000a], for the derivation of the "algebra" from a set of non-scribal mathematical riddles for instance to [Høyrup 2001] ⟦=article I.3⟧. Both issues are also treated in [Høyrup 2002].

"to protrude", "to go out". The side *s* is represented by a rectangle ⊏⊐(1,*s*), the other side of which is a line of length 1 protruding from the square – heavily drawn in Figure 2.

A similar trick but a different word is used in TMS IX #1,[23] in which the length is added to a rectangular area, and it is explained that this corresponds to the "joining" of a "base 1" (KI.GUB.GUB 1) to the width – see Figure 3 ⟦the text can be found in translation in article II.3⟧. A third trick, used in the text YBC 4714 #30–39, consists in introducing a "second" width of 25 in order give concrete meaning to the statement that the difference between the squares on the two sides of a rectangle is equal to 25 times the smaller of these sides.

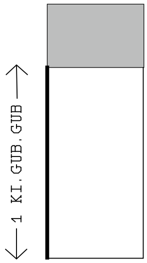

Figure 3. The rectangle provided with a "base".

However, the texts that avoid "joining" sides to areas and "heap" them instead are relatively late; those belonging to the earliest text groups express no scruple when "joining" sides to areas, and thus make implicit use of a notion of "broad lines", lines which on their own possess a virtual breadth of one length unit (an inherent "*wāṣītum* 1"). Broad lines turn out to be widely spread in practical geometries, where the use of a fixed basic unit of length can be presupposed (we still sell cloth with its physically determined breadth according to the same system).[24] They have always tended to disappear from more scholarly mathematics – as expressed by Plato (*Laws* VII, 819D–820B, ed. [Bury 1926: II, 104–10]), the Greeks should be ashamed for being ignorant, not only of the problem of incommensurability of magnitudes of the same kind but also of the fact that lengths, surfaces and solids are neither exactly nor "moderately" [ἠρέμα] commonsurate – that is, for example, that a surface of "3 feet" (*viz* in length, tacitly 1 foot broad) has no common measure with a line of 5 feet.

The use of "heaping" of measuring numbers as a way to make sense of what the Old Babylonian school masters no less than Neugebauer would consider "nonsensical" joinings is thus another secondary development, a creation of the school; the non-scribal environment from which the problems were first taken over had no use for it. The various devices by which "broad lines" are transformed into rectangles whose lengths are the corresponding "Euclidean lines" (i.e., "lengths without breadth", in the words of a definition which is already found in Aristotle's *Topica* 143^{b}11 [ed. Tredennick & Forster 1960: 591]) were also inventions of the scribe school. According to the vacillating verbal expression of the same basic idea they were probably later than the elimination of broad lines through

23 Text edition in [TMS, 63] – but see the corrections and the reinterpretation in [Høyrup 1990: 320*f;* 2002: 89–95].

24 See the discussion in [Høyrup 1995] ⟦translated as article I.7⟧.

the distinction between "heaping" and "joining" – "heaping" is in general use throughout the later corpus and always spoken of in the same term.[25]

"Heaping" is certainly not the only term to be used without variation throughout the Old Babylonian corpus or most of it (actually, even the text groups and often those very texts that "join" lines to areas employ "heaping" for certain other additions); most of the essential terminology is shared, which is the main reason that the only consistent attempt made until recently to distinguish between separate text groups [Goetze 1945] had to be based on orthographic criteria. But closer inspection reveals a number of subtle differences, of which I shall list some of the most significant:[26]

- In some text groups, the fact that (e.g.) 3 is the side of a square with area 9 is expressed in the Sumerian phrase "9.e 3 íb.s i$_8$", "alongside 9, 3 is equal"; others, probably influenced by the use of tables of square roots, employ the Sumerian verb íb.si$_8$ as a noun (we may translate it "the equalside"), and state that "3 is the equalside of 9".
- In two (early) text groups, íb.si$_8$ is replaced occasionally or consistently by another conjugated form ba.si$_8$ of the same verb; in one of them this term is used as a verb, in the other as a noun. All other text groups use ba.si$_8$ only when sides of a cube or a rectangular prism are referred to and in more generalized functions.
- Some text groups (early as well as late) refer to "each" of the sides of a square or to "all four" sides – according to various criteria groups that remain close to that non-scribal geometrical practice which had once supplied the riddles; others avoid this usage consistently.
- Text groups from the periphery of the ancient Sumerian area – regions that had long been under the cultural influence of Sumer but had been subjected only briefly to the "Neo-Sumerian" empire in the 21st century BCE – announce the appearance of a numerical result by saying that "you see" the number. All groups from what had once been the Sumerian core area avoid the phrase, with the exception of one very early group from Ur which sometimes uses the Sumerian equivalent pà d; they do so not in ignorance of existence of the expression – for instance, a question what to do "in order to see" the value of a magnitude[27] shows that the idea was familiar – but apparently as a consequence of deliberate choice. Some of the core groups state that a result "comes up", one group that the calculation "gives" it. In all groups but this one, "giving" occurs exclusively in connection with numerical calculations within

[25] Counting the Akkadian verb *kamārum* in syllabic writing and the two logographic writings of the term (g̃ar.g̃ar and UL.GAR) as one.

[26] I refer again to [Høyrup 2000a] and to the more matured discussion in [Høyrup 2002: 317–361].

[27] YBC 4608, obv. 22, 28 [MCT 49*ff*].

the sexagesimal system. Nine (early) texts from Eshnunna in the periphery, all of them found in the same room, use "seeing" in problems linked to the riddle tradition and "coming up" in problems belonging with traditional scribal computation, and couples the two terms consistently to different ways to ask for values; texts from other localities, and even one from a neighbouring room, confirm the historical affiliations of "coming up" and "seeing" but reveal that the linking between a problem's "home tradition" and its way to ask questions is mistaken, and should have been turned upside down.

- The nine texts which couple the way to announce results, the way to ask questions and the "home tradition" of problems start all problems by the formula "If somebody asks you thus, …". This obvious borrowing from the riddle tradition (here used however also for problems with a different historical affiliation) is present in a few texts from the same region, but never appears elsewhere, *except* in one text which uses it in abbreviated form in a single problem which also on several other accounts demonstrates to be a folkloristic citation of non-school usage. Once again the formula (which survives within the practical-geometrical tradition until the late Middle Ages together with "each" or "all four" sides of a square) is seen to have been known, and its absence from the texts thus to be a result of filtering.
- Some text groups invariably start the prescription by a formula "You, by your proceeding"; others restrict themselves to a terse "You"; still others omit the opening formula altogether.
- early texts often employ two terms for removal, one (*nasāḫum*) meaning "to tear out", the other (*ḫarāṣum*) "to cut off', or use the latter only; if making use of both, they tend to "cut" from lines and "tear" from areas; the first of these verbs possesses a Sumerian logographic equivalent (z i), the second not (which implies that it will have belonged to the non-scribal tradition). Later groups eliminate "cutting".

These and a number of similar observations demonstrate that the Babylonian school masters were actively engaged in the creation of canons for *how mathematics should present itself*, tabooing alternatives; they also show that different schools – even schools located within the same town and active during the same decades, as revealed by comparison of different texts from Tell Harmal in Eshnunna – did not agree fully on what was canonical. Some of the choices may have aimed at fencing off the school from non-school practice – thus the avoidance of "seeing" in texts from the core, the elimination of "cutting" and of "each side"/"all four sides", and the ousting of the riddle formula "If somebody …". Others – for instance the deliberate linking of the ways to ask questions and announce results – seem to reflect a wish to keep traditions alive in memory (mathematicians of our days are not the first to be "historically interested", nor are they the first to reinvent history). Still others – the use or non-use of introductory formulae in prescriptions, the generalization of the riddle formula to all problems types in one group and of "giving" to all kinds of resulting in another – can hardly be seen as anything but *stylistic* choices.

All of these possibilities – fencing off, *Traditionspflege*, style – belong to the category with which postmodernist historiography of science has been much concerned, those characteristics which are "shared by science and organized crime", in a locution borrowed from the Popper-Kuhn exchange in [Lakatos & Musgrave (eds) 1974]. The "rule of concreteness" and the elimination/justification of the "broad lines" represent a different category, that cardinal virtue on account of which, in Benjamin Farrington's words, science cannot be ethically neutral but "must be true".[28] More precisely, they are instances of *critique* in a Kantian sense, asking for the *extent* to which and the *conditions under which* what is usually done or believed is justified – "Untersuchung der Möglichkeit und Grenzen derselben", "examination of its possibility and limits", in Kant's words (*Critik der Urtheilskraft*, BIII [Werke V, 237]). Explaining the solution of the *igûm-igibûm* problem as done above,[29] the teachers can be supposed to have discovered that the moving back of the piece that had been displaced contradicted the precise wording of the traditional formula "join and remove", and that they had to turn the phrase around; that they also expanded it is likely to reflect a will to emphasize the concrete meaningfulness of the operation. Similarly, the school environment will have made the notion of the broad line implausible or outright inconceivable.[30] At an early moment, this will have induced the schoolmasters to separate the statement of a problem, made in terms of the "heaping"

28 [Farrington 1938: 437]. Even this norm is of course, *qua* institutional imperative, essential to the long-term "career strategy" ⟦indeed, survival strategy⟧ of the scientific institution as such; but since postmodern science studies committed academic parricide on Robert Merton as part of their own career strategy, this has largely escaped their attention. Moreover, the fact that even Old Babylonian scribe school teachers felt obliged toward it shows that this norm, just as any other moral norm, was a generalization that went beyond what immediate utility and self-interest seemed to ask for (in their case, a generalization from the institutional obligations to find the correct result and to be able to teach efficiently how to find it).

29 Most of the Babylonian mathematical texts are parsimonious in giving such explanations (though less so than was believed as long as the whole terminology was interpreted in a purely arithmetical key), but several texts from Susa contain didactical expositions (for instance, expounding the use of the KL.GUB.GUB); a few texts from other regions contain rudiments of a similar pedagogy, confirming the hunch of Neugebauer and others that the texts went together with oral instruction explicating the meaning and purpose of steps). See [Høyrup 2002: 85 and *passim*].

30 This may have been a consequence of the teaching of the topic, of the need to have a particular notion of the line of *no* breadth; in similar torment when adding "roots" to "squares", Pedro Nuñez [1567: fols. 6r, 232r] had to explain that roots are to be understood as rectangles whose width is "la unidad lineal". It may, however, also be correlated to the cognitive organization of the Mesopotamian school since its fourth-millennium beginning around what Luria [1976: 48*ff*] calls "categorical classification", in contradistinction to his "situational thinking" – see [Høyrup 2000b: 16]. "Situational thinking", mental organization of the world in terms of customary and invariable situations, is indeed a generalized correlate of the presupposition of the "broad line": that everybody knows and agrees what *the* standard breadth has to be.

of measurable numbers, from the geometric procedure by which it was solved; in the latter, the sides could be represented by rectangles. Later, various schools invented (each in its own words) ways to justify the trick of the procedure.

The geometry of *Elements* II.1–10 can be understood as a critique of the cut-and-paste procedures of the surveying tradition. They presuppose the *definition* of what a right angle is (likely never to have been discussed by the practical geometers of earlier times, who will have had no difficulty in distinguishing a good from a skew corner) as well as the *postulate* that was necessitated by this definition (since it turned out not to be self-evident from the definition that all right angles are equal). On this basis, the proof of II.6 (which we may take as our prototype) constructs the rectangles and squares of Figure 1 meticulously and shows the necessary equalities; in this way the text shows that what had "always" been done is indeed justifiable on the best theoretical foundations. This corresponds to a general characteristic of Greek philosophy, and vindicates the view that the "Greek miracle" consisted to a large extent in this kind of critical questioning. As we see, however, critique was no Greek privilege but also undertaken by the Old Babylonian schoolmasters. They did not make a critique for all times to come, and Euclid had his role to play. But after David Hilbert's *Grundlagen* the fleeting character of every critique should no longer come as a surprise: even Euclid's critique turned out to be in need of re-critique[31] – and Kant's *Critik der Urtheilskraft* is in a way a critique of the preceding critiques of pure and practical reason.

Summing up the observations made here on the Old Babylonian material we may conclude that much of what the texts do not say or do not do must be explained, not from what their authors *could not think* but instead in terms, either of *what they did not find it professionally fitting to say*, or of *what they found it incoherent to say*.[32] Thus, in both cases, of what they *refused to say*. Better, perhaps, in terms of what they *refused to write down* – some of the slips suggest that they may have used the tabooed language in oral expositions.

31 For this Hilbert was of course only needed in view of the ever-recurrent returns of didactics to Euclid as the supreme model; much of the medieval commentary tradition, Islamic as well as Latin, already submitted the holy text to critical desacralization.

32 My impression from the texts that were used in school to inculcate professional attitudes and self-importance ("examination texts" and proverbs dealing with scribes) is that intellectual coherence was no part of the explicit norms regarding what was professionally fitting. But not all norms are in need of being made explicit: few of us ever had to be told that it is unfitting to eat your soup with your feet on the table – it is as self-defeating as teaching mathematics through incoherent explanations.

Egyptian flashback

With this in mind we may return to the question of the Egyptian canon. If the Egyptians knew to treat the problem $p \div q$ as "a *representative of the solution*, that is, as a number" but "refused to use this kind of number when stating a result", then we are again confronted not with a case of what the calculators were *unable to think* but of what they *refused to write down*.

Even in this case, the canon is likely to have been produced by the school. Firstly, there is the argument *post hoc, ergo propter hoc:* the scribe school only replaced master-apprenticeship teaching at the onset of the Middle Kingdom [Brunner 1957: 11–15], that is, at the dividing point between the Old Kingdom irregular $\dot{5}$ $\dot{5}$ and the canonical expression of fractional quantities in Middle Kingdom mathematical and administrative papyri. Secondly, third millennium computation had made use of sub-units instead of fractions, which is indeed much more convenient for practical purposes. Sub-units, however, presuppose rounding and thus preclude the teacher's unambiguous decision whether "you have found it correctly" (the recurrent phrase from the teacher's annotations to the Moscow Mathematical Papyrus, ed. [Struve 1930]). Since the full and systematic unfolding of the unit fraction *system* in the Middle Kingdom thus corresponded to *a need* which only came into being by the emergence of the school, it is likely to have been *brought about* by the school – and with this system, in which denominators might go into the hundreds or even further, repetitive writings of p times $\dot{q}$, in the vein of $\dot{5}$ $\dot{5}$, were certainly neither practical nor practicable.

But this can hardly be the *raison d'être* of the canon. For the higher numerals, the Egyptians made use of multiplicative writings much in the manner of Diophantos, first eliminating in this way the unit 1000000 and next also 100000 [Sethe 1916: 9].[33] The Egyptians clearly *could* think in this way if they wanted to. Gardiner knew so – this kind of multiplicative writing is precisely what is meant by his "*r-7 4* or the like". The hieratic slips $\dot{5}$ and 3 in RMP #81 show that they actually *did* think like this on occasion.

Why then? It is not to be excluded that Gardiner got a point, and that the Egyptian school masters when figuring out what could be meant by an aliquot part q explained it in a way that precluded that more than one copy could legitimately be present. It may

[33] For instance, 27,000,000 could be written as 270 below the sign for 100,000, and 40,000 as 4 below the sign for 10,000; as in Diophantos, we see, the unit which is counted ("the denominator") is written above the number counting it ("the numerator").

Even in Jacopo of Florence's *Tractatus algorismi* from 1307 [ed. Høyrup 1999: 6], the same notation is used when the meaning of the Hindu-Arabic numerals is explained, "700" being for instance explained as $\overset{c}{vii}$ and "400000" as $\overset{m}{cccc}$. I shall leave aside as undecidable the question whether this constitutes a case of borrowing (through channels unknown to us) or of independent invention and thus evidence that the notation falls "natural".

also have to do with the computational technique and its use of repeated doublings, as proposed by van der Waerden [1938: 361] and accepted by Clagett [1999: 25]. We cannot know, nor can we exclude the possibility that both explanations are wrong and that a third motive has to be looked for. In any case, the canon was the outcome of *deliberate choice*, not of mental divergence.

Greek "numbers"

Nobody suspects that the ancient Greeks made their geometry in the Euclidean manner because they were intellectually incapable of thinking in more heuristic ways. For this, the testimonials of Greek heuristic thinking are too copious. The only account where mental inability has been imputed to the Greek geometers is Sabetai Unguru's rejection [1975] of the idea that the real reasoning of *Elements* II, *Elements* X and Apollonios's *Conics* is algebraic. I see no reason to challenge Unguru's arguments.

When it comes to Greek theoretical *arithmetic*, however, claims about the limits or distinctiveness of Greek thought abound. As is known, the *arithmói* of Greek arithmetic, translated "numbers", are supposed to be the integers 2, 3, 4, … – 1 being the "root of number" but no number itself. This is born out by numerous passages in Aristotle's *Metaphysics*, at times as a plain and obvious fact, at times as something which "is said" or "said by some"; it is stated less clearly in *Elements* VII, deff. 1–2; and it was repeated countless times until Boethius (and, in the wake of the latter, another set of countless times until the Renaissance). Fractions, of course, are no *arithmói*.

Here, it is often claimed (names and exact quotations omitted for reasons of charity) that the Greeks *could not* think otherwise. Since they understood number as a "collection of units", they "failed to understand" that 1 is a number.

Several fallacies are involved. Firstly, endemic preaching against sin is evidence of the existence of endemic sin, not of virtue; no ancient Greek writer ever asserted that "nothing" is not a number, because this was not an idea that would ever come to him. If it was necessary to explain so often that unity was no number, then the temptation must have been great to see it as one. That unity and "numbers" were treated together and on a par in practical reckoning is obvious and may already suffice to explain from where temptation might come. But we do not need to leave the domain of the theoreticians. Reading one definition further in *Elements* VII we find a definition of "being a part" which presupposes that the part is a number;[34] accordingly, 2 is a part of 12 (the 6th part), but 1 is not a part of 6 if the definitions are taken seriously.

[34] Μέρος ἐστὶν ἀριθμὸς ἀριθμοῦ ὁ ἐλάσσων τοῦ μείζονος, ὅταν καταμετρῇ τὸν μείζονα – "A number is a part of a number, the smaller of the larger, if it measures the larger" [ed. Heiberg 1883: II, 184]. It is not said explicitly that only numbers can be parts, but no other definition states which other kinds of parts exist. In consequence, 1 cannot be a part of any number unless, at least for practical purposes, a number itself.

Discussions about the legitimacy of definitions always tend to become futile, and we might well allow Euclid this quirk. But definitions have consequences, and this one has the consequence that the parts of 6 are only 2 and 3, for which reason proposition IX.36 about perfect numbers becomes false. Obviously Euclid did not mean exactly what he said, but rather that "unity or a smaller number is a part of a larger number if it measures it"; the slip is not serious *unless* we believe that there was a fundamental difference – which we may conclude that there was not.[35]

The case of fractions is no different. Again, accountants would certainly divide unity "in many parts". This, indeed, is Socrates's complaint in the *Republic* (525D–526A, ed. [Shorey 1930: II, 162–164]). But so did Diophantos – his $\frac{77}{1240\frac{1}{4}}$ is the answer to a request for *a number*.

We are forced to conclude once again that the conceptual otherness which is reflected in the sermons about the nature of number is not caused by any inability to think otherwise; the sermons censure an ever-recurrent tendency to neglect in mathematical practice taboos resulting from philosophical critique. This critique (maybe Pythagorean, maybe not) had once asked what number *really is* (a question which practical reckoners may never have asked, knowing number too well as the stuff they were always dealing with); it had been found that the only justifiable answer was that number was a *collection of units*. But learning thus what number really was entailed learning also what it *could not possibly*, and therefore *should not* be.[36] The Greek mathematicians has some difficulties in taking to heart the latter part of the lesson, as we have seen.

This was certainly not the last time in history that a mathematician cared more about conquering new mathematical ground (whether insights or results) than about consolidating philosophically what he already possessed. Accumulating a treasure in Popper's Third world often involves paying a price in the first, and even the philosopher may sometimes be in doubt whether the gain outweighs the loss: Søren Kierkegaard, as is known, tried to reestablish a bond with his former fiancée Regine once she had found a husband who did not write monumental books about why he should forsake her. Within mathematics, the price to be paid for respecting a critical taboo is the blocking of such further conceptual

35 Van der Waerden [1962: 108] was thus in excellent company when treating as mere "quibbles" the distinction between unity and numbers – quibbles with which there was no reason to burden an introduction to Greek arithmetic (as distinct from Greek philosophy of mathematics).

36 We may remember Vogel's demonstration [1936] that the whole terminology for ratios – claimed not to be *numbers* but *relations between pairs of numbers* (tacitly including unity) was derived from the terminology for fractions, in a way that shows ratios to be a way to save fractions in a philosophically acceptable way once they had been outlawed as numbers. It was a fortunate accident that this concept could later be extended beyond the scope of fractions, once the "ineffable" ratios turned up – those ratios that had no possible name within a language forged after the practice of fractions. In the Modern epoch the notion of decimal fractions (and later the critiques of Richard Dedekind and others) reopened the gates to the realm of numbers for both.

and operational developments as might spring from active use of the tabooed concepts or operations. The prevailing Greek proscription of fractions affected the development of arithmetic until the 17th century and beyond (even after being rediscovered, Diophantos remained peripheral on this account, modern “Diophantine” analysis treating of integers), just as the Egyptian outlawing of those repeated aliquot fraction which Greek calculators accepted prevented them from preparing the further steps we find with Diophantos; in other words, tabooing at one level may produce genuine conceptual divergence at the following. On the other hand, forgetting that *one* should be excluded from the realm of number along with fractions enabled the Greeks to do things that otherwise might have been forbiddingly cumbersome.

These observations suggest a further twist to the preceding argument. As argued in the bulk of the article, the absence of such conceptualizations from ancient sources as a modern mathematical reader might expect to find there does not prove that the ancient authors *were not able* to think more or less in our patterns – it may also be due to an explicit rejection of this way of thinking, either because of the existence of some canon or because they deemed it conceptually incoherent; only close analysis of the sources at large will, in the best of cases, allow us to distinguish between cognitive divergence and cognitive proscription. Conversely, however, the absence of critique where we would like to find it does not necessarily imply that the ancient authors were unable to realize the inconsistencies in what they did; they may have decided that they did not care about critical hairsplitting as long as things worked as they should – anticipating thus d’Alembert’s famous ⟦though possibly apocryphal⟧ recommendation to carry on work in infinitesimal calculus and his conviction that faith would result from success or habit.

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Chapter 20 (Article II.3) Tertium Non Datur, or, on Reasoning Styles in Early Mathematics

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

In much general historiography of mathematics, also of the fairly serious kind, Greek mathematics is presented not only as the beginning of systematic deduction and axiomatic presentation but also as the point where mathematics changed from a purely empirical activity to a practice based on reasoning and proof. In a number of recent writings defending the honour of "non-Western" mathematics, seen more or less as an undifferentiated lump, it is on the contrary claimed that there was nothing particular in the ancient Greek proofs, even though (it is paradoxically claimed) for instance the Indians had a different understanding of what constitutes a proof.

Basing itself in the main on the relation between Babylonian and Euclidean mathematics, the following tries to clarify the issue, making use of the notions of "naive" versus "critical" reasoning.

Contents

ÁRPÁD SZABÓ *in memoriam*

Two convenient scapegoats

Some philosophers of mathematics hold that real proof is quite recent and that, for instance, Euclid's arguments for the correctness of his theorems and constructions do not count as "proofs" (those contributing to the present volume are less dogmatic!). The rest of the world (in as far as it knows at all about the topic) sees things differently. Contemporary mathematicians may find Euclid's proofs insufficient or shaky, but they agree with their predecessors that Euclid's strings of arguments from the properties of the objects involved do constitute proofs.[1] According to this view, Greek theoretical geometry is thus based on proofs. Does that mean that mathematical proof was invented by the ancient Greeks (and, by tacit but rampant corollary, that it is thus yet another "proof' of "Western" superiority)?

Some writers on mathematics and its history have indeed claimed proof to be a Greek invention (without necessarily deducing from that the corollary that "our" saturation of selected spots of the world with napalm, cluster bombs and depleted uranium is morally justified). In [1972: 3, 14], Morris Kline wrote the following lines:

> Mathematics as an organized, independent, and reasoned discipline did not exist before the classical Greeks of the period from 600 to 300 B.C. entered upon the scene. There were, however, prior civilizations in which the beginnings or rudiments of mathematics were created.
>
> […]
>
> The question arises as to what extent the Babylonians employed mathematical proof. They did solve by correct systematic procedures rather complicated equations involving unknowns. However, they gave verbal instructions only on the steps to be made and offered no justification of the steps. Almost surely, the arithmetic and algebraic processes and the geometrical rules were the end result of physical evidence, trial and error, and insight.

Such blunt statements (as well as the less blunt but similar attitudes of many fellow writers) have called forth objections from other quarters.[2] An as example one may quote

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[1] Such "internal" arguments must of course be distinguished from other types of arguments. To say that something is true because it is stated by Euclid or in the Bible constitutes no mathematical proof; nor do deductions from metaphorical connotations of the terms involved or from metaphysical postulates – for instance, Cusanus's postulate that the maximal and the minimal coincide in combination with the observation that area measurement divides complex areas into triangles, from which follows that God must be triangular, that is, Trinity.

[2] In some sense these anti-Eurocentric objections have often been paradoxical, their aim being to show that "non-Western" cultures had the same kind of (meta-)mathematics as the Greeks: implicitly, the ideals of (what we read into) Greek mathematics are accepted.

As I shall argue in the end, certain mathematical cultures (not ethnic but professional cultures)

George Gheverghese Joseph's statement [1991: 89*f*] that if "the Greek dependence on Egypt and Babylonia is now recognized, the myth of the 'Greek miracle' will no longer be sustainable".[3] Unfortunately for Joseph's intended undermining of "one of the central planks of the Eurocentric view of history of progress" [1991: 90], the whole discussion of Egyptian and Babylonian mathematics is nothing but support for Kline's view.[4] Admittedly, Richard Gillings [1972: 233] is quoted to the effect that a

> nonsymbolic argument or proof *can* be quite rigorous when given for a particular value of the variable; the conditions for rigor are that the particular value should be *typical,* and that a further generalization to *any* value should be *immediate*

– but Joseph does not show that (nor discuss in which sense) the various *rules* applied to particular cases he quotes from Egyptian and Babylonian material can really be read as paradigmatic (or "potentially general") "argument or proof" in Gillings's sense.

In the following sections of the paper I shall show that much of Old Babylonian mathematics *was* indeed reasoned in this sense; characterize the type of reasoning involved; confront it with Euclidean reasoning about analogous cases; use this to characterize the approach of Greek theoretical geometry as embodied by Euclid's *Elements;* and briefly discuss a different type of Greek mathematical reasoning. In the end I shall widen the perspective toward other mathematical cultures.

Old Babylonian geometric proto-algebra

Kline as well as Joseph speak about "Babylonian mathematics" as if this entity remained the same as long as the Babylonian culture lasted; so did until very recently almost everybody who dealt with the topic without being a specialist of exactly this historical field. At closer inspection, however, there are important differences between the mathematics of the Old Babylonian and the Seleucid periods (c. 1900–1600 BCE and c. 300–100 BCE, respectively). The large majority of texts comes from the Old Babylonian period, on which I shall concentrate at first.

The Old Babylonian mathematical corpus consists of three parts: tables, tablets for rough numerical work, and problem texts. Only the third group is relevant for the present

have had the attitude that under particular circumstances some mathematics should *not* be reasoned, and have had it for a good reason.

3 Elsewhere, Joseph [1991: 125–129] goes into direct though imprecise polemic with Kline.

4 It is immaterial for the present purpose that they are often awfully wrong in details (terribly wrong datings, freely invented "translations", confusion of modern interpretation and ancient text, similar confusion between algorithm and theoretical algebra – see [Høyrup 1992]) and thus allow opponents of the author's general aim to conclude that no good arguments can be found in favour of the existence of non-Greek, not Greek-derived mathematics. In the view of anybody who shares the aim, this is of course the most serious shortcoming of the book.

discussion – actually only the "procedure" texts, texts which prescribe how to solve the problem stated in the beginning.

A large part of the problem texts have been understood since they were first interpreted in the 1930s to be of "algebraic" character.[5] Taken at their words, most of them deal with the measurable sides and areas of rectangles and squares, but these were taken to serve as mere dummies for unknown numbers and their products. Correspondingly, the operations that were performed were supposed to be arithmetical additions, subtractions, multiplications, etc. In this reading, the procedure descriptions look like mere prescriptions of numerical algorithms, with no indication of the way these have been found. A historian like Otto Neugebauer, who knew the corpus well, was fully aware that the procedures could not have been found without genuine mathematical reasoning, and presupposed that the texts had gone together with a system of oral instruction explaining the reasons for the steps; those general historians who knew only one or two simple examples in translation often believed that they had been found by trial and error (Kline, as we see, combines the two ideas).

Only a thorough investigation of the structure of the terminology and of the discursive organization of the texts reveals that the texts have to be taken at their geometrical words.[6] The problems are indeed (in a loose sense) homomorphic with those of numerical equation algebra, but many of the operations are geometric, not arithmetical.

As a first example we may look at the text YBC 6967, which contains a single problem dealing with two numbers *igûm* and *igibûm* belonging together in the table of reciprocals, "the reciprocal and its reciprocal". This problem thus illustrates another respect in which the technique is similar to modern equation algebra: a functionally abstract "basic representation" (with us abstract numbers, with the Babylonians measured or measurable ⟦but still functionally abstract⟧ segments and areas) is used to represent magnitudes

[5] The history of these interpretations is described in [Høyrup 1996a]. ⟦See also article II.5].⟧

[6] The first thorough exposition of this analysis is [Høyrup 1990]; equally thorough but probably more reader-friendly is [Høyrup 2002].

Part of the outcome of the structural analysis (and one of the reasons that the arithmetical interpretation breaks down) is the sharp distinction between two different additive operations (not merely synonyms for the same operation), between two different subtractive operations, two different halves, and no less than four different "multiplications". Since we shall encounter the additions below, they may serve as example. One of them I shall translate "appending", the other "accumulation". The former stands for a concrete joining to a magnitude which conserves its identity (in the same sense as addition of the interest conserves the identity of my bank account – interest on a loan *is* indeed called "the appended" in Babylonian); the other may be used about the purely arithmetical addition of the measuring numbers of ontologically different magnitudes – e.g., of lengths and areas, of areas and volumes, or of men, days, and bricks carried by the men in question during the days in question.

belonging to other ontological domains but involved in relations that are structurally similar to those characterizing the basic representation.

The text runs as follows in literal translation:[7]

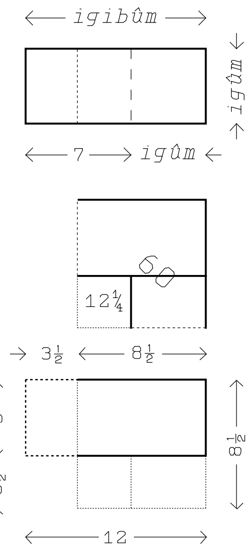

Figure 1. The representation of the *igûm-igibûm* problem of YBC 6967.

Obv.

1. [The *igib*]*ûm* over the *igûm,* 7 it goes beyond
2. [*igûm*] and *igibûm* what?
3. Yo[u], 7 which the *igibûm*
4. over the *igûm* goes beyond
5. to two break:[8] 3°30′;
6. 3°30′ together with 3°30′
7. make hold:[9] 12°15′.
8. To 12°15′ which comes up for you
9. [1` the surf]ace append: 1`12°15′.
10. [The equalside[10] of 1`]12°15′ what? 8°30′.
11. [8°30′ and] 8°30′, its counterpart,[11] lay down.

Rev.

1. 3°30′, the made-hold,
2. from one tear out,
3. to one append.
4. The first is 12, the second is 5.
5. 12 is the *igibûm,* 5 is the *igûm.*

---

[7] Based on the transliteration in [Neugebauer & Sachs 1945: 129]; as everywhere where no translator is indicated the English translation is mine. The numbers are expressed in a sexagesimal place value system (that is, a system with base 60), in which ′, ″ indicate decreasing and ` … `` increasing sexagesimal order of magnitude (and ° when needed "order zero"); 30′ is thus $30{\cdot}60^{-1} = ½$, 15′ = $15{\cdot}60^{-1} = {}^{1}/_{4}$. These indications of absolute order of magnitude are not present in the original – the number notation of the mathematical texts (obviously not that used in accounting and practical surveying!) is a floating-point system.

Words in [ ] are damaged on the tablet and reconstructed from parallel passages; words in ( ) are added for comprehensibility.

[8] "Breaking" is a bisection that produces a "necessary half", a half that could not have been chosen differently – e.g., that half of the base of a triangle that serves in area calculation. On the other hand, if a problem states that a square area and a half of the side are accumulated, the other, "accidental" half occurs – it might just as well have been a third.

[9] "Making *a* and *b* hold" stands for the construction of the rectangle contained by the sides *a* and *b* – henceforth ⊏⊐(*a*, *b*).

[10] The "equalside of *A*" (in the terms of other texts, that which "is equal along *A*") is the side of *A* when this area is laid out as a square; numerically it corressponds to the square root of *A*.

[11] The "counterpart" of an "equalside" is the side with which it has a corner in common.

What goes on may be followed in the diagram of Figure 1. We should expect the product of the two numbers to be 1, but it is actually meant to be 60 (whether due to the floating-point character of the number system or to the origin of the table of reciprocals as a tabulation of aliquot parts of 60 is uncertain). The two numbers are thus represented by the sides of a rectangle with area 1` (as obvious, e.g., from the reference to 1` in obv. 9 as a "surface". Since we are told that the *igibûm* exceeds the *igûm* by 7, the length of the rectangle exceeds the width by 7. This excess (with appurtenant section of the rectangle) is bisected, and the outer part moved around so as to contain together with the inner part a square □(3½), whose area will be 12¼ When the original rectangle (transformed into a gnomon) is joined to this, a square with area 60+12¼ = 72¼ is produced. The "equalside" of this area is 8½, and so is its "counterpart". When that part of the rectangle which was "made hold" is restored to its original position, we get the original length, the *igibûm,* which will thus be 8½+3½ = 12. But before we can restore it, we must remove it from the place where it was put; this removal produces the *igûm,* which must therefore be 8½−3½ = 5.

As we see, no attempt is made to discuss *why* or *under which conditions* the operations performed are legitimate and lead to the correct result. On the other hand it is intuitively obvious, once we are familiar with the properties of rectangles, that everything *is* correct. In this sense the prescription is, as formulated by Karine Chemla ([1991, 1996], and elsewhere) regarding Chinese mathematics, algorithm and proof in one.

The clay tablet contains no drawing; a few others do, but only as support for the statement, never as a supplement to the prescription. For this reason we cannot know the precise character of the diagrams that supported the reasoning – they may have been drawn in sand strewn on a brick floor, on a wall, or in any other medium that has not been conserved; we do not even know to which extent trained calculators would make actual drawings, and to which extent they would rely on mental geometry. We may be confident, however, that drawings were made use of at some stage of the instruction – mental geometry builds on previous experience with material geometry, just as mental addition of multi-digit numbers presupposes previous exposure to pen-and-paper algorithms for almost all of us; we may also be fairly confident that the diagrams in question were structure diagrams and not made carefully to scale – field plans, at least, had this character (see Figure 2, a plan from the 21st century BCE). As we see, only the right angles (those angles which are essential for the determination of areas) are rendered correctly; in general, the Babylonians seem not to have regarded angles as quantifiable magnitudes – expressed in a pun, an angle which was not "right" was simply considered "wrong".

The notion of a "naive" proof integrated in the algorithm may astonish us, but should not do so. How, indeed, will we normally treat the corresponding problem in symbolic algebra if we merely need to solve it? More or less in the following steps:

$$x–y = 7 \quad xy = 60 \tag{1}$$

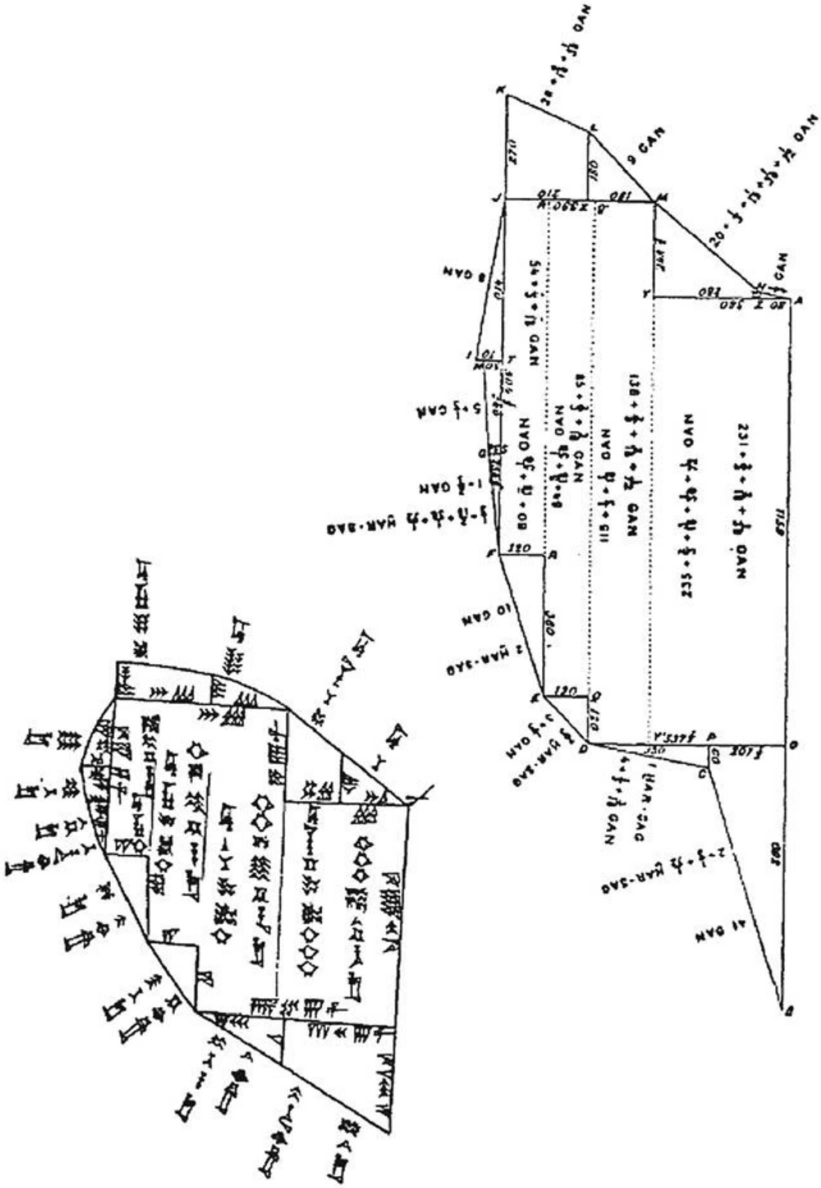

Figure 2. Field plan as drawn on the tablet (left) and in true proportions (right). From [Thureau-Dangin 1897: 13, 15].

$$\frac{x-y}{2} = 3\tfrac{1}{2} \tag{2}$$

$$\left(\frac{x-y}{2}\right)^2 = 12\tfrac{1}{4} \tag{3}$$

$$\left(\frac{x-y}{2}\right)^2 + xy = 12\tfrac{1}{4} + 60 = 72\tfrac{1}{4} \tag{4}$$

$$\left(\frac{x+y}{2}\right)^2 = 72\tfrac{1}{4} \tag{5}$$

$$\frac{x-y}{2} = \sqrt{72\tfrac{1}{4}} = 8\tfrac{1}{2} \tag{6}$$

$$x = \frac{x+y}{2} + \frac{x-y}{2} = 8\tfrac{1}{2} + 3\tfrac{1}{2} = 12 \tag{7}$$

$$y = \frac{x+y}{y} - \frac{x-y}{2} = 8\tfrac{1}{2} - 3\tfrac{1}{2} = 5 \tag{8}$$

We would obviously be able to justify every step if asked by somebody who did not follow the idea – but we would hardly justify the step from (3) to (4) with exact reference to the appropriate Euclidean axiom (or corresponding arithmetical theorem or axiom). Just as the Babylonian calculator, we thus proceed *naively;* so did any equation algebra until the advent of the Modern era. And just as that of the Babylonian calculator, our approach is *analytic:* we take the existence of the solution for granted, manipulate it as if it were known, and stop when we have disentangled the unknowns from the complex relationships in which they were involved.

Whereas the geometrical diagrams on which the reasoning was made have not survived, a few texts have transmitted the kind of explanations which must normally have been given orally. All are from Susa, a peripheral area (which may be the reason that explanations which elsewhere were transmitted within a stable oral tradition had to be put into writing). One – TMS XVI – explains the transformations of two linear equations.[12] The first transformation runs as follows in translation:[13]

1. [The 4th of the width, from] the length and the width to tear out, 45′. You, 45′

2. [to 4 raise[14], 3 you] see. 3, what is that? 4 and 1 posit,[15]

---

[12] The use of the term "equation" is no anachronism. The equations of a modern engineer or economist state that the measure of some composite magnitude equals a certain number, or that the measure of one magnitude equals that of another; exactly the same is done in the Babylonian texts.

[13] Based on the hand copy and transliteration in [Bruins & Rutten 1961: 91*f*, pl. 25], with corrections from [von Soden 1964]. Cf. revised edition of the full tablet in [Høyrup 1990: 299–302]. The translation in the original edition should be used with caution, and the commentary is best disregarded completely.

[14] "Raising" designates the determination of a concrete magnitude by means of a multiplication, and presupposes a consideration of proportionality. Originally the metaphor referred to the determination of a prismatic volume with height *h*, obtained by "raising" the base from its virtual height of 1 cubit (presupposed by the metrology, which measured volumes in area units) to the real height.

[15] "Positing" appears to mean "taking note of" materially, at times on a counting board, at times

3. [50′ and] 5′, to tear out, ⌈posit⌉. 5′ to 4 raise, 1 width. 20′ to 4 raise,

4. 1°20′ you ⟨see⟩, 4 widths. 30′ to 4 raise, 2 you ⟨see⟩, 4 lengths. 20′, 1 width, to tear out,

5. from 1° 20′, 4 widths, tear out, 1 you see. 2, the lengths, and 1, 3 widths, accumulate, 3 you see.

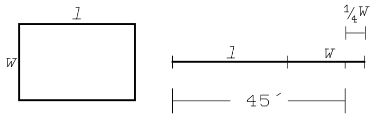

Figure 3. The situation of TMS XVI #1.

6. igi 4 de[ta]ch,[16] 15′ you see. 15′ to 2, lengths, raise, [3]0′ you ⟨see⟩, 30′ the length.

7. 15′ to 1 raise, [1]5′ the contribution of the width. 30′ and 15′ hold.

8. Since "The 4th of the width, to tear out", it is said to you, from 4, 1 tear out, 3 you see.

9. igi 4 de⟨tach⟩, 15′ you see, 15′ to 3 raise, 45′ you ⟨see⟩, 45′ as much as (there is) of [widths].

10. 1 as much as (there is) of lengths posit. 20, the true width take, 20 to 1′ raise, 20′ you see.[17]

11. 20′ to 45′ raise, 15′ you see. 15′ from ${}^{30}15'$[18] [tear out],

12. 30′ you see, 30′ the length.

The equation deals with the length (ℓ) and the width (w) of a rectangle – see Figure 3; in the actual case, however, this concrete meaning is relatively unimportant. In line 1, we are indeed told (in symbolic translation) that

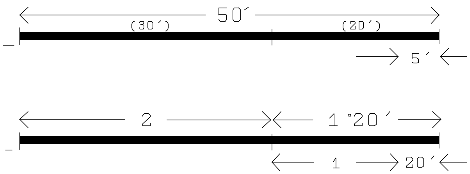

Figure 4. The transformations of TMS XVI #1.

$$(\ell+w)-\tfrac{1}{4}w=45'.$$

At first we are instructed to multiply the right-hand side by 4, from which 3 results. In line 2, the meaning of this number is asked for; the explanation given in lines 2–5 can be confronted with Figure 4, which may correspond more or less closely to something

by writing a length along a line as in Figure 2.

[16] igi n designates the reciprocal of n. For numbers where this was possible, division by n was performed as a raising to igi n (in administrative calculation it was always possible, since all technically relevant coefficients were rounded to numbers that possessed a convenient igi).

Finding igi n was spoken of as "detaching" it; the idea was probably that one part was detached from a bundle of n parts of unity.

[17] This step may refer to a distinction between a "real" field with dimensions 30 and 20 (180 m × 120 m, since the tacitly presupposed length unit was the "rod" equal to c. 6 m) and a "model field" 30′ × 20′, i.e., 3 m × 2 m, certainly more easily drawn in the school yard; since the text does not indicate absolute order of magnitude this must remain a hypothesis.

[18] This renders the non-standard way () in which "45" is written in this place in the tablet.

the author had in mind, and which is anyhow useful for us. As we observe, no problem is solved, the explanations presuppose (and the student is thus supposed to know) that the length is 30′ and the width 20′, their sum 50′ and the fourth of the width 5′.

In line 6, the equation is multiplied by ¼ from which follows both the "contribution of the width", that is, the value of the member $(1-\frac{1}{4})w$, and the *coefficients* ("as much as there is") of length and width. All in all, the explanations thus aim at giving concrete meaning to the outcome of the multiplication and to the original equation, not "proving" anything to be correct – no statement is involved which could be true or false except the claim that 4×45 = 3 – but making everything transparent, thus facilitating "naive" understanding of the correctness of procedures.

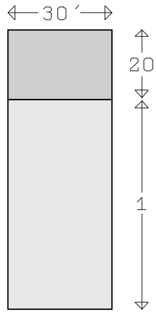

Figure 5. The configuration described in TMS IX #1.

Two other didactical expositions are found in the text TMS IX #1 and #2.[19] Both deal with geometry of the kind that was used to represent the *igûm* and *igibûm* in YBC 6967. They run as follows:

#1

1. The surface and 1 length accumulated, 4[0′. ¿30, the length,? 20′ the width.]
2. As 1 length to 10′ ⌈the surface, has been appended,]
3. or 1 (as) base to 20′, [the width, has been appended,]
4. or 1°20′ [¿is posited?] to the width which 40′ together ⌈with the length 'holds?']
5. or 1°20′ toge⟨ther⟩ with 30′ the length hol[ds], 40′ (is) [its] name.
6. Since so, to 20′ the width, which is said to you,
7. 1 is appended: 1°20′ you see. Out from here
8. you ask. 40′ the surface, 1°20′ the width, the length what?
9. [30′ the length. T]hus the procedure.

#2

10. [Surface, length, and width accu]mulated, 1. By the Akkadian (method).
11. [1 to the length append.] 1 to the width append. Since 1 to the length is appended,
12. [1 to the width is app]ended, 1 and 1 make hold, 1 you see.
13. [1 to the accumulation of length,] width and surface append, 2 you see.
14. [To 20′ the width, 1 appe]nd, 1°20′. To 30′ the length, 1 append, 1°30′.
15. [¿Since? a surf]ace, that of 1°20′ the width, that of 1°30′ the length,
16. [¿the length together with? the wi]dth, are made hold, what is its name?
17. 2 the surface.
18. Thus the Akkadian (method).

19 Based on the transliteration and hand copy in [Bruins & Rutten 1961: 63*f*, pl. 17], with corrections from [von Soden 1964]. Cf. revised edition in [Høyrup 1990: 320–323]. Even in this case, the translation and the commentary in the original edition ask for benign neglect.

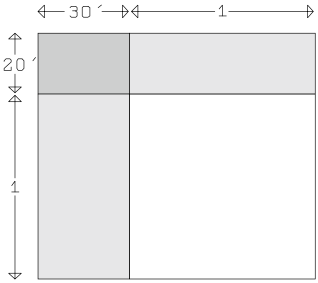

Figure 6. The configuration of TMS IX #2.

In #1, as we see, we are told that the arithmetical sum of the length and the area of a rectangle is $A+\ell = 40'$; once again, the explanation of what goes on presupposes the student to know that the length is $30'$ and the width $20'$. The text then explains how this is to be given a concretely meaningful interpretation. The trick is to replace the length ℓ by a rectangle ⊏⊐$(1,\ell)$, which corresponds to joining an extra "base 1" to the width, as shown in Figure 5 (the orientation of which follows from the designation of the extension as a "base"). The resulting total "width" is $1°20'$; since the total area is $40'$, this is seen to correspond to the length $30'$, as it should.

In #2, we are told instead the arithmetical sum of the length, the width and the area, $A+\ell+w = 1$. Once again, the dimensions are presupposed to be known, $\ell = 30'$, $w = 20'$, as can be seen in line 14. This time we are told to add ⊏⊐$(1,1) = 1$ to the sum $A+\ell+w$; the result is then shown to be the area of a new rectangle with length $L = 1+30' = 1°30'$, width $W = 1+20' = 1°20'$ – cf. Figure 6.[20] This section of the text is said to explain the "Akkadian method"; since the trick that distinguishes #2 from #1 is the joining of a quadratic complement to a (pseudo-)gnomon, the "Akkadian method" is likely to be exactly this trick, basic for the solution of all mixed second-degree problems. Once again, the exposition serves to make clear why and how the methods works.

#3 of the tablet, the last problem and a problem in the proper sense (omitted from the quotation), combines the equation of #2, $A+\ell+w = 1$, with an equation of the same type as the one explained in TMS XVI though more abstruse – namely

$$\tfrac{1}{17}(3\ell+4w)+w = 30'.$$

This is reduced, now without didactical explanation, to

$$3\ell+21w = 8°30',$$

after which the corresponding equation for "the length and width of the surface 2" (L and W) is derived,

[20] This presence of several "lengths" and "widths" shows why the exposition needs to presuppose that the measures of the configuration are known: these measuring numbers serve as identifying tags, and are needed for this purpose in the absence of letter or similar symbols.

Even many genuine problem texts refer to the value of certain entities before they are found. This may give the impression that the problems are overdetermined and their authors hence mathematically incompetent. That, however, is a mistaken reading: the information which is *made use of* never exceeds what is necessary; this constitutes the set of "given numbers", which is always kept strictly apart from those numbers which are "merely known" and used as identifiers.

$$3L + 21W = 32°30'.$$

Since ⊏⊐(L,W) = 2, ⊏⊐(3L,21W) is found to be 2·3·21 = 1`3 (i.e., 63), and in the end the resulting rectangle problem for $\Lambda = 3L$, $\Omega = 21W$,

$$\Lambda + \Omega = 32°30', \quad ⊏⊐(\Lambda, \Omega) = 1`3$$

(the additive analogue of the problem solved in YBC 6967) is solved, and first L and W, next ℓ and w are found. No didactical explanation of how to solve the rectangle problem is extant, but we may safely assume that such an explanation was at hand and that its style was similar to what we know from TMS XVI and TMS IX #1–2.

Before we leave the Old Babylonian period it should be pointed out that certain aspects of the procedure descriptions reflect the presence of "critique", that is, the question about the reasons for and the limits of the validity of the procedure; this question is the antithesis of the "naive" approach. One instance is the precedence of "tearing-out" over "appending" in YBC 6967, rev. 2–3, the other the explicit introduction of the "base 1" in TMS IX #1 ⟦cf. article II.2⟧.

That these features of the text are "critical" only becomes visible when the historical development of Old Babylonian "algebra" is understood, which requires another structural analysis of the corpus, this time associating the distribution of synonyms and characteristic phrases with orthography and what (little) is known about the archaeological provenience of tablets (most, indeed, have been bought by museums on the antiquity market), and correlation of the problems found in the Old Babylonian corpus with those found in a number of other historical contexts (Seleucid and other Late Babylonian problem texts, ancient Greek theoretical, Neopythagorean and practitioners' mathematics, Arabic algebra and agrimensorial texts, Jaina and Italian *abbaco* mathematics). I shall not attempt to reduce the necessary complex arguments to what can be contained in a few paragraphs[21] but only sum up the relevant results ⟦but see article II.8⟧.

In the later third and incipient second millennium BCE, a restricted number of geometrical riddles circulated in a lay (that is, non-scribal, non-schooled) and probably Akkadian-speaking[22] environment of surveyors/practical geometers. A number of these were to

[21] The structural analysis of the corpus is described in [Høyrup 2000b], and (with some extensions and minor revisions) in [Høyrup 2002: 317–361]. The place of Old Babylonian "algebra" in the network of mathematical cultures was first investigated in [Høyrup 1996b]; a more thorough exposition is [Høyrup 2001] ⟦= article I.3⟧. Information on the latter topic is also given in [Høyrup 2002: 362–417, *passim*].

[22] The hegemonic and scribe school language of the third millennium was Sumerian. However, the presence of Akkadian (later represented by a Babylonian and an Assyrian dialect) is attested already before 2500 BCE, gradually rising to become the dominant language in the early second millennium. With extremely few exceptions the language of the Old Babylonian mathematical texts

be solved by means of the kind of naive cut-and-paste geometry which we have encountered in YBC 6967 and by application of the trick of quadratic completion (thus for good reason designated the "Akkadian method" in TMS IX; the trick seems to have been discovered at some moment before c. 1900 BCE, and probably after c. 2200 BCE): to find the side of a square from the sum of the side or "all four sides" and the area, or from the difference, one or the other way around; to find the sides of a rectangle from the area and the diagonal or from the area together with the sum of or difference between the sides (with a few variants); problems dealing with two concentric squares (with given sum of/difference between the sides and the areas) were apparently solved by means of standard diagrams.

In the 19th century BCE, these problems were adopted into the Old Babylonian scribe school, where they gave rise to the development of the so-called "algebra" (which is much more refined than can be seen from the above examples: solving mixed third-degree problems by means of factorization – reducing biquadratic problems and even a bi-biquadratic problem stepwise – inverting the role of unknowns and coefficients – etc.). As it turns out, those text groups which are closest to the lay tradition do not respect the "norm of concreteness" according to which "tearing-out" must precede "appending" of the same entity but use the elliptic phrase "append and tear out"; some early texts, moreover, follow the habit of many non-Mesopotamian lay surveying traditions and operate with a notion of "broad lines", that is, with the idea that a line carries an inherent standard width[23]. For this reason, they are able to "append" sides to areas, which indeed they do.

The school environment, however, appears to have found it difficult to accept the conflation of linear and planar extension, and therefore formulated the inhomogeneous sums as "accumulations" (namely, of the measuring numbers), devising moreover a variety of designations for the standard width which transforms a side into a rectangular area[24]. Some schools also seem to have found it absurd to "append" something which is not yet at hand, and therefore introduced the "norm of concreteness". If "critique" is understood as investigations of *why* and *under which conditions* our usual naive ways and conventional wisdom hold good,[25] then these are full-blown examples.

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is Akkadian, though the writing often makes heavy use of Sumerian word signs (as English writing may make use of the medieval word sign for Latin *videlicet,* rendered as *viz* yet presupposing a pronunciation "namely").

[23] As does cloth today, when we buy "three yards of curtain material". The notion of the "broad line" and its appearance in a number of practical geometries is examined in [Høyrup 1995] [translated as article I.7].

[24] One of these designations is the "base" of TMS IX #1; but at least two alternatives are attested in the corpus.

[25] "Untersuchung der Möglichkeit und Grenzen derselben", as expressed in Kant's *Critik der*

The chronological dissection of the Old Babylonian corpus allows a final observation of importance for our topic.[26] All above examples were formulated around paradigmatic cases, yet in agreement with Gillings's criteria for when an argument from a paradigmatic case can be considered rigorous – cf. p. 540. This is no accident: almost all Old Babylonian mathematical texts that present us with explicit or implicit arguments have this character. There are, however, exceptions, and a few texts do indeed formulate rules in general terms. These rules may build on insight and argument, and can hardly have been invented without the intervention of some kind of mathematical insight; the rules themselves, however, only prescribe steps to be performed, and contain no trace of an argument.[27] Interestingly, all such attempts at general formulation belong in the earliest texts. The way such rules turn up in the sources suggest that they were a borrowing from the lay tradition, within which they may indeed have been very useful.[28] Within the school, however, they were soon eliminated, being both ambiguous when not supported by an example and pedagogically useless (probably because they were deprived of argument). The absence of abstract general rules is thus, like the compliance with the norm of concreteness, no consequence of a primitive mind unable to free itself from concrete thought; to the contrary, both have resulted from deliberate pedagogical or philosophical choice.

Euclidean geometry

Figure 1 is quite similar to the diagram of *Elements* II.6 – see Figure 7. Since the underlying mathematical structures are also analogous (to the extent a problem can be analogous to a justification of the way it is solved), it seems obvious to look closer at this Euclidean proposition.

In Thomas Heath's faithful translation [1926: I, 385] it states the following:

> If a straight line be bisected and a straight line be added to it in a straight line, the rectangle contained by the whole with the added straight line and the added straight line together with the square on the half is equal to the square on the straight line made up of the half and the added straight line.

Urtheilskraft (B III [Werke V, 237]).

26 See [Høyrup 2002: 344, 383, and *passim*].

27 Nor should they, this is not the nature or purpose of a rule – our multiplication table contains no hint of the role of associativity and distributivity of the operations involved.

28 The general rule is an adequate tool for an oral tradition, being more easily remembered mechanically and transmitted faithfully than the full paradigmatic example; explanations and examples can then be improvised once the master knows what is meant by a possibly ambiguous rule. A parallel is offered by the relation between fixed formulae and relatively free use of these by the singer in oral epic poetry, see [Lord 1960: 99–102 and *passim*]).

Next follows what Antiquity would apparently see as a particular example with indubitable paradigmatic value[29] but which Kline (and most modern readers) have come to regard as actually and not only potentially general:[30]

> For let ⟨the⟩ straight line *AB* be bisected at the point *C,* and let ⟨the⟩ straight line *BD* be added to it in a straight line; I say that the rectangle contained by *AD, DB* together with the square on *CB* is equal to the square on *CD.*

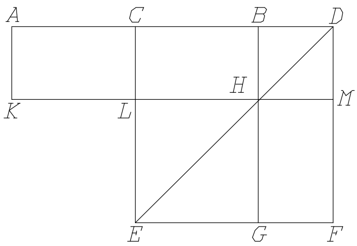

Figure 7. The diagram of *Elements* II.6.

The proof starts by constructing the latter square (*CEFD*) and drawing the diagonal *DE.* Next through *B* the line *BHG* is drawn parallel to *CE* or *DF* (*H* being the point where the line cuts *DE*) and through *H* the line *KM* parallel to *AB* or *EF.* Finally, through *A* the line *AK* is drawn parallel to *CE* or *DF.*

Now the diagram is ready, and with reference to the way the construction was made ⊏⊐*AL* is shown to equal ⊏⊐*HF*. Adding ⊏⊐*CM* to both, the gnomon *CDFGHL* is seen to equal ⊏⊐*AM*. Further addition of □*LG* shows that ⊏⊐*AM* together with □*LG* equals □*CF*, as stated in the theorem.

The second part of the proof follows the pattern of the cut-and-paste procedure of YBC 6967 precisely. The important difference is the presence of the first part. Thanks to this, things are not just "seen", they are as firmly established as required by the norms of Greek geometry – we do not move areas around and glue them together, we *prove* that one area (⊏⊐*AL*) is equal to another ⊏⊐*HF*). Even the fact that the gnomon *CDFGHL* together with □*LG* is identical with □CF, though not argued in detail, could be proved rigorously by repeated use of proposition II.1.

The first part of the proof of proposition II.6 can thus be seen as a *critique* which consolidates the well-known. Other propositions and proofs from the sequence *Elements* 11.1–10 invite to make similar observations and interpretations. To this we may add that the riddles of the surveyors' tradition were doubtlessly known in classical Antiquity – as we shall see (below, p. 555), the riddle of "the four sides and the area" turns up in the pseudo-Heronian *Geometrica.* The whole sequence repeats matters that were familiar in the surveyors' tradition at least since the earliest second millennium BCE; many of the propositions, moreover, are never used explicitly later on in the work, which supports

29 Apart from the use of the habitual format rule-example and the precise wording, this interpretation is supported, for instance, by Aristotle's analogous reference to geometric arguing which is correct if only we avoid including in the premises we draw on the particular characteristics of the drawing made on the ground (*Metaphysics* M, 1078^{a}19–20). See also the detailed discussions in [Mueller 1982: 11–14] and [Netz 1999: 247-258 and *passim*].

30 Trans. [Heath 1926: I, 385], with minor corrections in ⟨ ⟩.

the interpretation that their critical consolidation was an aim in itself. Finally, all are proved independently, although a derivation of one from the other would often have been easy (actually, II.5 and II.6 are equivalent, and so are II.9 and II.10); what needs to be consolidated is thus not only the customary knowledge contained in the propositions but also the traditional naive-geometric *argument*.[31]

Greek theoretical geometry as a whole was evidently much more than a consolidation of the well-known; in as far as its ideals of what constitutes a *proof* are concerned, however, book II of the *Elements* may be regarded as representative. In aiming at critique of the already familiar it is certainly no first in the history of mathematics – as we have seen, something similar was made in the Old Babylonian scribe school, and it is part of the dynamics of any institutionalized teaching of mathematics at levels where appeals to the reasoning of the students are required.[32] In the Old Babylonian school, however, the role of critique had been peripheral and accidental; in Greek theoretical geometry it was, if not *the* very centre then at least an essential gauge.[33]

Stations on the road

In the Old Babylonian mathematical texts we find names for particular lines (lengths, widths, various transversals, etc.); but we find no term for linear extension in general. Nor is any term for an angle (or a right angle) to be found. This does not mean that surveyors could not speak about lines unless they were already defined as the length or width of a field, the length or height of a wall, a carrying distance, etc., nor that they were unable to refer to the corner of a building; but *tubqum* ("corner") was not used as a technical term in mathematics. In general, it is doubtful whether the terminology of Old Babylonian mathematics can at all be characterized as "technical". Instead, as concluded in [Høyrup 2002: 302],

[31] Being necessarily ignorant of the whole prehistory, Heath [1926: I, 377] formulated this as follows:

> What then was Euclid's intention, first in inserting some propositions not immediately required, and secondly in making the proofs of the first ten practically independent of each other? Surely the object was to show the power of the *method* of geometrical algebra as much as to arrive at results.

[32] This topic is dealt with in [Høyrup 1985], and, more crudely but more precisely in aim and with broader historical scope, in [Høyrup 1980].

[33] In the introduction to the *Method*, Archimedes argues that "we should give no small part of the credit to Democritus who was the first to make the assertion [that the cone is the third part of the cylinder, and the pyramid of the prism] though he did not prove it" [trans. Heath 1912: 13]. The rhetoric of the argument implies that the opposite attitude prevailed; rhetoric may distort things but becomes ineffective if the recipient knows that it is fully off the point – which we may therefore suppose that it was *not,* the recipient (Eratosthenes) being as conversant as anyone with both the mathematics and the norms that governed it at his times.

> it is rather a very standardized use of everyday language to describe an extra-linguistic – computational and naive-geometrical – practice which was always *more* standardized than the linguistic description. The linguistic description was thereby analogous to our heuristic explanations in standardized ordinary language of what goes on in those symbolic formulae which with us constitute the level of real technical operation.

An early step in the unfolding of Greek theoretical critique was the establishment of definitions. Irrespective of Aristotle's claim that Socrates "was the first to concentrate upon definition",[34] discussions of semantic delimitations go back as far in Greek (proto-) philosophy as we can follow it – a very early example is Hesiod's pointing out in *Works and Days* [ed., trans. Mazon 1979: 86] that the word "strife" (ἔρις) corresponds to two very different things (namely peaceful competition and cruel war). The definition of number as a "multitude composed of units"[35] is likely to go back at least to the fifth century BCE, and many other definitions were known to, and discussed by, Plato and Aristotle. Of particular interest are the definitions of the various classes of (rectilinear) angles [trans. Heath 1926: I, 181]:

10. When a straight line set up on a straight line makes the adjacent angles equal to one another, each of the equal angles is *right* […].
11. An *obtuse angle* is an angle greater than a right angle.
12. An *acute angle* is an angle less than a right angle.

These were known to Aristotle, who refers to them in *Metaphysics* M 1084^{b}7. But they may have been a relatively fresh invention in his days, since Plato's Socrates speaks in *Republic* VI, 510C [trans. Shorey 1930: II, 111] of the three kinds of angles as things of which geometers "do not deign to render any further account to themselves or others, taking it for granted that they are obvious to everybody".[36]

A clear notion of a right angle is evidently essential for making proofs like that of *Elements* II.6. In Aristotle's times the above definition was apparently supposed to be sufficient. This follows from what can be derived from Aristotle's writings about the status of the Euclidean postulates. On the whole, he does not seem to have heard of them [McKirahan 1992: 133–137], which would suggest that their need had not yet been felt.

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34 *Metaphysics* A, 987^{b}3, trans. [Tredennick 1933: I, 43]. The Greek term is ὁρισμός, related to the Euclidean term ὅρος, the former meaning something like "delimitation"/"marking out by boundaries", the latter "limit"/boundary.

35 Itself an outcome of critique, which remained fateful for more than 2000 years and encumbered the theoretical justification of *algebra* in the early Modern era, since this attempt to make unambiguous and stable sense of the notion of a number excluded both *1* (a fact which Euclid forgets when defining a "part" in *Elements* VII, immediately after he has repeated the habitual definition of a number!) but also fractions.

36 The passage may also mean, however, that they allow no further discussion *beyond* the definitions they have given, in which case the definitions will obviously have been older.

Only the second postulate appears to have been known to Aristotle in a formulation close to what we find in the *Elements* – *Physics* III, 207^{b}29–31 [trans. Hardie & Gaye 1930] explains that mathematicians "do not need the infinite and do not use it. They postulate only that the finite straight line may be produced as far as they wish".

This implies that no need had as yet been discovered around the mid-fourth century for postulate 4, "that all right angles are equal to one another", and thus, since this principle is essential for a large number of proofs of the equality of figures, that it was tacitly believed to be inherent in the definition. In Euclid's time, on the other hand, it was recognized that this was not the case. Although critique may have been just as compulsory for Greek geometers of the early fourth century as for their third-century successors, the level at which critique was actually performed was raised in the historical process – which of course cannot astonish if we recognize that mathematical rigour is a human product in process, never absolute and never finished once and for all.

Other Greeks

The community of "theoreticians" (however that was delimited) was not the only community of the classical world to deal with mathematics. On one hand, the social need for mathematical practitioners was certainly not lower than it had been in the older Egyptian and Mesopotamian civilizations (nor probably significantly higher); on the other, the diffuse area encompassing Neopythagoreanism, Hermeticism, Gnosticism and Neoplatonism was also fond of mathematical metaphors and astounding mathematical insights.[37] In sources stemming from either community, instances or traces of mathematical reasoning can be located. In both cases what we find is naive, not critical. I shall present one example from each.

The first, belonging to the practitioners' tradition, comes from the pseudo-Heronian *Geometrica*.[38] It is a Greek version of the riddle of "all fours sides and the area":

[37] [Cuomo 2000] is a pioneering investigation of the situation and interplay of these groups in late Antiquity, in particular as reflected in Pappos's *Collection*.

[38] *Geometrica* 24:3, ed. [Heiberg 1912: 418], photographic reproduction of the manuscript [Bruins 1964: I, 53]. As with the Babylonian texts, my translation is meant to be pedantically literal. Actually, we should speak of "Heiberg's" rather than of any pseudo-Heron's *Geometrica.* Heiberg produced the bulk of the conglomerate from two ancient treatises which were already composite and cannot be traced back to a common source (as told quite explicitly by Heiberg, but in Latin and in a different volume of the Heronian *Opera omnia* [Heiberg 1914: xxiii–xxiv], for which reasons the fact has generally gone unnoticed). These two treatises are represented, respectively, by Heiberg's mss A+C and mss S+V. Chapters 22 and 24, however, are independent treatises (24 another conglomerate) which happen to be contained in the same codex as *Geometrica*/S but at a distance. See [Høyrup 1997: 77] ⟦= article I.9⟧.

> A square surface having the area together with the perimeter of 896 feet. To get separated the area and the perimeter. I do like this: In general (καθολικῶς, i.e., independently of the parameter 896 – JH), place outside (εκτίθημι) the 4 units, whose half becomes 2 feet. Putting this on top of itself becomes 4. Putting together just this with the 896 becomes 900, whose squaring side becomes 30 feet. I have taken away underneath (ὑφαιρέω) the half, 2 feet are left. The remainder becomes 28 feet. So the area is 784 feet, and let the perimeter be 112 feet. Putting together just all this becomes 896 feet. Let the area with the perimeter be that much, 896 feet.[39]

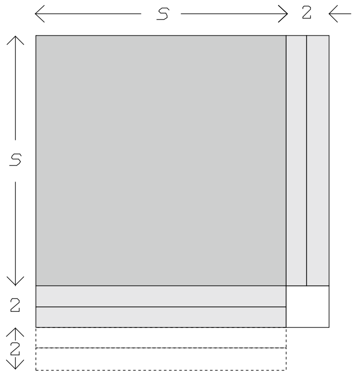


Figure 8. The procedure described in *Geometrica* 24.3.

The procedure that is described is shown in Figure 8 (the manuscript only contains a drawing of a square with inscribed values for the side and the area; apparently, the geometry is meant to be either mental or performed independently by the reader[40]). As we see, the procedure is identical with what we have seen in the text YBC 6967, apart from those details that follow from the fact that we are dealing with a square and not with a rectangle. The style is certainly reasoned: "I have taken away underneath the half, 2 feet are left. The remainder [when these too are removed] becomes 28 feet"; but it is fully naive. The text also points out which numbers belong to the type *in general* (square area and perimeter) and do not depend on the particular parameters of the example, safeguarding thus potential generality; this is currently done in the various *Geometrica*-components and also in kindred medieval treatises, and already in one text from Old Babylonian Susa.

The various Neopythagorean writings are less generous when it comes to revealing the reasoning behind the mathematical facts they relate – maybe because astounding mathematical facts, once we understand their grounds, tend to be less astounding and therefore less serviceable for the display of wisdom beyond ordinary human reason ⟦see the overview in article I.10⟧. Sometimes, however, reasons shine through. One interesting case is found in Iamblichos's commentary to Nicomachos's *Introduction:*[41] namely

39 Heiberg does not grasp the geometrical procedure that is described, for which reason his commentaries are misguided, imputing the faulty understanding on the ancient copyist.

40 This is also the case in the *Liber mensurationum,* an Arabic treatise building on the surveyors' tradition (known from Gerard of Cremona's Latin translation, ed. [Busard 1968]): the sequence of problems about squares starts by a drawn square, that of rectangle problems with a rectangle, etc. Only a few 14th- and 15th-century Latin and Italian descendants of the tradition contain drawings illustrating the whole procedure.

41 Ed. [Pistelli 1975: 75$^{25–27}$], cf. [Heath 1921: 113*f*].

the observation that 10×10 laid out as a square and counted "in horse-race" (see Figure 9) reveals that

$$10 \times 10 = (1+2+\ldots+9)+10+(9+\ldots+2+1)$$

whence

$$10 \times 10+10 = 2T_{10},$$

T_n being the triangular number of order n. This argument will have been common Pythagorean or Neopythagorean lore, if we are to believe Iamblichos's exposition, though hardly a discovery made within this environment.[42] In any case, the naive type of reasoning will not have been left behind when the Pythagorean scientologists took over from existing mathematics that which they managed to understand (which could be neither the theory of *Elements* X, Apollonian *Conics*, Archimedean infinitesimal methods, nor "Heron's" formula for the triangular area).

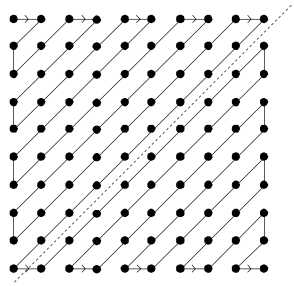


Figure 9. 10×10 arranged as a "race-course".

Proportionality – reasoning and its elimination

Does this mean that mathematics is always in some way reasoned, either naively or critically? In some sense yes, simply because we are unlikely to count as "mathematics" activities which are wholly devoid of understanding, however much they have to do with countable items or take place in geometrical space. But mathematics need not always *be taught,* nor to *be exercised* as a reasoned practice. When learning to drive a car you probably received a number of instructions and explanations, about changing gears, about braking and aquaplaning, etc. But woe to your passengers if you use your conscious mental reserves too intensively on thinking about these matters when you move in the traffic.

A mathematician behaves no different. Most of the transformations of symbolic expressions are performed automatically, leaving energy for conscious reflection on the more intricate and still unfamiliar aspects of the problem that is treated; the activity of

42 Firstly, in belongs squarely within the style of *psēphos* arithmetic that can be presupposed to be at the basis of the "doctrine of odd and even"; this was generally familiar at too early a moment to be Pythagorean – Epicharmos Fragment B 2 ([Diels 1951: I, 196; earlier than c. 475 BCE]) refers to the representation of an odd number ("or, for that matter, an even number") by a collection of *psēphoi* as something trivially familiar. Secondly, the ensuing formula for the triangular number,

$$T_n = \frac{n^2+n}{2},$$

belongs no less squarely within a cluster of summation formulae shared between Seleucid and Egyptian Demotic sources which betray no Greek influence in any other respect [Høyrup 2000a]. Together with the whole technique of *psēphos*-based reasoning it is thus almost certainly a borrowing from Near Eastern practical mathematicians.

the mathematician thus remains reasoned, if only at a higher level.

But the routine activity of the mathematical practitioner may be different in character. Remaining in the pre-Modern epoch, we may illustrate this through a look at the way simple linear problems were dealt with.

A typical late medieval rule for solving such problems can be found in Jacopo da Firenze's *Tractatus algorismi* from 1307.[43] It runs as follows:

> If some computation should be given to us in which three things were proposed, then we should always multiply the thing that we want to know against that which is not similar, and divide in the third thing, that is, in the other that remains.

After this follows a sequence of examples, beginning with this:

> I want to give you the example to the said rule, and I want to say thus, VII *tornesi* are worth VIIII *parigini*.[44] Say me, how much will 20 *tornesi* be worth? Do thus, the thing that you want to know is that which 20 *tornesi* will be worth. And the not similar (thing) is that which VII *tornesi* are worth, that is, they are worth 9 *parigini.* And therefore we should multiply 9 *parigini* times 20, they make 180 *parigini,* and divide in 7, which is the third thing. Divide 180, from which results 25 and $\frac{5}{7}$. And 25 *parigini* and $\frac{5}{7}$ will 20 *tornesi* be worth. And thus the similar computations are done.

This is the rule of three, and may be familiar. ⟦The rule and its presence in various cultures is treated in detail in article I.5⟧. But try to explain why it works without using paper and symbolic manipulations to somebody who is not too well trained in mathematics![45] The reason for the difficulty is of course that the intermediate result 9 *parigini* × 20 *tornesi* has no concrete interpretation.

Babylonian, Egyptian and ancient Greek calculators would have proceeded differently. Their normal procedure would have been to divide first (by whatever method they would use for division) 9 *parigini* by 7 *tornesi.* The result has an obvious concrete interpretation, the value of 1 *torneso* in *parigini.* Next, this could be multiplied by 20 in order to find the value of 20 *tornesi.*

Why was this easy and didactically efficient procedure given up? The key is inherent in the remark "by whatever method …". Division is difficult, and often leads to rounding (either for reasons of principle, namely if you have to multiply by a non-exact reciprocal, or because it may lead to a very unhandy string of aliquot parts). Subsequent multiplication will lead to multiplication of the rounding error, quite apart from the practical difficulty of multiplying an inconveniently composite numerical expression. Better therefore postpone the division and make it the last step.

43 MS. VAT Lat. 4826, fol. 17r. I translate from my own transcription of the manuscript [1999].

44 The *parigino* and the *torneso* are coins, minted in Paris and Tours, respectively.

45 Or observe how even the mathematically well-trained person grasps a piece of paper and starts writing down symbols when confronted with the verbal rule!

Why, then, was it not given up before?[46] Once again, the explanation is straightforward and of a practical nature. It was set forth by Christian Wolff alias Doktor Pangloss in his *Mathematisches Lexikon* [1716: 867]:

> It is true that performing mathematics can be learned without reasoning mathematics; but then one remains blind in all affairs, achieves nothing with suitable precision and in the best way, at times it may occur that one does not find one's way at all. Not to mention that it is easy to forget what one has learned, and that that which one has forgotten is not so easily retrieved, because everything depends only on memory

– in other words, only procedures that are performed so often that you run no risk of forgetting them (like changing gears in a car) can be safely taught as mere skills. Probably the scribes of Near Eastern Antiquity did not perform the kind of proportional operations we are speaking of so often that the appeal to their understanding could be given up safely.

More complex linear problems were often solved by means of the so-called "double false position", which is even more opaque. The intelligible alternative to this rule can be illustrated by another quotation from Jacopo (fol. 22r):

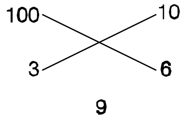
> I have new *fiorini* and old *fiorini.* And the *old fiorino* is worth *soldi* 35, and the new *fiorino* is worth *soldi* 37. And I have changed 100 *fiorini* new and old together, and I have got for them *libre* 178. I want to know how many new *fiorini* and how many old *fiorini* I had. Do thus, posit the case that all were of one of these rates, that is, all 100 of whatever rate you want. And let us say that they are all 100 old *fiorini.* And know how much they are worth for *soldi* 35 each, they are worth 175. Now say thus, from 175 until 178 there is *libre* 3, which are *soldi* 60. Now divide *soldi* 60 in the price difference which there is from one *fiorino* to the other, that is, from 35 *soldi* until 37, which is 2. Divide 60 in 2, 30 results. And 30 *fiorini* shall we say have been of the opposite (sort) of those {…} which we said were all old. And therefore we shall say that these 30 have been new, and the rest until 100, which is 70, have been old. And thus I say that they were.

This is easily understood (once you know that 1 *libra* is worth 20 *soldi*) – and precisely the same method (starting only from a fifty-fifty assumption) is used in the Old Babylonian problem VAT 8389 #1 [ed. Neugebauer 1935: **I**, 317*f*, III, 58]. If the double false position had been applied, the procedure had been much less comprehensible. One false assumption might be that all were old, in which case they would have been worth 3500 *soldi* = 175 *libre* – three less than I really get. The other false assumption might be that only 10 were old[47] and 90 hence new; in this case, I would have got 184 *libre.* The whole thing

[46] In fact it was – but in India, where the characteristic terms of the rule of three can be traced back to c. 400 BCE [Sarma 2002], and in China, where it is introduced in chapter 2 of the *Nine Chapters on Arithmetic* from the first century CE [trans. Vogel 1968: 18*ff*]. Medieval Arabic mathematicians (and probably practical reckoners) borrowed it from India.

[47] The Indians might have chosen that none were old, since they operated with both zero and negative numbers; but this simple choice was not accessible around the Mediterranean.

might be inserted in a graphical scheme



in which you were to perform a cross-multiplication, add and divide by the sum of the two errors as written at bottom,[48] finding the real number of old *fiorini* to be $\frac{100\times6+10\times3}{9} = 70$.

The principle can be explained as a linear interpolation; the real origin may be the alligation rule. But the texts never give any explanation, they simply set it forth as a rule to be followed. The obvious danger is that it may happen to be applied to non-linear situations, and that the reckoner would have no possibility to know that this was wrong.[49]

The moral is that Doktor Pangloss was right as soon as we get beyond the most routine applications of mathematics. A fundament in reason *is* an advantage not only in mathematical theory (where it belongs to the definition and is thus no mere advantage) but also in every application that goes beyond complete routine. It is therefore to be expected that mathematics teaching in any mathematical culture which went beyond mere routine (on its own conditions for what could constitute routine) did include appeals to reason – whether naive or critical, and whether in Greek style (or that dubious reading of the Greek style in which we project ourselves) is a different matter. If we cannot find traces of this reasoning in extant sources we may safely conclude that this is due, *either* to failing understanding of the sources on our part, *or* to the insufficiency of extant sources as mirrors of educational practice. *Tertium non datur.*

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[48] Presupposing that one error is an excess, the other a deficit.

[49] I am referring here to Mediterranean texts. Even though Arabic writers ascribe the rule to India, the simple form is not found in extant Indian sources. But what may be a correct iterated use in a non-linear situation turns up in a Sanskrit text from the 15th century [Plofker 1996; 2002] – if so, Indian reckoners knew what they were doing when applying the rule, and why.

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Chapter 21 (Article II.4) Embedding – Multi-purpose Device for Understanding Mathematics and its Development, or Empty Generalization?

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

See "Introduction"

Contents

Yvonne Dold-Samplonius and Paulus Gerdes
in memoriam

Introduction

The following reflections were spurred by an invitation to present something of my own choice at the IX Congreso de la Asociación Española de Semiótica "Humanidades, ciencia y tecnologia" in Valencia in December 2000.[1] Semiotics as such was never my field, but I decided to take advantage of the occasion and explore an idea located somewhere in the boundary region between the history of mathematics, philosophy of mathematics, psychology, linguistics and semiotics.

This was an idea that had dawned to me some years earlier and which I had never found the time to elaborate. It had to do with the notion of "embedding" and its possible applicability to certain higher-level questions in the historiography of mathematics (and, in as far as the history of mathematics is relevant for the philosophy of mathematics, also problems belonging to this latter domain).

The concept – that is, the specific technical meaning of the word, which itself is obviously older and more broadly used – originated in grammar (more precisely in syntax). In the sentence "the canary which had been devoured by the cat had been the joy of my mornings", the relative clause "which had been devoured by the cat" is *embedded* in the main clause; as a whole it functions as an element of the main clause, and it can be replaced (with a change of word order) by a single adjective – for instance, "yellow".

Embedding proper thus occurs when a whole subordinate clause – a sentence in itself – occupies the place of a sentence member (be it in a main clause, be it within a higher-level subordinate clause) or of some other phrase (as when a relative clause fills the place of an adjective). The most developed form of embedding is the iterative expansion of this latter type as described, for instance, in the recursive schemes of generative grammar (on recursion, cf. examples below).

Embedding-like phenomena characterize language in various shapes and at different levels, not only in sentence syntax. One, only virtual and perhaps not properly carrying the name, is the Saussurean interplay of "syntagm" – a sequence of places in a sentence – and "paradigm" – the set of possible values of the "variable" occupying a particular place.[2] It is related to the use of general terms (e.g., "animal") in language that may

[1] The paper was accepted for the proceedings, which however never appeared. I am grateful for the possibility to publish a revised version in the present context.

[2] That is, for example, the "syntagm" subject – verb – object, where (from the grammatical point of view) the place "subject" can be filled by any member of the "paradigm" consisting of nouns (and pronouns in the nominative), the place "verb" by the paradigm consisting of transitive verbs, and "object" by a paradigm containing nouns and accusative forms of pronouns.

stand for any one of a number of particulars (*in casu* "cat", "canary", "eel", …), but with the difference that the general terms are actually present in the sentence "the animal is alive", whereas the places in a syntagm are potentialities which (in the best Aristotelian manner) are only actualized by being filled out by paradigm members – the syntagm in itself is no sentence but an abstract scheme. It is also somewhat similar to the idea of a Cartesian product, but in this case with the difference that here the places in the syntagm are occupied by *identical* paradigms,[3] whereas the places in a sentence syntagm are to be filled out by different paradigms.

My intention in the following is to explore whether and to which extent the concept can be applied fruitfully and coherently to three areas pertaining to the structure and history of mathematics:

– The structure of numerals and the emergence of place-value and quasi-place-value notations.
– The relation of algebraic symbolism to preceding representations.
– The alleged absence of "revolutions" from the development of mathematics.

I shall tie all three discussions to the notion of "embedding". It remains to be seen whether it is applicable *in the same or related ways* in all three cases, and thus whether applying it provides some real insight; if the three uses are unrelated, it is an empty metaphor which might as well be discarded.

Empty or not, "embedding" is a spatial metaphor. I shall close by some reflections on spatiality, language and mathematics.

Numerals

A theme which "historically interested" mathematicians are fond of treating is the emergence of place-value notations. In agreement with the "Lamarckian fallacy"[4] so close at hand in every evolutionary thought, *our* present position is seen as the goal of preceding changes; maybe further developments shall attain even higher peaks (this was what Nietzsche supposed, adding an *Übermensch* to Lamarck's ladder of perfection), but these will by necessity ascend *from us*. There may be blind alleys in evolution (even Lamarck supposed that animals that happened to live in the sea might develop into more perfect fishes but could not attain the perfection of man); but the blind alleys are always represented by "the others".

[3] An example would be a statistical table showing the variation of the population of a number of cities over time: for each year, the same cities are listed in the table.

[4] The term refers to Lamarck's original thought as set forth in his *Philosophie zoologique* from 1809 – *man* is the perfect being, and other animals strive in their development toward this perfection. The fallacy is absent from the "neo-Lamarckian" doctrines from the late 19th century, from which the teleological element has been eliminated, and where the originally ancillary inheritance of acquired characteristics has become the central explanatory device.

The place-value system can be explained as an embedding in various ways.[5] One is recursive on the level of places only,

$$\{\texttt{place sequence}\} \texttt{->} \begin{cases} \{\texttt{place}_{\texttt{f}}\} \\ \{\texttt{place sequence}\}\{\texttt{place}\} \end{cases}$$

where {place$_f$} ($_f$ for "first") may be filled out by any of the digits 1, 2, …, 9 constituting the paradigm {digit$_f$}, and {place} by any of the digits 0, 1, 2,…, 9 (the paradigm {digit}). In plain words, the minimal string representing a number consists of a single place that may be filled by any of the digits 1, 2, …, 9; others number strings may be produced by appending one or more places to the right that can be filled by any of the digits 0, 1, 2, …, 9. Because of the incomplete identity of the paradigms {digit} and {digit$_f$}, this is no full Cartesian product, although it comes closer to this type than the linguistic syntagm-paradigm structure. For use in the following, I shall call it "type I" description.[6]

The scheme which best corresponds to current explanations of the system ("type II" description) avoids the explicit reference to places (as did studies of syntax prior to the advent of structuralism), but it still separates the writing from the arithmetical meaning,

$$\{\texttt{written number}\} \texttt{->} \begin{cases} \{\texttt{digit}_{\texttt{f}}\} \\ \{\texttt{written number}\}\ \{\texttt{digit}\} \end{cases}$$

The corresponding numerical value is explained as a sum $\sum_{i=0}^{n} a_i \cdot 10^i$; that is, it still refers to the single places (even this is an analogue of the way a pre-structuralist syntactical analysis ascribes meaning to a sentence). A recursive definition which does not refer to the numbers of the places can be made in the shape of an algorithm with a single loop $\langle\text{value}\rangle := \langle\text{value}\rangle \cdot 10 + \langle\text{next digit}\rangle$, starting with $\langle\text{value}\rangle = \langle\text{first digit}\rangle$ and ending when $\langle\text{next digit}\rangle$ is the last digit, corresponding to the formula

$$(\ldots((d_n \cdot 10 + d_{n-1}) \cdot 10 + d_{n-2}) \cdot 10 \ldots + d_1) \cdot 10 + d_0.$$

It is rarely pointed out that the place-value notation implies a more refined type of embedding ("type III"), which cannot be formalized as a simple recursive scheme of the type used in generative grammar, and which I shall therefore approach through examples.[7] It has the strange property that it can be interpreted in different ways – to be "polysemic" – at the intermediate stage but to lead to the same final total meaning

5 In the interest of simplicity, I shall at first restrict the discussion to the writing of positive integers. Later, the reference to historical examples will force us to introduce fractions.

6 If we admit an initial string of zeroes in the writing of a numeral, the distinctions between {digit} and {digit$_f$} and between {place} and {place$_f$} are evidently superfluous. The cost is that numbers no longer correspond to numerals but to equivalence classes among numerals.

7 Not only is this property of the place-value notation rarely pointed at or explained, but mathematics teachers tend to censure students' spontaneous taking advantage of the principle in locutions like "three point twenty-five".

irrespective of the choice of intermediate interpretations.[8] In type-I and type-II interpretation, a number of type *d*|0|0|…|0 (which may be understood as an additive contribution to a more complicated multi-place number) means $d\times(1|0|0|\ldots|0)$; apart from the recursive definition of {place sequence}, embedding is thus only present in the sense that a "1" can be replaced by any digit. But in a multi-place number *a*|*b*|*c*|*d*|*e*|*f*|…|*r*, any sequence of digits may actually be taken out to represent a number counting the units at its own lowest place; thus, *a*|*b*|*c*|*d*|*e*|*f*|…|r = (*a*|0|0|0|*e*|*f*|…|*r*)+(*b*|*c*|*d*×1|0|0|…|0) – less abstractly, 234875 = $234\times10^3+875 = 23\times10^4+48\times10^2+75 = 2\times10^5+3487\times10^1+5$, etc. That is, if a multi-place number is put into the place of a unity of any level, all its "overflowing" places end up where they "should" stand.

3600	600	60	10	1
	1	0	2	5
	1	0	2	5
	2	0	5	0
1	0	2	5	0
1	0	2	5	0
2	0	5	4	0

This property is essential for the simplicity of algorithms. In order to understand why this is so one may look at how addition works in the mixed decimal-seximal system of the Babylonians.[9] A number 1$^{|}$0/2$^{|}$5 (where separation "$^{|}$" stands for a factor 10 and separation "/" for a factor 6) added to itself gives 2$^{|}$0/5$^{|}$0, while 1/0$^{|}$ 2/5$^{|}$0+1/0$^{|}$ 2/5$^{|}$0 = 2/0$^{|}$ 5/4$^{|}$0. Multiplications of course become even more bothersome, and root extractions virtually impossible if not reduced to an implicit sexagesimal system.

Such mixed place-value systems are rare in the historical record, though their non-place-value analogues are very common in pre-metric metrologies; best known since still in use are probably the Roman numerals, with their levels 1, 5, 10, 50, …. The place-value example that comes to my mind beyond the Babylonian system is the Maya calendar system, which is vigesimal except for the step that ensures a unit of 360 instead of 400 days – obviously a choice dictated by actual calendarian convenience.[10] It is therefore

8 This property is shared with the associative composition of group theory and thus with arithmetical addition and multiplication – $a\times(b\times c) = (a\times b)\times c$. In contrast, the algebraic expresssion $a+b\times(c+d)\times f$ is certainly not to be identified with $(a+b)\times c+d\times f$; nor are the logical sequences $p\Rightarrow(q\Rightarrow r)$ and $(p\Rightarrow q)\Rightarrow r$ equivalent.

9 Mostly, historians think of the Babylonian system as a "sexagesimal" system, a place-value system with base 60. That the Babylonians themselves understood the system rather (but not exclusively) as a decimal-seximal system in the Old Babylonian period (c. 2000 BCE to c. 1600 BCE) follows from the way "intermediate zeroes" are inserted in a text from Susa and from the way roundings are made [Høyrup 2002: 15 n.19, 263]. In the Seleucid epoch (third to second century BCE), some "intermediate zeroes" normally considered erroneous suggest that the same way of thinking still prevailed.

10 See [Closs 1986: 299–307]. To be precise, the irregularity of the system occurs in what can be regarded as the fractional part, calendarian distances being counted in the vigesimal place-value notation in units of 360 days; below this interval, up to 17 units of 20 days and up to 19 single days are counted.

not without interest that the earliest Latin *theoretical* exposition of "Arabic" reckoning – due to Jordanus of Nemore and from the early 13th century – proposes an analogue of this mixed system for fractions.[11] Instead of describing how to compute with the sexagesimal fractions currently used by astronomers (minutes, seconds, thirds, etc.), Jordanus introduces "consimilar fractions" (in modern symbols $\Sigma_1^n a_i \cdot p^{-i}$), for which the factor p by which each place decreases is constant; this is an obvious generalization of the sexagesimal fractions ($p = 60$) and also encompasses decimal fractions ($p = 10$) as another special case; it might seem rather empty if it had not gone together with the introduction of another category: "dissimilar fractions", for which the factors of decrease vary (in modern symbols $\Sigma_1^n \left[a_i \prod_1^i p_j^{-1}\right]$). The "dissimilar fractions" correspond to the "ascending continued fractions" which were commonly used in Semitic languages (Arabic, but also Babylonian – see [Høyrup 1990] ⟦= article I.1⟧) – composite fractions of the type "one half, and two thirds of one half, and four sevenths of one third of one half". In these, the successive denominators would be chosen *ad hoc*, and therefore had to be made explicit.[12]

From the mathematical point of view, the dissimilar fractions constitute the general and the consimilar a "degenerate" case (sexagesimal and decimal fractions being one step further degenerate by having a predetermined and no general factor of decrease). In spite of this, the dissimilar fractions were never accepted by anybody apart from the inventor, and for good reasons: number notations are first of all tools for computation, and the criterion for acceptance is not mathematical generality or "beauty" but a compromise between computational ease and agreement with pre-existing number concepts and habits; given his non-Semitic linguistic environment, Jordanus erred on both accounts.

Place-value systems constitute a special (elliptic) variant of multiplicative writings of numbers, and in this respect they correspond (ellipsis apart) to the normal way of expressing higher numerals in all languages which possess these. As spoken examples we may refer to English *sixty-four* (interpreting *-ty* as a variant of *ten*), corresponding to "types I/II", and <u>*two hundred sixty-four thousand*</u> *three hundred and nineteen*, where the underlined part is close to the principle of "type III" (without sharing its inherent flexibility – we would never find **twenty-six myriads four thousand thirty-one-ty and nine*). In writing, multiplicative notations are known for example from younger Hieroglyphic and Middle Kingdom Hieratic Egyptian, where, respectively, 27,000,000 may be written as 270 below the sign for 100,000, and 40,000 as 4 written below the sign

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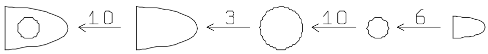
11 See [Høyrup 1988: 337*f*].

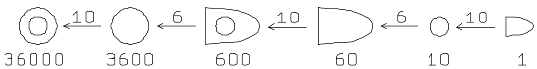
12 Either as here in words or (thus in late medieval Maghreb mathematics and in Fibonacci's *Liber abbaci*) as $\frac{4}{7}\frac{2}{3}\frac{1}{2}$.

for 10,000 [Sethe 1916: 9]. In the Greek alphabetic notation we also find a variant related to "types I/II" – thus in Diophantos *Arithmetic* II.xxiv [ed. Tannery 1893: I, 121] Ṁα.͵δχμα, meaning 1 (=α) myriad (Ṁ), 4 (=δ) thousand (͵) and 641 (=χμα). The Greek type is certainly an imitation of spoken numerals, which in Ancient Greek follow the same pattern; given the unpredictable level of the multiplicand, the Egyptian system is more likely to have been at least in part independent of spoken language.

From here we may turn to the earliest beginning of writing in proto-literate Mesopotamia. Since the eighth millennium BCE, a system of characteristic tokens made of burnt clay had been used in the Near East, seemingly for accounting purposes – see, e.g., [Schmandt-Besserat 1992]. Some of these tokens (small and large cones and spheres) appear to have represented various standard containers (and thus measures) of grain, whereas flat circular discs probably stood for sheep and other livestock. With the advent of writing in the later fourth millennium, representations of the spheres and cones came to stand for standard units of grain. The relations were as follows [Damerow & Englund 1987: 136]:



The same signs (produced indeed by impression of the same particular stylus) were also used (presumably a *new* use) for (perhaps only "almost-abstract") numbers, but with a different sequence of factors [Damerow & Englund 1987: 127], in which we recognize the decimal-seximal structure of the later place-value system:



We shall return anon to the reasons that these numbers should possibly be characterized as "almost-abstract" only. For the moment we observe first of all that the use of a factor sequence for the number series that differs from that of the grain measures cannot easily be explained without the assumption that at least the lower part of the number series rendered the structure of a pre-existent (and thus oral) numeral system. The writing of 600 is clearly meant multiplicatively, and corresponds to the structure of the Sumerian word for 600 (g e š. ù, "sixty-ten"[13]); this word, however, is not attested until much later. Whether a spoken word for 3600 (with the appurtenant multiplicative word for 36000) was already in existence when writing was created is rather doubtful, as is the status of an early equivalent of g e š. ù as a proper numeral; for certain purposes, indeed, written counting was based on a different "bisexagesimal" system with units 1, 10, 60,

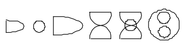
13 I.e., ten sixties, Sumerian having postposited adjective and numeral.

120, 1200 and 7200[14] [Damerow & Englund 1987: 132]. The spoken terms may therefore rather have been constructed from the written numerals; whether the decimal-seximal structure of spoken numerals extended to three levels (from which a systematic unlimited expansion is easily derived) before the intervention of writing is thus quite dubious. It is a fair guess, in any case, that the utmost-left grain unit is a fresh emulation of the multiplicative structure of "normal" numerals, as are the writings of 1200 and 7200 in the "bisexagesimal" system.[15]

Before the invention of writing, not only grain accounting but also the counting of livestock was made "concretely", 2 sheep (e.g.) being indicated by two sheep-discs. The introduction of numerals had as its purpose to change this, and in written accounts the same meaning was indicated by juxtaposition of a drawing of the sheep-disc and the numeral 2. In this sense, the numbers can be regarded as abstract. Two reasons suggest that we should perhaps add an "almost". One is the existence of the bisexagesimal system. Since we do not understand the exact bureaucratic procedures within which it was used, we cannot say whether its existence has any implications for the number concept that make it less abstract than ours; it may imply nothing more than our habitual counting of wine bottles in dozens and, more recently, of bytes in units of 1024 and 1,048,576 spoken of for convenience as 1000 (k) and 1,000,000 (M). The other is the use of the numbers without reference to a unit when the dimensions of rectangles are indicated. This habit of leaving implicit a "basic" unit (in length measures the nindan or "rod" of c. 6 m) stayed alive for millennia in Mesopotamian mathematics, and can hardly be taken as evidence for failing understanding (we also tell that something happened on the third [day] of October [in year number] 1989 at 2 [hours] o[f the] clock); but it does demonstrate that the users of the system did not feel it was compulsory to separate quantity systematically from quality and make both explicit. From a contemporary mathematical perspective it is tempting to see this as a symptom of "primitivity", that is, of a not fully unfolded number concept – forgetting that even we omit the quality in certain cases where it is implied unambiguously by the context.

An interpretation of the place-value system in this light may result in unexpected insights. We may compare the system we actually use (now for fractional numbers) in "type-II"-interpretation with what Stevin proposed in *La Disme* in 1585 [ed. Sarton 1935]:

[14] The signs were written thus (increasing values toward the right):



[15] In the case of the grain units, however, this *is* nothing beyond a fair guess. Sometimes tokens are provided with a circular punching, which almost certainly gave them a specific meaning, possibly a larger value. The impression of a small circle in writing could be an emulation of this punching. Even if this should be the case, however, the precise meaning "×10" may well have gone together with the creation of writing.

to write “our” 375.72 as 375⓪7①2② . As in the Maghreb notation for the “dissimilar” or “ascending continued” fractions or in the usual notation for angular minutes and seconds, the value of each fractional place – its “quality” – is made explicit separately from its “quantity”, the digit. This makes explanation of the meaning more obvious but prevents easy recursiveness.

In “type-I” or “type-II” interpretation, our present notation already “recedes” into “primitivity” when compared with Stevin’s original proposal; in “interpretation III”, where unambiguous recursiveness can no longer be formulated, and where the same number may be interpreted as an embedding in several different equally valid ways (which, as pointed out, is the very reason that convenient algorithms can be formulated), we are even farther removed from any clear distinction between qualitative and quantitative dimensions (not to speak of making the structure explicit). The best linguistic analogue is the kind of contact language which speakers familiar with an ergative deep structure may conceptualize in *their* way, and which speakers whose mother tongue has an accusative deep structure may without difficulty understand as *they* are accustomed to.[16]

Historically, all place-value systems probably arose through transformation of preceding systems where the multiplicative structure was clear, that is, where digits multiplied values of identified places or their analogues.[17] As in spoken language (where, to mention simple English examples, *thirteen* is found instead of **three-ten* and *twenty* instead of **twain ten* or **two tens*), such mathematical rigour is worn off in use. In both cases, “embedding” is a reconstructed deep structure, no longer (and perhaps never historically in complete form) a clean surface structure. Even in the case of spoken numerals, the deep structure is likely to be only a possible or at best a highly plausible reconstruction, not the only logical possibility.[18]

For historians of mathematics, these observation imply a moral: there is no reason to see the introduction of a place value system as an indubitable intellectual progress. For purposes of practical computation, the progress is not to be doubted; nor is it, indeed, in the Babylonian “primitive” deletion of standard units. But conceptual ambiguity – be

[16] See [Silverstein 1971] on Chinook Jargon.

⟦In a first, very rough approximation, ergative languages are organized as if transitive verbs were in the passive voice – “The soup is prepared (by me)” instead of “I prepare the soup”.⟧

[17] Such analogues may be columns in an abacus – or they may be the values of specific signs like the early Mesopotamian signs for 1, 10, 60 etc., in which case the “digits” are the fixed patterns in which specific numbers of such signs are organized.

[18] Similar ambiguities can be found in other linguistic domains. I think in particular of the doubts whether it is meaningful to refer to a “verb phrase” (and thus to split the sentence into subject and predicate, the latter containing verb+object) in creole and certain other languages. For creoles, see [Bickerton 1981: 53 and *passim*]; for Dyirbal in its relation to related languages, [Dixon 1977: 382]; for Sumerian, [Gragg 1973: 91].

it pragmatically adequate ambiguity – is not what mathematicians normally see as the aim of *their* specific enterprise.

Symbols and other symbols

In [1842: 302], Nesselmann proposed in his *Algebra der Griechen* a three-stage scheme for the history of algebra. His "first and lowest" stage is that of "rhetorical algebra", in which everything in the calculation is explained in full words.[19] The second, "syncopated algebra", makes use of standard abbreviations for certain recurrent concepts and operations, even though "its exposition remains essentially rhetorical". The third is "symbolic algebra"; here, "all forms and operations that appear are represented in a fully developed language of signs that is completely independent of the oral exposition".

Al-Khwārizmī's *Algebra* (from the early ninth century CE) is pointed out to represent (together with other Arabic works[20]) the most consistent version of the rhetorical principle, as even numbers are written in full words. Iamblichos and "the oldest Italians and their disciples, for instance Regiomontanus" are counted in the same category in spite of their use of non-verbal numerals. Diophantos and later European algebra until the 17th century is syncopated,[21] "although already Viète has sown the seeds of modern algebra in his writings, which however only sprouted some time after him" (the following pages mention Oughtred, Descartes, Harriot and Wallis).

Nesselmann's stages (or types) are regularly cited, even though many histories of mathematics interpret any use of abbreviations as "symbolization". Worse, even those who cite him rarely notice Nesselmann's main point: that symbolization allows operations directly on the level of the symbols, without any recourse to thought through spoken or internalized language.[22] It may hence be of some value to rethink the scheme; as we shall see, our present framework is useful for that purpose.

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19 Here and in the followings, all translations into English are mine when nothing else is indicated.

20 The introduction of syncopation ⟦and indeed incipient symbolization⟧ in late Medieval Maghreb algebra was indeed only discovered by Franz Woepcke in [1854].

21 P. 304, n. 15 points out that some parts of Diophantos's *Arithmetic* are written without any use of abbreviations, and are thus purely rhetorical.

22 In Nesselmann's own words

> We may execute an algebraic calculation from the beginning to the end in fully intelligible way without using one written word, and at least in simpler calculations we only now and then insert a conjunction between the formulae so as to spare the reader the labour of searching and reading back by indicating the connection between the formula and what precedes and what follows.

Since then, (mis)use of logical arrows in sequence has sometimes eliminated the conjunctions – but already because the arrows are misused, they are mere abbreviations ("syncopations") and cannot serve as symbols in Nesselmann's sense (cf. note 8).

In Diophantos's *Arithmetic*, we find symbols for the unknown number (the *arithmós*) and its powers, spoken of as "signs" (σημεῖον). The unknown itself is written with a simple sign, close to ς; for all other powers (*dynamis* = $ς^2$, *kybos* = $ς^3$, *dynamodynamis* = $ς^4$, etc.), phonetic complements are added to the symbol (Δ^{Y}, K^{Y}, etc.);[23] complements are also added to the sign for the monad ("power zero"), and for numbers that stand as denominators in fractions,[24] except when fractions are written in compact form ($ς^{5}_{16}$ meaning $\frac{16}{5}$). Addition is indicated by juxtaposition, subtraction and subtractivity by ⩚ (λεῖψις, "missing" etc.). One sign only is used for direct operation: the designation of the "part denominated by" n (better, the reciprocal of n, since non-integer n occur), explained in the introduction to be indicated by a sign $^{\times}$ for powers of the unknown. In III.xi, a number is posited to be $ς^{\times}$. When then ς turns out to be $^{77}_{41}$ (that is, $\frac{77}{41}$), the number itself is stated immediately to be $^{41}_{77}$ ($\frac{41}{77}$). Diophantos thus knows *at the level of symbols* (and supposes his reader to understand) that $(ς^{\times})^{\times} = ς$, and that

$$\left(\begin{matrix} q \\ p \end{matrix}\right)^{\times} = \begin{matrix} p \\ q \end{matrix}.$$

From here we may jump to late medieval Italy. In Dardi of Pisa's mid–14th-century *Aliabraa argibra*[25] we find on fol. 3ʳ this explanation of how to multiply a square root (℞) by a number:

> If you wish to multiply ℞ of number by number, as 6 times ℞ of 3, you should reduce the number to ℞ that is 6, which makes 36, which 36 you should multiply by 3, it amounts to ℞ of 108, and so much makes ℞ of 3 times 6 or 6 times ℞ of 3, that is ℞ of 108, which ℞ is surd [indiscreto], and so we prefer it as a surd number, that is, R of 108.
>
> ℞ of 3 —— times 6 —— that is ℞ of 36 times ℞ of 3 —— makes ℞ of 108.

23 One should remember that Diophantos wrote without distinguishing between capital and small letters. Apart from reminding of the phonetic reading, the complements thus also indicated that the symbols for the *dynamis* and the *kybos* were not to be read as 4 and 20, respectively.

It has been suggested (thus [Heath 1921: II, 457] on the basis of the way the sign is written in Medieval manuscripts that the sign for the ἀριθνὸς comes from a contracted αρ. However, the form in the papyrus P. Mich. 620 (probably from the early second century CE, and not known to Heath), *viz* ϛ̀ [Vogel 1930: 373], does not support this reconstruction.

24 In I.23 [ed. Tannery 1893: I, 92], $^{50}/_{23}$ appears as ν̄ κγ^{ων}, "50 of 23rds", and $^{150}/_{23}$ slightly later as "150 of the said part"

25 On Dardi and his algebra, see [Van Egmond 1983]. I use the earliest extant manuscript Vatican, Chigi M.VIII.170, written in Venetian in c. 1395 (referring to the recent stamped foliation).

We notice that ℞ is used not only where we would use a mathematical symbol $\sqrt{}$ but also in the discursive text, and that it is followed in all functions by the preposition “of” exactly as the fully written word “root”/*radice* would be.

Somewhat closer to symbolization is the summary of the explanation of the multiplication of binomials *in croze* (that is, cross-multiplication; this example fol. 18^{v}):

Here, *c* stands for *cosa* (“thing”), that is, the first power of the unknown, and *ç* for *censo*,[26] its second power; *e* means “and”. Drachmas are used for “power zero”. The scheme imitates one which is used to explain the multiplication (10–2)×(10–2) on fol. 4^{v}, itself modelled after the explanation of the cross-multiplication of two-digit numbers; it may thus be regarded as an extension of “type-I” embedding in which digits are replaced by algebraic monomials. Similar but more fully developed schemes are still found in Stifel’s *Arithmetica integra* from 1544 and other 16th-century works.

Other treatises from Dardi’s century go somewhat further, and write divisions by polynomials as fractions. Thus we find in *Trattato dell’alcibra amuchabile* [ed. Simi 1994: 421:

$$\frac{\texttt{100}\qquad\qquad\qquad\texttt{100}\qquad\qquad}{\texttt{per una cosa}\qquad\texttt{per una cosa e più 5}}$$

corresponding to our $\frac{100}{x}+\frac{100}{x+5}$. The solution of the problem $\frac{100}{x}+\frac{100}{x+5}=20$ is then explained verbally with reference to the operations performed on the symbolic expression in parallel to the addition $\frac{24}{4}+\frac{24}{6}$ (the aim being of course that the trained reckoner be able to operate directly on the formal fractions). Here, as we see, the places of numbers in a more intricate arithmetical expression may be taken over by algebraic polynomials.

The limits of this one-level embedding are illustrated by the way complex entities are expressed in Cardano’s *Ars magna* from 1545 (this example from [Cardano 1545: 34^{r}1). What we would express as

$$\sqrt[3]{42+\sqrt{1700}}+\sqrt[3]{42-\sqrt{1700}}$$

appears here as “℞.V: cubica 42 p: ℞ 1700 p. ℞V: cubica 42 m: ℞1700” – “p.” representing *più*/“plus”, “m.” *meno*/“less”, “℞” “radice”, and “V” (for *unita*/“united” or *universale*) indicating that the root is taken of two members. “℞V” thus expresses that a two-member expression is embedded at the place of the radicand. However, the whole notation is so cumbersome that mental operation at the level of symbols is impossible; it facilitates

26 In the 14th-century Venetian dialect of the manuscript – probably that of the original – the writing of this word would probably have been *çenso* (but it never appears in full). In the 15th century, north-eastern Italian dialects would mostly write *zenzo*.

writing but not understanding – as most algebraic syncopation it calls for a translation into the corresponding full verbal expression if one is to penetrate its mysteries. Only Bombelli's *L'algebra* from [1572] (claimed very adequately by the author to be primarily a rewriting in understandable form of what Cardano and other precursors had already done but set forth opaquely) introduces algebraic parentheses for composite radicands (written L.... ⅃, and used for multiple nesting) and an arithmetical notation for powers in which $\overset{n}{\smile}$ represents our x^n. The latter notation is obviously akin in spirit to Stevin's almost contemporary notation for decimal fractions. The absence of a "placeholder" or manifest representative of the unknown makes it unfit both for operation with several unknowns and for embedding of a whole algebraic parenthesis at the place of an unknown; it can be seen to share the strength as well as the weaknesses of a Stevinian notation for consimilar as opposed to a Fibonaccean notation for dissimilar fractions (see note 12). Bombelli thus provides one of the essential building stones for the creation of a fully symbolic algebra, but he uses it only for composite radicands and stops short of producing this algebra himself ⟦cf. article II.14⟧.

Strictly speaking, algebraic embedding does not begin with the incipient use of syncopation for symbolization purposes but rhetorically. In one problem of the algebra-chapter of Leonardo Fibonacci's *Liber abbaci* from 1228 [ed. Boncompagni 1857: 422], a *census* (the Italian *censo*) is re-baptized *res* ("thing", becoming *cosa* in Italian), which allows Leonardo to speak of its square as *census*. The same trick is inherent in a number of 15th- and 16th-century Latin and European vernacular terms for the higher powers; as an example chosen at random we may quote Pedro Nuñez's *Libro de algebra* [1567: 24]:

> The first of those quantities which we call *dignidades*, which are ordered thus in proportion, is the *cosa*, which for this reason was given unity as denomination. The second is the *censo*, to which fell 2 as denomination. The third is the *cubo*, which has 3 as denomination. The fourth is the *censo de censo*, which has 4 as denomination. The fifth is called *relato primo*, whose denomination is 5. The sixth is *censo de cubo*, or *cubo de censo*, and its denomination is 6.

We find the same system in Luca Pacioli's *Summa* and Stifel's *Arithmetica integra*. Diophantos's system is different, however; here [ed. Tannery 1893: I, 6] the sequence is *arithmós, dýnamis, kýbos, dynamodýnamis, dynamokýbos, kybokýbos*. This is also found in al-Karajī's *Fakhrī* (Arabic list quoted in [Woepcke 1853: 48]) and in ibn Badr's *Recapitulation of Algebra* [ed., trans. Sánchez Pérez 1916: 18] – and still in Viète's *In artem Analiticen Isagoge* [ed. Hofmann 1970: 3].

The former system, as obvious also from the grammatical form "census *of* cube", etc., is built on embedding, the latter not. In the former system, the treatment of – say – second-degree problems where the unknown is itself a cube are therefore immediately seen to be reducible, and it is indeed equivalent to Leonardo's positing of a census as

a thing; in the latter, reducibility has to be understood, it is not exhibited directly by the terminology.

But this embedding is of course rudimentary; it allows the easy treatment of biquadratics and similar problems, but does not permit that a power of the unknown be replaced by a polynomial or other composite expression. Diophantos and al-Karajī treat the various powers as *entities*; in modern terms, Nuñez, Pacioli and Stifel have come to consider the power as a *function* or an operation, but only when the argument is another power – in other cases it remains an entity. The complete change of powers to being operations could only be effected when a generalization of Bombelli's parenthesis function was combined with a convenient notation for powers. Only Descartes has both in his *Geometrie*.[27]

The development of algebraic embedding thus turns out to go together with the full shift to symbolization in Nesselmann's sense. This is not strange; only the development of certain symbolic notations allowed the unambiguous expression of embedding, and the avoidance of monsters corresponding to

$$(a + b^{k-[m)+n} \times c] - d$$

(in principle, punctuation in a written rhetorical exposition could serve, but a sufficiently consistent punctuation also did not exist in the 16th century – perhaps not even today). On the other hand, only the use of embedding made it possible to handle complex expressions so conveniently that computations could be made without recourse to extensive verbal explanations.

With this in mind, we may take a look at two notations that are neither rhetorical nor participants in the development toward Modern European symbolic algebra – first the Indian notation, which Nesselmann refers to as an earlier case of symbolic algebra.

As examples we may consider two equations from Bhāskara II (b. 1115) – see [Datta & Singh 1962: II, 31*f*]. An equation which in our terminology translates

$$5x + 8y + 7z + 90 = 7x + 9y + 6z + 62$$

is actually expressed

yâ 5   *kâ* 8   *nî* 7   *rû* 90
yâ 7   *kâ* 9   *nî* 6   *rû* 62

whereas our

$$8x^3 + 4x^2 + 10y^2x = 4x^3 + 0x^2 + 12y^2x$$

corresponds to

yâ gha 8   *yâ va* 4   *kâ va yâ.bhâ* 10
yâ gha 4   *yâ va* 0   *kâ va yâ.bha* 12

[27] Descartes did not need to combine the two, and therefore does not do so; what he needs and uses repeatedly are polynomial parentheses multiplied by powers of a variable. But Descartes made the tools available for those who were to need them for wider purposes in the following generations.

As Datta and Singh quote David Eugene Smith [1923: II, 425*f*], this notation is "in one respect […] the best that has ever been suggested" because it "shows at a glance the similar terms one above the other, and permits of easy transposition". It corresponds well to the single-level embedding of place-value numbers in Stevin's notation.[28]

What it does not permit is direct multiple embedding, for instance replacement of y*â* by a polynomial. Nor is it intended for that, it is only used for linear reductions of equations, as one will discover looking at the context of Bhāskara's formula in the *Vijā-gaṇita* [trans. Colebrooke 1817: 248]; the rest of Bhāskara's argument is syncopated, though more systematic in its use of abbreviations than Diophantos.

Indian *schemes* (if not Bhāskara's whole text, but the same could be said about Viète) are thus justly seen as a symbolic notation by Nesselmann; but Smith is right that it is the best "in one respect" only – namely within the restricted framework of problems actually dealt with by Bhāskara and his fellows, and for the even more restricted use made of it within this framework; it was not open-ended. It this way, it presents us with a parallel to the "shortcoming" of the place-value system for fractions as compared with Fibonacci's more flexible and more explicit but less handy notation for dissimilar fractions.

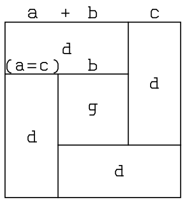
The other example is European, and borrowed from Jordanus of Nemore's *De numeris datis*, written somewhere around 1220–30. The work is a quasi-Kantian critique of the procedures of algebra, modelled after Euclid's *Data*; it tries so to speak to demonstrate that what is currently done "empirically" in Arabic and post-Arabic *al-jabr* can be made by theoretically legitimate methods based on arithmetical theory – see [Høyrup 1988] and [Puig 1994]. I translate one of the propositions from the Latin text in [Hughes 1981: 58] (the diagram is added in the interest of intelligibility, in agreement with the exposition in [Puig 1994] – nothing similar is found in the original[29]):

> If a given number is divided into two and if the product of one with the other is given, each of them will also be given by necessity.
>
> Let the given number *abc* be divided into *ab* and *c*, and let the product of *ab* with *c* be given as *d*, and let similarly the product of *abc* with itself be *e*. Then the quadruple of *d* is taken, which is *f*. When this is withdrawn from *e, g* remains, and this will be the square on the difference between *ab* and *c*. Therefore the root of *g* is extracted, and it

[28] One may add that the use of abbreviations for unknowns, powers and operations prevents that arithmetization of the designation of powers which reduces the multiplication and division of powers of the unknown to a purely formal process.

[29] Whether Jordanus thought of something similar is uncertain but possible (the failure to point out at first that *a* is meant to equal *c* might suggest that this was evident from a diagram); in any case the diagram may be helpful for a modern reader. The proof when read in the context of the treatise as a whole does not need it: even though there are no explicit references, the unexplained jumps build on propositions that are proved earlier or in Jordanus's *Elements of Arithmetic*.

> will be *b*, the difference between *ab* and *c*. And since *b* will be given, *c* and *ab* will also be given.
>
> The working of this is easily verified in the following way For instance: Let 10 be divided into two numbers, and let the product of one with the other be 21, whose quadruple is the same as 84, it is taken away from the square of 10, that is, from 100, and 16 remains whose root is extracted, which will be 4, and that is the difference. It is taken away from 10 and the remainder, that is, 6, is halved; The half will be 3, and this is the minor part, and the major is 7.



Not uncommonly, the use of letters have made interpreters see this as an early instance of symbolic algebra. Nothing could be more mistaken – in terms of the caption of the present section, these letters are indeed "other symbols". The letters serve to make the argument general, and are thus a parallel to the line segments of geometrical demonstrations. But the argument cannot be made by manipulations of the symbols, in particular because every new step is expressed in new symbols that have to be identified verbally; even that rudimentary embedding is avoided which consists in conserving the name 4*d* for the outcome of a multiplication of *d* by 4.

It is of some interest that Jordanus' letter notation in these proofs may have been inspired by the algorithms for computation with place-value numbers in type-I interpretation – see [Høyrup 1988: 337]. When presenting demonstrations for these algorithms Jordanus uses letters for digits, not for numbers; they thus represent the place in which the digit has to be inserted. In this sense embedding is of course also a feature of the proofs of *De numeris datis* – the letters represent places where any digit can be inserted instead of the letter; but embedding in this sense is inherent in any attempt to formulate an argument or statement in general terms, be it arithmetical, geometrical, or ethical – cf. above, p. 567.

Embedded theoretical domains?

In 1968, Raymond L. Wilder formulated as the last of 10 "'laws' governing the evolution of mathematical activity" that

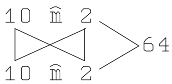
> Mathematical evolution remains forever a continuously progressing affair limited only by the contingencies [of] the opportunities for diffusion, such as may be provided by a universally accepted symbolism, increased outlet for publications, and other means of communication[, the] Needs of the host culture [and the long-term stifling effects of a] static cultural environment [or] an adverse political or general anti-scientific atmosphere

(quoted from the reprint [Wilder 1978: 200*f*]). In a similar list of ten "laws", Michael J. Crowe [1975: 165*f*] also proposed as the tenth that "revolutions never occur in mathematics", in the sense that no previously accepted entity is ever "overthrown [or] irrevocably discarded". He gave as an example that "Euclid was not deposed by, but reigns along with, the various non-Euclidean geometries", and added that his law does not pre-

clude the existence of revolutions in “mathematical nomenclature, symbolism, metamathematics (e.g. the metaphysics of mathematics), methodology (e.g. standards of rigor), and perhaps even in the historiography of mathematics”. In contrast, the shift from Ptolemaic to Copernican astronomy is seen as a genuine revolution.

Comparable formulations abound, not least among mathematicians reflecting on the history of their field. They do not deny that new things happened when (e.g.) the range of numbers was enlarged so as to encompass complex numbers or quaternions or when non-Euclidean geometries were accepted in the 19th century; but they go on to tell that quaternions contain the complex numbers as a subset, for which the usual arithmetic of complex numbers holds good; that complex numbers encompass real numbers as a subset, for which …; … and that the integers contain the positive integers as a subset for which the rules of *Elements* VII–IX remain valid; similarly, it is normally held, not that Euclidean geometry “reigns along with the various non-Euclidean geometries” but that all of these turn out to be special cases of an all-encompassing geometry.

The alleged absence of revolutions in mathematics is thus explained as an embedding of old theories within more general frameworks. It could be added that the possibility of embedding older domains has often been used as a defining condition when the scope of mathematical theories was widened. One case was already referred to above – a very simple case, which is not suspect of being motivated by explicit concerns for embedding. When Dardi of Pisa wants to show (fol. 4^{v}) that (–2)×(–2) *must* be +4, he uses this scheme:



The idea is that cross-multiplication should still be valid, just as if we multiply “10 plus 2” by itself. Obviously, 10×10 is still 100, and 10×(less 2) as well as (less 2)×10 must reasonably be “less 20”. Since the whole product (8×8) has to be 64, (less 2)×(less 2) must hence be +4 – it can be neither –4 nor 0, since then the whole product would be 56 or 60.

To claim that no revolutions occur in mathematics amounts to asserting that *all* theoretical shifts in mathematics either consist in such embeddings of the old within something larger or in the addition of new fields on interest, corresponding to the addition of spectroscopy to existing physics (this latter example is given in [Crowe 1975: 165]).

The current way to prove this claim is a *petitio principii*. If one wants, for instance, to prove that the geometry of *Elements* II is “covert algebra”[30] and thus isomorphic with a substructure of modern (or Cartesian) algebraic theory, then he has to strip the text of all those features that do not fit the claim, by declaring them to be non-essential and mere results of the unfortunate limitations of the framework within which the ideas

30 Thus Hans Freudenthal’s characterization of *Elements* II.5 [1977: 189].

had to be expressed. In the actual case this not only implies that we take it to be non-essential that Euclid's theorems deal with equalities of areas and lengths and not with numbers (which could still be defended by the observation that areas and lengths can be mapped isomorphically onto the set of positive real numbers) but also that we neglect the fact that propositions 5 and 6 are algebraically though not geometrically identical (we just have to switch some names), as are propositions 9 and 10 (4 and 7 are so if we use proposition 1).[31]

Similar arguments could be used in many other cases. On the level of whole theoretical domains, embedding thus does not describe the actual historical process, since what is embedded is *not the conceptual network of the old theory* but *a substructure of the new theory itself* which has some superficial similarity with *certain features* of the old theory; in the best cases it is homomorphic with *those features of the old theory which the new theory wants to conserve*. This is no different from the conservation of epicycles in Copernicus's theory, the conservation of Copernicus's heliocentricity in Kepler's, or Newton's conservation of Kepler's idea that the same physics should hold true below and above the moon. From this perspective there was thus no revolution in Early Modern astronomy.

All in all, the purported protective embedding of everything once made by earlier mathematicians by their successors is rather an expression of the *prevalent ideology* of mathematicians – and probably not of an intra-scientific ideology alone. In a survey of the political opinions of US university faculty, Everett Carll Ladd and Seymour Lipset [1972: 1092] found mathematicians to be somewhat more conservative than physicians, considerably more than physicists, and far more than teachers of the social sciences, the humanities,– and even law. Probably, intra-scientific and extra-scientific ideologies reinforce each other. Thomas Kuhn once stated [1963: 368] that scientists, though "*trained*

[31] In symbolic translation, *Elements* II.1–10 can be expressed as follows ($\square(a)$ stands for the square on the segment a, and $\sqsubset\!\sqsupset(p,q)$ for the rectangle contained by p and q):

1. $\sqsubset\!\sqsupset(a,p+q+\ldots+t) = \sqsubset\!\sqsupset(a,p) + \sqsubset\!\sqsupset(a,q) + \ldots + \sqsubset\!\sqsupset(a,t)$.
2. $\square(a) = \sqsubset\!\sqsupset(a,p) + \sqsubset\!\sqsupset(a,a-p)$.
3. $\sqsubset\!\sqsupset(a,a+p) = \square(a) + \sqsubset\!\sqsupset(a,p)$.
4. $\square(a+b) = \square(a) + \square(b) + 2\sqsubset\!\sqsupset(a,b)$.
5. $\sqsubset\!\sqsupset(a,b) + \square({}^{a-b}/_{2}) = \square({}^{a+b}/_{2})$.
6. $\sqsubset\!\sqsupset(a,a+p) + \square({}^{p}/_{2}) = \square(a+{}^{p}/_{2})$.
7. $\square(a+p) + \square(a) = 2\sqsubset\!\sqsupset(a+p,a) + \square(p)$.
8. $4\sqsubset\!\sqsupset(a,p) + \square(a-p) = \square(a+p)$.
9. $\square(a) + \square(b) = 2[\square({}^{a+b}/_{2}) + \square({}^{b-a}/_{2})]$.
10. $\square(a) + \square(a+p) = 2[\square({}^{p}/_{2}) + \square(a+{}^{p}/_{2})]$.

If b is replaced by $a+p$ in propositions 5 and 9, propositions 6 and 10 result; if b is replaced by p in proposition 4, and if we use proposition 1 to show that $\square(a)+\sqsubset\!\sqsupset(a,p) = \sqsubset\!\sqsupset(a,a+p)$, proposition 7 results when $\square((a)$ is added to both sides. Application of similar small modifications will show that all propositions 4–10 if seen as algebraic identities are trivially equivalent.

to operate as puzzle-solvers from established rules, […] are also *taught* to regard themselves as explorers and inventors who know no rules except those dictated by nature itself". But mathematicians, as we see, are often not taught so; instead, they learn that progress is their field has always consisted in "changing in order to conserve", in agreement with a famous slogan of political conservatism.

Embedding and spatiality

Charles Darwin emphasized that evolution often makes use of existing organs which are put to new use. One example is the swim bladder, which in certain fishes was so well furnished with veins that it could serve for supplementary breathing; when adequate circumstances occurred, selection pressure gave rise to the development of genuine lungs.

Obviously, the human language faculty has made use of a pre-existing organ – namely the brain. We may ask which specific "organ" within the brain was made use of, but since many brain centres are involved in the use of language, no exhaustive answer is likely to emerge.

If we ask for syntax alone, however, at least a partial answer exists. According to Ron Wallace [1989: 519*f*], "In all mammals except humans, both sides of the hippocampus are cognitive-mapping structures[32] […]. In humans, the right hippocampus is specialized for mapping, the left for the production of verbal material". The evidence that the "cognitive-mapping system could function as a deep structure for language" is analyzed in [O'Keefe & Nadel 1978: 381–410].[33] Moreover, in almost all cases where the origin of grammatical case systems can be traced, they derive from frozen spatial metaphors – see [Anderson 1971]: something was done *by* X, that is, when *X* was close by and therefore probably responsible; and it was done *for* Y, that is, in front of *Y*, and hence probably for *Y*'s sake. Other languages, not least those where case is grammaticalized as inflection, underscore the point – see also [Anderson 2006: 115–148].

It thus seems fully justified to speak of the "embedding" for instance of relative clauses, using metaphorically a term whose genuine meaning is spatial (namely to place something within another thing or material): the syntactical operations in language really seem to imitate spatial activity.[34]

32 [As explained in the abstract of the article (p. 518), "cognitive maps" are neurological models of space, which "probably characterize all mammal species. The human cognitive map appears to be unique, however, in being closely related to communication". / JH]

33 Cf. also a commentary by Ron Wallace in [Burling 1993: 43*f*], and William Calvin's arguments [1983: 121] that "enhanced throwing skill could have produced a strong selection pressure for any evolutionary trends that provided additional timing neurons. This enhanced timing circuitry may have developed secondary uses for language reception and production".

34 According to scattered observations by Piaget, it is also "a characteristic of operatory thought that it achieves at the level of thought the same decentration, reversibility, and composability which

In [1980: 248*f*] Noam Chomsky suggested in passing that "certain forms of mathematical understanding – specifically, concerning the number system, abstract geometrical space, continuity, and related notions" belonged, along with the language faculty, to a set of domains in which "humans seem to develop intellectual structures in a more or less uniform way on the basis of restricted data". Already in [1975], James Hurford analyzed numerals from the point of view of generative grammar, in what he later characterized as "a paradigm example of Kuhnian normal science" [Hurford 1987: 43]".

In this last-mentioned work, Hurford was led (p. 305*f*) by analysis of universals and universal irregularities in the formation of number systems and comparison with other features of language to the conclusion that only one of a list of five innate contributions to the number faculty – namely the "Cardinality Principle", the "disposition to make the sizeable leap from a memorized sequence of words to the use of these words expressing the cardinality of collections"[35] – "is special to numeral systems; the rest are very familiar in human language more generally"; further, that these five

> innate capacities are sufficient to determine the number faculty in Man, but insufficient to determine the universal morphosyntactic peculiarities found in the human linguistic systems that express number. Man has the capacity for language and for number, capacities which his ancestors at some stage lacked. Children, born with the capacity to acquire language and number, acquire them simultaneously, and this simultaneity is significant.[36] Language is the mental tool by which we exercise control over numbers. Without language, no numeracy. […]. The capacity to reason about particular numbers, above about 3, comes to humans only with language.
>
> If the human number faculty itself is largely a by-product of innate linguistic

was achieved at the sensori-motor level during the second year of life", as summed up in [Høyrup 2000: 249].

[35] The others are: (i) "The concepts of collection and individual object, and the relation between them"; (ii) "The ability to represent arbitrary links between signified and signifier (the Saussurean Sign)"; (iv) "The ability to acquire and control syntactical rules forming longer expressions out of the simple vocabulary, together with associated semantic interpretation rules"; (v) "The ability to assemble such rules into highly recursive rule sets".

To the last point one should perhaps add the qualification that "Every language has a numeral system of finite scope" [Greenberg 1978: 253]. In contrast to what occurs in certain written number systems (not least in place value systems), the recursiveness in any actual spoken language, though possibly high, is never unlimited.

[36] [Actually this simultaneity should probably be formulated differently. Both according to Piaget's results and my own observations, the integration of cardinality and ordinality, the certainty that loops are not permitted in the number jingle, and the immediate rejection of repetition of the same item twice in counting, only turn up around the age of five to six. Language, of course, is acquired before; but the acquisition of recursive syntax, not least the use of relative clauses, occurs around the same time [Romaine 1988: 232*ff*]. / JH]

> capacities, the linguistic subsystems dealing with number are shaped by further principles, which are not innate in individuals. The two main such principles are:
>
> Languages and their subsystems grow gradually over time. Their structures exhibit traces of this growth in the form of discontinuities and irregularities.
>
> Pragmatic factors make certain forms favoured for communication and such pragmatic preferences become grammaticalized, that is regarded by new acquirers as having the status of grammatical rules.

So, according to Hurford, the understanding of the possibility to count a collection of items (the "cardinality principle") may be a language independent universal (where he overlooks the integration with ordinality, cf. note 36); but in his view the embedding involved in the construction of higher numerals is transferred from the corresponding structure in general language, and has no independent status.

If we regard number systems alone, it is indeed close at hand to regard their pragmatic characteristics (*thirteen* instead of **three-ten*, Italian *sedici* for 16 but *diciasette* for 17, etc.) as manifestations of features that characterize language in general. Possibly, this analysis might even be projected upon written number systems and abacus-type representations and their kin – for instance, the cancellation of Stevin's place identifications might be seen as an analogue of the English deletion of the relative pronoun when it occurs in object position.

However, the inclusion of symbolic algebra in the panorama suggests a somewhat different interpretation. As we have seen, it is exactly when symbolism leaves language efficiently behind that it develops the capability of multiple embedding. Moreover, this embedding refers *directly* to spatiality. This is true of the new root sign that replaced ℞, and which allows that we write

$$\sqrt[3]{42+\sqrt{1700}}+\sqrt[3]{42-\sqrt{1700}}$$

instead of Cardano's "℞.V: cubica 42 p:℞1700 p. ℞V: cubica 42 m: ℞1700"; but it is also true of the various types of parentheses, which all suggest an actual enclosing – not only our modern ( ), [ ], { } and ⟨⟩ but also Bombelli's L. … ⅃ [37] and Descartes' $\left.\begin{matrix}+p\\-q\\+r\end{matrix}\right\}$. No mathematician ever had the idea to enclose something in)…(or }…{ or anything similar. The use of < and > for "smaller than" and "larger than" are also directly linked to that possibility of repeated embedding which corresponds to the transitivity (and actual spatial meaning) of the relations. Other symbols derive from abbreviations (e.g. Σ and ∫ for "sum", δ and Δ for "difference"), but it appears that symbols that have a spatial

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[37] Bombelli's manuscript shows that he intended the even more explicit └────┘ (with embedding └ └──┘┘) – but this was asking for more than the typesetter would accept. See the reproduction of a manuscript page in [Bombelli 1966: xxxiii, fig. 2].

interpretation are directly iconic, and that their character is in disagreement with Saussure's principle that the relation between the linguistic signifier – the actual shape of a word – and its signification is generally arbitrary.[38] Mathematical symbolism seems to be tied *directly* to our capacity for processing spatial information, and not only indirectly through our linguistic-syntactic capacity.

Seen in this light, even the number faculty may be less subordinated to the language faculty than concluded by Hurford for anything going beyond the cardinality principle, and connected to spatial connection directly and not only through the mediation of general language. As suggested by Noam Chomsky, it might perhaps be involved with "abstract geometrical space, continuity, and related notions".[39]

These (at least partially distinct) couplings of language, number and algebraic symbolization to our faculty for processing spatial information suggests that the shared notion of "embedding" is *more* than a gratuitous metaphor in as far as these three domains are concerned. The "embedding" of theories, however, even in cases where it describes real generalization and is no mere expression of conservative ideology, is not easily linked to spatiality proper, and should probably be understood as a different phenomenon. It should instead be linked to Eugene Wigner's famous "Unreasonable Effectiveness of Mathematics in the Natural Sciences" [1960] – both show that there is some kind of objective truth in mathematics, which does not coincide with the single theory but conditions it.

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38 See the various contributions to [Haiman 1985] for examples of similar iconic exceptions to the general rule in the domain of syntax proper.

39 In an article published after the original version of the present paper was prepared [Hauser, Chomsky & Fitch 2002], Chomsky and two collaborators go even further, reaching a position close to what is suggested here. As summarized in the abstract, they hypothesize that the FLN, the "language faculty in the narrow sense" (which excludes such things as the general sensori-motor and the conceptual-intentional systems)

> only includes recursion [which is furthermore] the only uniquely human component of the faculty of language. We further argue that FLN may have evolved for reasons other than language, hence comparative studies might look for evidence of such computations outside of the domain of communication (for example, number, navigation, and social relations).

Recursion, as also evident in the above formalizations of the place-value system, is closely related to embedding, in particular to multiple embedding (as also made clear in [Hauser, Chomsky & Fitch 2002: 1577]).

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Chapter 22 (Article II.5) What Is "Geometric Algebra", and What Has It Been in Historiography?

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

Much ink has been spilled these last 50 years over the notion (or whatever it is) of "geometric algebra" – sometimes in disputes so hot that one would believe it to be blood.

However, nobody has seemed too interested in analyzing whether others have used the words in the same way as he has himself (*he*, indeed – as a feminist might declare, "all males, of course"). So, I shall try to analyze what concepts or notions have been referred to by the two words – if any.

Contents

Denn eben, wo Begriffe fehlen,
Da stellt ein Wort zur rechten Zeit sich ein.
Mit Worten lässt sich trefflich streiten
Goethe, *Faust* I, 1995-1997

Before “geometric algebra”

Since the possibly earliest written treatment of algebra carrying that name, algebra and the geometry of rectangles and line segments have been linked. When al-Khwārizmī was asked by the caliph al-Ma’mūn to write a brief presentation of the art of *al-jabr wa’ l-muqābalah*, he decided not only to let it contain “what was most subtle in this calculation and what is most noble, and what people need” in various commercial and mensurational practices.[1] Knowing from his familiarity with those who were engaged in translation of Greek mathematics (so we may reasonably surmise) that mathematics ought to be based on argument, he also provided geometric proofs for the algorithmic prescriptions for the solution of the mixed second-degree equation types – not by appealing to the propositions of *Elements* II, which his target group was perhaps not likely to know, but by borrowing from the surveyors’ geometric riddle tradition.[2] This is most obvious in the case of the algorithm for the equation type “possession and square roots equals number”, normally translated $x^2+\alpha x = \beta$,[3] whose algorithm corresponds to the formula

$$x = \sqrt{\beta + (\frac{\alpha}{2})^2} - \frac{\alpha}{2}\,.$$

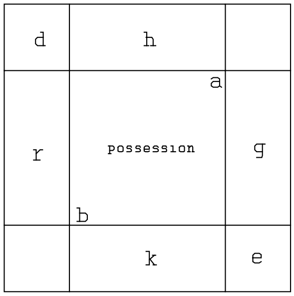
The adjacent diagram shows the justification as it appears in Gerard’s version [ed. Hughes 1986: 237] (the Arabic version only differs by using Arabic letters). It is adapted to the

[1] My translation (as all translations in the following if no translator is identified), here from the French in [Rashed 2007: 94]. When the two aims have been in conflict (as in most of the German quotations), my priority has been literal faithfulness (not always obtainable, however) rather than elegance.

[2] On this, see for instance [Høyrup 2001] ⟦= article I.3⟧.

[3] A pedantic note: Literally (and probably in an original riddle appearance), the translation should be $y+\alpha\sqrt{y} = \beta$. However, by presenting the type “possession equals number” in normalized and the type “roots equal number” in non-normalized form al-Khwārizmī shows that *his* real unknown is the root. If not, a statement “possession equals number” would already be its own solution, while the normalized “root equals number” would be a problem.

In order to see that, one needs to look at Gerard of Cremona’s Latin version [ed. Hughes 1986: 233], which represents an earlier stage of the work than the surviving Arabic manuscripts (see [Høyrup 1998], and cf. [Rashed 2007: 86]). In these, all equation types appear in non-normalized from, even though the primary accompanying illustration examples are invariably normalized.



equation "a possession and 10 roots equal 39 dirham". The central square represents the possession, and the 10 roots are represented by four rectangles, each of breadth $2^1/_2$ and length equal to the side of the square, that is, the square root of the possession. The four corners, with area $4\times2^1/_2\times2^1/_2=25$, are filled out, and the resulting larger square thus has an area equal to 39+25 = 64; etc. This is fairly different in style from what we find in Euclid, not strictly deductive but an appeal to what can be "seen immediately".[4]

According to the rules of grammar, "geometric algebra", whatever it means, must refer to some kind of algebra, only modified or restricted by the adjective. Accordingly, this is *not* geometric algebra but merely a justification of a certain algebraic procedure by means of a borrowing from a different field.

Half a century later, Thābit ibn Qurrah offered new proofs [ed., trans. Luckey 1941]. He does not mention al-Khwārizmī at all but only refers to the procedures of the *ahl al-jabr*, the "*al-jabr* people" – presumably those reckoners whose technique al-Ma'mūn had asked al-Khwārizmī to write about. Most likely, Thābit did not regard al-Khwārizmī's justifications as proofs proper; his, indeed, are in strict Euclidean style, with explicit reduction to *Elements* II.5–6.

Abū Kāmil does refer to al-Khwārizmī in his algebra, and he writes a full treatise on the topic; but his proofs are equally and explicitly Euclidean [ed., trans. Rashed 2013: 354 and *passim*]. Roshdi Rashed (p. 37) speaks of Thābit's and Abū Kāmil's proofs as "geometric algebra" (Rashed's quotes).[5] However, what we see is once again *not* ("geometric") algebra but merely a justification by means of a borrowing – this time from rigorous Euclidean and not from intuitively obvious geometry.

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4 This procedure corresponds to the formula

$$x=\sqrt{\beta+4\cdot\left(\tfrac{\alpha}{4}\right)^2}-2\cdot\tfrac{\alpha}{4},$$

not to the algorithm that is used. Al-Khwārizmī must have chosen it, either because it was the first to come to his own mind, or because he supposes his readers to be familiar with this configuration (which corresponds to the riddle "the four sides and the area of a square equal *A*", in circulation since 2500 years in the area and staying alive until Luca Pacioli).

It is easy to make a proof that corresponds directly to the algorithm that is used, and al-Khwārizmī does present it as an alternative. Here, the style is closer to the Euclidean norm, closer also than the proofs corresponding to the two other mixed equation types. The assumption is near at hand that this alternative was added in a second instance, perhaps after discussion with mathematicians more engaged in Euclid.

5 Rashed identifies it with what "certain historians since Zeuthen have erroneously believed to find in Euclid and Apollonios, among others" – mistakenly, as we shall see.

Later Arabic algebrists are no different, and there is no reason to discuss them separately. The same can be said about Fibonacci's *Liber abbaci*, and about the use of geometric justifications from Pacioli to Cardano and his contemporaries.

Slightly different is the case of Jordanus of Nemore's *De numeris datis*. In his attempt to create a theoretically coherent stand-in for Arabic algebra based on axiomatic arithmetic,[6] Jordanus created the arithmetical (and quasi-algebraic) analogues of a number of theorems from *Elements* II (and much more). If the phrase had not already been occupied by a different signification, it would not be totally misleading to speak of this reversely as "algebraic geometry", that is, geometry translated into something like algebra. In any case, it is no more "geometric algebra" than what we have already discussed. Nor is, of course, Pedro Nuñez' or René Descartes' use of algebra as a tool for solving geometric problems (on very different levels, to be sure).

When did it start? Tannery or Zeuthen?

In conclusion, we find no "geometric algebra" in the mathematics that was produced since algebra got its name if not necessarily its essence (whether we identify this essence with equation techniques or with Noether-Artin theory). If it makes sense to speak about "geometric algebra", then it must be as a description of techniques that antedate al-Khwārizmī. And that is indeed what those who speak about it have done, with the exception of Rashed (and a few lesser figures). Since none of those who produced mathematics at that moment can have had the idea that they worked on some kind of algebra, "geometric algebra" can only be an interpretive tool, and thus a tool wielded by historians of mathematics.

When discussing historiography, some writers (mostly but not only French) point to Paul Tannery as the one who introduced the idea of "geometric algebra". This is misleading. The onslaught on the idea was launched by Árpád Szabó in [1969: 457*f*], and we may therefore look at what is said there. Szabó refers to an article from Tannery's hand, originally from 1882 but republished in [Tannery 1912: 254–280] when stating that

⟨1⟩ Those propositions in Euclid which are habitually – since a work by P. Tannery – regarded as "algebraic propositions in geometrical dress – have in reality only this much to do with algebra that *we* can indeed quite easily point to *our* algebraic equivalents of these propositions.

He quotes only Tannery's title "De la solution géométrique des problèmes du second degré", and would indeed have been unable to find the idea of "algebraic propositions in geometric dress" expressed in the article. Instead, Tannery [1912: 254] points out exactly what Szabó parades as his own objection:

[6] For this interpretation of Jordanus's intentions, see [Høyrup 1988: 335*f*].

⟨2⟩ When we speak about a second-degree problem, by our educational habits we are immediately brought to think of the general equation:

$$x^2 + px + q = 0.$$

Maybe Szabó has been entangled in his polemical intention – p. 456 n. 3 he speaks scornfully of Thomas L. Heath's translation of the *Elements* as "his compilation"; alternatively, he is a prisoner of his own educational habits and does not know that the mathematical notion of "second degree" is not restricted to algebraic equations, and was not so in Tannery's times.[7] Slightly later in Tannery's text it is made clear that the more abstract *problem* can be expressed *either* in terms of an algebraic equation *or* as a geometric problem.

In an article from 1880, "L'arithmétique des Grecs dans Pappus" (reprinted in [Tannery 1912: 80–105]) it becomes clear that the idea of translation between mathematical disciplines is not totally foreign to Tannery. *But it goes the other way.* In a discussion of Pappus's "means" he points out that the calculation of the sub-contraries to the harmonic and the geometric mean involves second-degree equations. He goes on [Tannery 1912: 93]:

⟨3⟩ We are led to the conclusion that the inventor knew how to resolve these equations; it is hardly doubtful, after what we know about works made during this epoch (that of Plato) that is was relatively easy for him to find the geometric solution; but since the theory of *means* was, after all, a speculation about numbers, there are strong reasons to believe that he already knew to translate into an arithmetical rule the construction to be made geometrically.[8]

In one passage – but only one, as far as I can find out – in the three volumes of Tannery's *Mémoires scientifiques* that are dedicated to the "exact sciences in Antiquity" does Tannery speak of "geometric algebra", namely in an article from 1903. The context is a discussion of Greek mathematical *analysis* and *synthesis*, and here Tannery [1915: 167*f*] states that the geometric language developed in the fourth century, combining dia-

[7] One needs only take a look at Michel Chasles' *Rapport sur les progrès de la géométrie* from [1870], where second-degree curves and surfaces turn up repeatedly. These can certainly be described in analytic geometry by second-degree equations, but their characteristic geometric properties do not depend on the choice of that tool – in projective geometry, second-degree curves are those that are equivalent to circles.

It can be added that even an "equation" is not in itself algebraic – that depends on the way it is solved. The use of nomograms is one non-algebraic alternative; ⟦earlier alternatives are the use of a single and a double false position – not to speak of such timeless methods as more or less reasoned guessing and trial-and-error⟧. Cf. below, quotation ⟨69⟩.

[8] Tannery, we notice, speaks of arithmetic, not algebra. The rules in question (the counterparts of *Elements* II.5–6 and *Data* 84–85) are indeed exactly those which I argued (*pace* Rashed) should not be seen as algebra, at least not when used as mere justifications of an algorithm.

grams and words,

⟨4⟩ presented at the same time all the advantages of the use of letters in Viète's analysis, at least for powers 2 and 3. They had thereby been able to form, probably already at the time of the first Pythagoreans, a veritable geometric algebra for the first degrees, with very clear awareness that it corresponded precisely to numerical operations.

Even though they did not, on the other hand, reach the general concept of coordinates, their way to examine the conics was fully analogous to our analytical geometry […].

Here, Tannery takes over the phrase Zeuthen had coined in 1886 (see imminently), but with a slight reserve ("a veritable …"). In *La géométrie grecque* [Tannery 1887], the phrase does not turn up at all, it seems.

In conclusion: Tannery did not originate the idea of a "geometric algebra"; he used the phrase in a single case only; and he did not use other terms for what Szabó (or some later user) took it to cover.

Since Szabó (and various followers of his) claim that Hans Georg Zeuthen borrowed the idea of a "geometric algebra" from Tannery, and since Zeuthen does use the expression in *Die Lehre von den Kegelschnitten im Altertum* from [1886] (first Danish edition in 1884), Zeuthen is the likely originator.[9] Then what did he mean by it?

Not what Szabó and many others believe or at least claim. Zeuthen was a mathematician engaged in advanced geometry. His starting point [Zeuthen 1886: 6] was the *theory of proportions* as provided ("as generally assumed", thus Zeuthen) by Eudoxos with a new and generalized foundation, eventually adopted by Euclid as a way to handle the similarity of figures. The agreement in terminology and propositions makes it perfectly clear – Zeuthen again – that the Greek mathematicians were fully aware of the link between the arithmetical proportion theory of *Elements* VII–IX and the general theory of *Elements* V:

⟨5⟩ From this follows, however, that also when using the propositional instruments of the theory of proportions, the ancients – just as we, when we express our algebraic operations in proportions – were able to use the thought of the calculational operations underlying the proportions as personal inspiration.

According to present-day conceptions, however, a use of proportions that can somehow be mastered is inseparable from the employment of a symbolic language that makes manifest their connections and the transformations that are possible according to familiar theorems, and allows one to impress them firmly in memory. Truly, Antiquity had no such symbolic language, but a tool for *visualization of these as well as other operations in form of the geometric*

[9] That is also the opinion of Bernard Vitrac [1990: I, 366], even though he sees in Tannery's reference to a "geometric algorithm" in the 1882 article the origin of the "algebraic interpretation" of *Elements* II (with an incomplete bibliographic reference to Tannery's article).

> *representation and handling of general magnitudes*[10] *and the operations to be undertaken with them.*

Symbolic algebra and geometry are thus seen as parallel in this respect, none of them expresses the other. If anything, Zeuthen claims that *we* make use of an "algebraic arithmetic of proportions" and *the Ancients* of a "geometric arithmetic of proportions".

A description follows of how a line in a diagram, even though its magnitude is actually determined, can function as a representative of a completely general magnitude, restricted only by the explicitly assumed presuppositions. Further (p. 7),

⟨6⟩ The application of this tool allowed one to continue irrespectively of the discovery of irrational magnitudes. This discovery, which hampered the use of arithmetical tools, would for that very reason be particularly favourable for the development of that geometric tool.

This leads to the introduction of the concept of a "geometric algebra" (which, in the way it is explained by Zeuthen, can legitimately be considered a concept):

⟨7⟩ In this way, a *geometric algebra* developed; one may call it thus, since, on one hand, like algebra, it dealt with general magnitudes, irrational as well as rational, on the other, because it used tools other than common language in order to visualize its procedures and impress them in memory. In Euclid's time, this geometric algebra had developed so far that it could handle the same tasks as our algebra as long as these did not go beyond the treatment of expression of the second degree.

That is, Zeuthen uses the term not because ancient Greek geometric theory (or a part of it) "translated" algebraic propositions or procedures but because it *fulfilled analogous functions*.[11] That is also stated in the *Vorrede* (pp. IX–X), which promises to show

⟨8⟩ that geometry with the ancients was developed not only for its own sake, but at the same time it served as an instrument for the theory of general magnitudes, just as algebra today, and that in this respect the doctrine of conics went beyond elementary investigations.

This must be kept in mind when Zeuthen further on in the book formulates himself in a way that suggests a more directly algebraic reading of ancient geometry – for instance on p. 12, when it is stated that

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10 A "general magnitude" (*allgemeine Größe*) is explained thus on p. 3: Through the Eudoxean definition of a proportion,

> these definitions of the magnitude of a ratio in its relation to others were the same as those that characterize the general magnitudes which underlie the algebra of today, and which continuously go through all values, not only such as stand in a rational ratio to a certain unit.

11 Analogous, not fully identical functions. Zeuthen points out repeatedly when one of the two representations (not always the same) is more flexible than the other.

⟨9⟩ The first 10 propositions in the Second Book of Euclid can be written in the following way:

$$1.\ a(b+c+d+\ldots) = ab+ac+ad+\ldots,$$
$$2.\ (a+b)^2 = (a+b)a+(a+b)b,$$
$$[\ldots]$$

That this is a shorthand and no interpretation of "what really goes on" is made clear on p. 13, which explains that

⟨10⟩ Our first equation merely expresses that a rectangle is cut by parallels to one of the sides (the height) in new rectangles, whose bases together make up that of the given rectangle.

When Zeuthen uses symbolic algebra, it is explicitly "our [...] algebraic representation" (p. 18).[12]

It should be kept in mind, however, that Zeuthen sees certain *problem types* as being *above* the distinction between algebra and geometry (as discussed above in connection with Tannery and the "second degré"). They can be expressed and solved by both, but that does not effect the problem itself – even though Zeuthen allows himself to speak of them as equations he does not believe (as do many later historians of mathematics, nobody mentioned, nobody forgotten) that a problem is in itself algebraic just because *we* are tempted to solve is by means of algebraic manipulations. That his "equations" are not meant to be algebra expressed through geometry but as descriptions of genuinely geometric procedures can also be seen in the reference on p. 21 to Apollonios's "use of the application of areas or quadratic equations".

Further on, first phase: Thomas Heath and Moritz Cantor

In [1896], Heath published a translation of Apollonios's *Conics* "edited in modern translation" with a long introduction; this introduction is what is of interest here. Heath is close to Zeuthen (even though he allows himself to disagree on certain points, for instance on p. lxxii), and takes over Zeuthen's concept of a "geometric algebra" identified with proportion theory combined with the application of areas (pp. ci–cv). But he is adamant, already in the first lines of the preface (p. vii), that Apollonios reaches his results "by purely geometric means".[13] On p. cxi he also points to the contrast between

12 And, as Ivo Schneider [2016: vii] points out:

In order to judge the adequacy or inadequacy of such a request [that of Sabetai Unguru to rewrite the history of Greek mathematics completely, see below] it is, for example, necessary to distinguish whether an author represents the contents of a Greek mathematical text in algebraic dress while referring to the underlying geometric argumentation of the original, or he claims the algebraic representation to correspond to the proper thought of the Greeks.

13 Actually this is said about "what amounts to the complete determination of the evolute of any

Apollonios's geometric method in *Conics* III.26 and Pappos's treatment of the same matter (*Collection* III, lemma 4), which proceeds "semi-algebraically".

So, Heath had understood Zeuthen perfectly, and uses the concept with the same care but perhaps with greater sharpness. When stating (p. cii) that the theory of proportions (one of the two constituents of his geometric algebra) "is capable of being used as a substitute for algebraical operations" this is meant transhistorically, not (as Szabó would have it) as a claim that the Greeks were in possession of algebra and then created a substitute.

In the preface to his similar *The Works of Archimedes* [1897: xl] as well as in his *History of Greek Mathematics* [1921: 150–153], we find the same explanation though abbreviated. In the latter work we also find the idea that the geometric algebra was used to solve numerical problems; this is concluded from the assumed invention of *Elements* II.9–10 for the purpose of "finding successive integral solutions of the indeterminate equations $2x^2-y^2=\pm1$". This was a dubious assumption already at the time (it has to do with the "side-and-diagonal-number algorithm" ⟦see article I.10⟧), but in any case this shows us that Heath did not believe in the existence of an arithmetical algebra that was translated into geometry.

Finally, in his translation of the *Elements* [1926: I, 372–374], Heath returns to the matter, still emphasizing that "geometrical algebra" builds on application of areas and proportion theory combined, and still in terms that echo those of Zeuthen. On p. 373 it is further stressed how important it is

⟨11⟩ to bear in mind that the whole procedure of Book II is geometrical; rectangles and squares are shown in the figures, and the equality of certain combinations to other combinations is proved by those figures.

So, the semi-algebraic way to prove the propositions from II.2 onward presumably introduced by Heron is argued to be later. Once again, that does not suggest the hypothesis that *Elements* II.1–10 should be a translation of a pre-existent algebra.

Slightly later (p. 383) comes the line which provoked Szabó's disdain for Heath's "compilation", namely "Geometrical solution of a quadratic equation". It turns out to be a headline, covering a discussion of *how* proposition II.5 and 6 can be used to solve "problems corresponding to the quadratic equations which are directly obtainable from them". So, this is no claim about what these propositions *are*, but about what they can be used for (and *were* used for from Thābit onward); and as everywhere, Heath speaks about correspondence, not of identity or underlying/preceding algebra.

The third edition of Moritz Cantor's *Vorlesungen* contains a reference to Zeuthen's idea, namely the passage [Cantor 1907: 285]

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conic", but it sets the tone.

⟨12⟩ The geometric shape, in which those problems (*Data 84–85*, and related matters in the *Elements*) appear, and which not improperly have been designated geometric algebra [a footnote refers to Zeuthen], would not suffice to refute all algebraic awareness […]. Euclid must have had to do with numerical quadratic equations, as this is the only way to explain the creation of Book X of his *Elements* [another footnote referring to Zeuthen].

This seems a very reduced variant of Zeuthen's point of view, and almost a rejection: Cantor sees Zeuthen's "geometric algebra" not as an expression of "algebraic awareness" but as possible objection to the presence of such an awareness (an objection which he then rejects). A comparison with the corresponding passages in the first and second editions [Cantor 1880: 245; 1894: 270] is illuminating. In the former, antedating Zeuthen's book, we find this:

⟨13⟩ The geometric shape, in which those problems appear, would in any case not suffice to refute all algebraic awareness.

The latter has

⟨14⟩ The geometric shape, in which those problems appear,would not suffice to refute all algebraic awareness […]. Euclid must have had to do with numerical quadratic equations, as this is the only way to explain the creation of Book X of his *Elements* [a footnote referring to Zeuthen].

As it becomes obvious, Cantor is not illuminated by Zeuthen's discussion, he simply glues Zeuthen's phrase to a formulation he had made himself years before, as a commentary to the "geometric shape" which should not mislead. His own idea is indeed rather different from that of Zeuthen – there is nothing about analogous function, nothing about a technique involving application of areas *and* theory of proportions. Instead, Cantor sees *Elements* II.5–6 and *Data* 84–85 as instances of a geometric form impressed upon an underlying algebraic awareness. The second part of the formulations from 1894 and 1907, on the other hand, is a genuine borrowing from Zeuthen, but once again it serves as support for a thesis that was already present in 1880.

Interestingly, all three editions contain a Chapter 35 "Number theoreticians, calculators, geometrical algebraists from c. 950 to c. 1100". Well before Zeuthen introduced "geometric algebra", Cantor thus spoke of certain mathematicians as "geometrical algebraists". They seem to be such as used theoretical geometry as a tool for algebra, for example al-Karajī and al-Khāyyamī.

Further on, second phase: Neugebauer and Thureau-Dangin

Neither Heath nor Cantor had any discernible influence on later references to the "geometric algebra". That the notion did not die a quiet death was due to Otto Neugebauer

and his interpretation of *Elements* II in the light of the newly discovered Babylonian "algebra".[14]

Neugebauer's designation of this Babylonian technique as an "algebra" hinged upon his somewhat idiosyncratic delimitation of this latter term as given in the first of three articles "Studien zur Geschichte der antiken Algebra". He says [Neugebauer 1932a: 1] to understand

⟨15⟩ the word "algebra" as substantially broad as possible, that is, I include also problems with a strong "geometric" emphasis, if only they seem to me to be on the way toward a formal operation with magnitudes that is ultimately "algebraic".

That is, what Neugebauer sees as *a developmental step* toward the mature algebra of the 17th century (CE) is *eo ipso* covered by the algebraic heading. "Formal operations" as Neugebauer finds them in the Babylonian context are operations that seem ontologically meaningless, acting on *measuring numbers* but not allowing any corresponding operation on the entities that are measured (adding for instance linear and planar extensions).

The second article, dedicated to Apollonios, starts by arguing why Apollonios is pertinent for a history of ancient algebra [Neugebauer 1932b: 215*f*]: This has, firstly,

⟨16⟩ a purely external reason: certain cuneiform texts, whose appurtenance to the area of algebraic problems is not to be doubted […], call for a precise insight in the Greek theory for second-degree expressions, in particular the "application of areas", as a precondition for a profounder historical interpretation. In this way the ancient theory of conics came automatically into the centre of the investigation.

This must have seemed enigmatic at the moment – the explanation of why Neugebauer sees a connection between the application of areas and Babylonian "algebra" (which he understood as a purely numerical technique) was only to be given in the third article. In 1932, readers will have had to concentrate on the second part of the argument – namely, that

⟨17⟩ in the apparently purely geometric theory of conics much is hidden that can provide us with keys to the so to say latent algebraic components of classical Greek mathematics. Here I do not refer to the familiar fact of the "geometric algebra", which we encounter everywhere; but I think of a wholly different facet of the "algebraic" (I am quite aware that such a conceptual delimitation can be

[14] As anybody familiar with the history of Mesopotamian mathematics knows, a distinction between periods is mandatory – in the present case at least between the Old Babylonian and the Seleucid epochs, from which "algebra" texts are known. But since this distinction is rarely made and never emphasized in the texts I discuss, I shall ignore the wringing of my bowels and speak as they do.

Whether or in which sense Old Babylonian or Seleucid "algebra" are "algebras" is unimportant in the present connection – after all, it depends on definitions.

> challenged): the existence of certain “algorithms”, according to which analogous cases can be dealt with quite schematically. Once such an “algorithm” exists, this may have direct immediate consequences that lead directly into the purely algebraical: Renunciation of homogeneity of the dimensions of the magnitudes that appear, emergence of a conventional symbolism, which then on its part leads to a widening of all conceptualizations, etc. That precisely such things do not occur in Greek geometry is part of our basic arsenal of historical insight. The first impression certainly confirms this claim. In the following I shall, however, try to produce the proof for specific examples that the external construction may differ strongly from the inner motivation of the demonstrations, and that precisely in this substructure very much hides which in a certain sense can be characterized as “algorithm”.

As we see, Neugebauer does not claim to continue Zeuthen’s mode of analysis – it is simply dismissed as uninteresting old stuff. Instead, what he says here is in line with his earlier idiosyncratic notion of algebra as concerned with “formal operations”; the reference to “algorithms” merely introduces a new facet. Neugebauer’s underlying algebra is not defined from method nor from analogous use, as is Zeuthen’s geometric algebra. It seems rather to be determined through opposition to the surface appearance of Greek geometry, which Neugebauer then argues *is* sometimes only surface.[15] Below the surface, he sees it as much closer to the Babylonian numerical technique that one would expect; the difference is mainly one of style (p. 217):

⟨18⟩ So, when I claim that there may be a deep difference between outward construction and inner method in Apollonios’ *Conics*, then I thereby emphasize the necessity of questions that can almost be characterized as dealing with the “history of style”.

This appears to have had even less impact than Zeuthen’s thinking. In principle, it may be justified to launch new ideas in the context of an analysis of the *Conics* if the *Conics* is the text which illustrates their carrying power best. Strategically, however, it is a mistake – the necessary technicalities of the topic reduces the audience to a very restricted circle, as Neugebauer knew – on p. 218 he observes that

⟨19⟩ From Zeuthen’s fundamental work “Die Lehre von den Kegelschnitten im Altertum” (Copenhagen 1886), astonishingly little has entered the literature. This may in part be due to the not always convenient and clear exposition, but in part also on the fact that Apollonios himself is anything but easy to understand.

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[15] Actually, certain formulations indicate that Neugebauer also thought in terms of analogous function – but analogous to recent algebraic theory, not (as Zeuthen) analogy with analytical geometry. Thus, p. 219 n. 5:

> The addition of the conjugated hyperbola is a genuinely new idea (which methodically corresponds precisely to the introduction of “ideal elements” enlarging the area of validity of a formal system).

What gained influence (duly transformed) was the third article, entitled "Zur geometrischen Algebra" [Neugebauer 1936]. At first, Neugebauer speaks about Babylonian mathematics (p. 246):

⟨20⟩ The most important outcome of the interpretation of Babylonian mathematics is the revelation of its algebraic character. I have often pointed out that this character mainly relies on the existence of a symbolic writing system, which in itself allows a kind of formal writing, and analyzed how the emergence of this technique is related to the general history of the Babylonian culture.

That we really have to do with essentially algebraic matters follows, in spite of frequent (yet not at all exclusive) geometric dressing, from the repeated appearance of non-homogeneous expressions (addition of "segments" to "surfaces" and "volumes". Similarly, "days" and "people" are added without reserve).

The first paragraph of the quotation refers to Neugebauer's belief that the use of ideographic writing in the Babylonian texts should be understood as an algebraic symbolism. This had been more fully explained in Neugebauer's *Vorgriechische Mathematik* [Neugebauer 1934: 68]:

⟨21⟩ Any algebraic working presupposes that one possesses certain fixed symbols for the mathematical operations as well as the magnitudes, Only the existence a conceptual notation of this kind makes it possible to combine magnitudes that are not numerically identified with each other and to derive new combinations from them.

But a symbolic notation of this kind was automatically at hand in the writing of Akkadian texts. As we have seen, two different ways to express oneself were indeed at hand here: either to make use of the syllabic way of writing, or to write with ideograms. Most Akkadian texts shift continually and quite arbitrarily between the two ways of writing. Now this outcome of a purely historical process was of fundamental importance for the mathematical terminology. There, indeed, it became the fixed habit to write mathematical concepts ideographically, operations as well as magnitudes. That means, then, that in a text written in Akkadian precisely the decisive concepts were written by means of conventional single symbols. Thereby one disposed from the very beginning of the most important basis for an algebraic development, namely an adequate symbolism.

For the present purpose it is immaterial that Neugebauer is demonstrably mistaken on this account.[16] Even if he had been right, however, we would once again be confronted with an idiosyncratic understanding – there is no request that *operations* be performable

16 Firstly, only a few "magnitudes" and one operation (not really an operation, but functioning as such) is almost always written with a single-symbol ideogram. All genuine operations are sometimes written in one, sometimes in the other way even within the same text. Secondly, the ideographic writing (and for that matter, but less often, syllabic text) is sometimes ambiguous and only to be understood from the global context.

at the level of "symbols". In Nesselmann's classical terms [1842: 302], this would be a case of *syncopated*, not symbolic algebra.

Returning to the article on "geometric algebra", we see that the other argument for the algebraic character of Babylonian mathematics is the use of "formal" operations, that is, of problem statements in terms of the measuring numbers of magnitudes.

The next section then deals with Greek geometry. It begins thus [Neugebauer 1936: 249]:

⟨22⟩ Zeuthen we may thank for an insight that is fundamental for the understanding of the whole of Greek mathematics, namely that in particular Book II and VI of Euclid's *Elements* express geometrically problems that are properly algebraic. In particular he has pointed out in many passages that the problems about "application of areas" in Book VI and the appurtenant propositions of the *Data* contain a full discussion of the equations of the second degree. He has further shown how this "geometric algebra" forms the basis for the "analytical geometry" of Apollonios's *Conics*, whose designations "ellipse", "hyperbola" and "parabola" still point back to the fundamental cases of the "application of areas" today.

This distorts what Zeuthen actually says. Instead of analogous use, we get a statement about an algebraic essence of geometrically formulated problems; proportion theory seems to have left the scene completely (but see note 17). Moreover, as we have already seen, Neugebauer's idiosyncratic understanding of "algebra" differs strongly from what Zeuthen had meant by that word.

Neugebauer continues (pp. 249–250):

⟨23⟩ The central problem that remains after Zeuthen's investigations is the question: *How does one come to so peculiar questions as asked by the "application of areas":* To "apply" a given area on a given line in such a way that a rectangle of given shape is missing ("elliptical" case) respectively in excess ("hyperbolic" case).

The answer to this question, that is, to the question about the historical cause of the fundamental problem of the whole of the geometric algebra, can now be given in full: It lies, on one hand, in the request of the Greeks (following from the discovery of irrational magnitudes) to ensure the general validity of mathematics through a transition from the domain of rational numbers to that of general ratios between magnitudes;[17] on the other in the ensuing need *to translate also the results of pre-Greek "algebraic" algebra into a "geometric" algebra.*

Once one has formulated the problem in this way, then everything else is completely trivial and provides *the smooth junction of Babylonian algebra to Euclid's formulations.* Still, one must then start from the state of Babylonian

[17] Though it is not mentioned earlier, Neugebauer is thus aware that it is not so much geometry as the theory of proportions that has to take the place of arithmetic.

> algebra […]: the "normal cases" of the Babylonian equations of the second degree are the problems, to determine two magnitudes x and y from
>
> (1) $xy = a \quad x+y = b$
>
> respectively
>
> (1*) $xy = a \quad x-y = b$
>
> Indeed, the immediate translation into geometry obviously runs: Given a segment b and an area c^2 […]. On shall divide b into two partial segments x and y in such a way that $x+y = b$ (the discussion of case (1) is sufficient) and that $x \cdot y = c^2$.

After a short scrutiny of how the application of an area with deficit works Neugebauer can conclude (p. 251):

〈24〉 Thereby is has been shown that the whole application of areas is nothing but the mathematically evident geometric formulation of the Babylonian normal form of quadratic equations. It is equally trivial to show that even the Greek solving method is nothing but the literal translation of the Babylonian formula (2):

$$(2) \qquad \left.\begin{matrix} x \\ y \end{matrix}\right\} = \tfrac{b}{2} \pm \sqrt{\left(\tfrac{b}{2}\right)^2 - c^2}$$

This is the foundation for subsequent references to the "geometric algebra" (again with further reinterpretations). The riddle why somebody should ask the odd questions of applying an area along a line as a rectangle with deficit or excess *before* its usefulness in the theory of conics had become manifest, together with the structural similarity of both question and method leads Neugebauer to the following scenario: *The discovery of incommensurability*[18] *made a direct continuation of the Babylonian arithmetical algebra impossible, at least within theoretical mathematics. In order to save its results, the Greeks therefore undertook to translate these into the language of geometry.* Basically and below the surface, however, the translations remained algebraic – thus Neugebauer.

That this article set the scene for the future understanding of "geometric algebra" is illustrated by Len Berggren's introduction to the topic in his "History of Greek Mathematics: A Survey of Recent Research" [1984: 397]:

〈25〉 The scholarly point at issue here is whether it is historically justified to interpret parts of Greek mathematics, typified by Book II of Euclid's *Elements*, as translations of Babylonian algebraic identities and procedures into geometric language.

Soon after the appearance of the article "Zur geometrischen Algebra", Neugebauer switched his main interest to astronomy [Høyrup 2016: 185*f*]; after the publication of the third volume of the *Mathematische Keilschrifttexte* in 1937 his only major publication on Babylonian mathematics was the volume *Mathematical Cuneiform Texts*, coedited with

[18] Already taken into account by Zeuthen but brought to the fore by the presumed identification of a "foundational crisis" to which this discovery should have given rise [Hasse & Scholz 1928].

Abraham Sachs in 1945. So, he had little occasion to return to the matter, except in the popularization *The Exact Sciences in Antiquity* (first published in 1951, second edition in 1957 reprinted in [1969]), where he presents the same scenario rather briefly, concluding the topic with these words [Neugebauer 1969: 150]:

⟨26⟩ Attempts have been made to motivate the problem of "application of areas" independently of this [Babylonian] algebraic background. There is no doubt, however, that the above assumption of a direct geometrical interpretation of the normal form of quadratic equations is by far the most simple and direct explanation. I realize that simplicity is by no means equivalent with historical proof. Nevertheless the least one must admit is the possibility of the above explanation.

In the article "The Survival of Babylonian Methods in the Exact Sciences of Antiquity and Middle Ages", Neugebauer still argues in favour of the link [Neugebauer 1963: 530]:

⟨27⟩ For Greek mathematics the picture now becomes quite clear. It hardly needs emphasis that one can forget about Pythagoras and his carefully kept secret discoveries. It is also clear that a large part of the basic geometrical, algebraic, and arithmetical knowledge collected in Euclid's *Elements* had been known for a millennium and more. But a fundamentally new aspect was added to this material, namely the idea of general mathematical proof.

The notion of a "geometric algebra", however, does not occur.

Not only Neugebauer virtually stopped his active work on Babylonian mathematics in 1937. So did François Thureau-Dangin. However, his tense but polite race with Neugebauer during the 1930s is a likely background to an article "L'origine de l'algèbre" which he published in [1940],[19] even though the direct occasion (p. 292) was a recent claim by Gino Loria, who,

⟨28⟩ in order to draw a limit between arithmetic and algebra, has proposed to characterize algebra by the methodical use of symbols for known and unknown quantities as well as for operations.

As Thureau-Dangin further reports Loria, algebra would thereby become a wholly modern method, not known by the ancients.

Thureau-Dangin instead (p. 293) wants to understand algebra as

⟨29⟩ an application of the analytical method in the resolution of numerical problems. The essence of the procedure is that one thinks of the number as a known number

[19] Thureau-Dangin is also in dialogue with an article by Solomon Gandz from [1937], "The Origin and Development of the Quadratic Equations in Babylonian, Greek, and Early Arabic Algebra". In this article, Gandz speaks as a matter of course about Euclid's "geometric algebra (II, 1–10)". Since Gandz says nothing more, it is impossible to know whether he was inspired by Zeuthen, by Heath, by Cantor, and/or by Neugebauer.

and formulates the problem, considered as already solved, in the shape of an equation, then transforming this equation step by step, which leads to a final shape in which the unknown number appears alone on one side and a known quantity on the other. If more than one unknown appears in the problem, it is reduced to a single unknown through elimination of the others by appropriate procedures.

Thureau-Dangin therefore sees a long prehistory, discussing at first the Arabic technique that provided algebra with its name (mentioning also Diophantos's explicitly analytic rules for reducing an equation). He then goes back in time (p. 295):

⟨30⟩ As long as only rational numbers were recognized as numbers and one did not know how to calculate with irrational radicals, the field of application of the algebraic method remained strictly limited. This is the likely reason that the Greeks were brought to develop a method which Zeuthen has called geometric algebra. Instead of operating on numbers, as does algebra proper, the geometric algebra operated on geometric magnitudes(line segments and rectilinear figures), that is, on continuous magnitudes, commensurate as well as non-commensurate.

A philologically informed discussion of the application of areas follows (simple, or with excess or deficiency). In this connection there is also a reference on p. 296 to Tannery's above-mentioned article from 1882.

Thureau-Dangin does not mention the use of proportion techniques; not does he take up the use of "geometric algebra" in the *Conics*. On their part, neither Tannery nor Zeuthen speak of the analytic character of algebra (both probably take it for granted, after all it is inherent in the name "analytic geometry"). All in all, however, Thureau-Dangin is a more faithful reader of the two than Neugebauer, who had used Zeuthen freely for his own purpose (and not mentioned Tannery).

After a reference to Tannery's suggestion of the primacy of arithmetic, Thureau-Dangin goes on with a presentation of Babylonian "algebra". He considers it an algebra in his sense, even though, as he says, the texts (as he read them) are purely synthetical, being convinced that the synthesis must have been based on some kind of analysis (p. 300):

⟨31⟩ In general, the Babylonian algebraic problems are resolved in a purely synthetic fashion: the analysis that has guided the scribe to the operations which he performs, the very shape of the equations which he thinks of, can only be reconstructed by conjecture.

Thureau-Dangin does not think of the geometric algebra of *Elements* II etc. as a translation of Babylonian knowledge. Truly, one might believe this if reading a formulation on p. 309 superficially:

⟨32⟩ Propositions 5, 6, 9 and 10 from the second book of Euclid translate into geometrical terms the equations by which the Babylonian method expresses algebraically the product of two unknowns or the sum of their squares.

But "translate" (*traduisent*) is in the present, not the past tense; it expresses correspondence or perhaps (much less likely) *our* translation. And on p. 300 we read that

⟨33⟩ Let us say it immediately: there is no trace in Babylonia of geometric algebra. In this way the question of the relative age of the two kinds of algebra is decided. In its origin, the geometric algebra is a purely Greek method and the numerical algebra, known by the Greeks, very probably has a Babylonian origin, as we shall see.

All in all, Thureau-Dangin's thinking *around* the idea of a "geometric algebra" is at variance with that of Neugebauer; even their respective ideas about what "geometric algebra" *is* seem discordant.

In the wake

As often happens, others climbed on the shoulders of the giants (not exactly dwarfs, but historians of more normal stature). Did they see longer?

Let us first look at B. L. van der Waerden, who probably exerted the largest influence, in particular through his *Science Awakening*. This work first appeared in Dutch as *Ontwakende Wetenschap* in 1950, which was translated into English in 1954 (I have not seen either of these ⟦[20]⟧). A corrected German edition (*Erwachende Wissenschaft*) appeared in [1956], and a second, further corrected English edition in [1961].

The second English edition was the main channel for spreading a transformed interpretation of Neugebauer's idea, so I shall refer to that. On pp. 118–124 we find a chapter "'Geometric Algebra'", while the immediately following pages 125–126 discuss the question "Why the Geometric Formulation?".[21] The former begins

⟨34⟩ When one opens Book II of the Elements, one finds a sequence of propositions, which are nothing but geometric formulations of algebraic rules. So, e.g., II 1: *If there be two straight lines, and one of them be cut into any number of segments whatever, the rectangle contained by the two straight lines is equal to the rectangles contained by the uncut straight line and each of the segments,* corresponds to the formula

$$a(b+c+\ldots) = ab+ac+\ldots$$

II 2 and 3 are special cases of this proposition. II 4 corresponds to the formula

$$(a+b)^2 = a^2+b^2 + 2ab.$$

[. . .]

Quite properly, Zeuthen speaks in this connection of a "geometric algebra". Throughout Greek mathematics, one finds numerous applications of this

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20 ⟦In the meantime I *have* got hold the first English translation; its few corrections concern other matters, and therefore it changes nothing in what is said in the following.⟧

21 In the German edition [van der Waerden 1956: 193] the corresponding headings are "Die geometrische Algebra" (without quotation marks) and "Wozu die geometrische Einkleidung?".

> "algebra". The line of thought is always algebraic, the formulation geometric. The greater part of the theory of polygons and polyhedra is based on this method; the entire theory of conic sections depends on it. Theaetetus in the 4th century, Archimedes and Apollonius in the 3rd are perfect virtuosos on this instrument.

This is accompanied by diagrams, close to those of the *Elements* though not identical.

As we see, there is no explanation here of what is meant by algebra; nor is there any earlier explicit discussion. Possibly, the author of *Moderne Algebra* thought the answer to be obvious (see quotation ⟨63⟩ for van der Waerden's explanation of his usage in 1976). In any case, his implicit understanding owes nothing to that abstract algebra whose prophet he had been. Positively, he seems to have endorsed Neugebauer's argument from formal operations: on p. 72 it is argued that the Babylonians may well have found some of their rules from geometric diagrams, as suggested among other things by their geometric terminology.

⟨35⟩ But we must guard against being led astray by the geometric terminology. The thought processes of the Babylonians were chiefly algebraic. It is true that they illustrated unknown numbers by means of lines and areas, but they always remained numbers. This is shown at once in the first example, in which the area xy and the segment $x–y$ are calmly added, geometrically nonsensical.

Elsewhere it seems that the possibility of direct expression as a symbolic equation and perhaps the use of operations that emulate those made on equations are taken as symptoms of algebraic thought.

As we see, van der Waerden follows Neugebauer, Heath and/or Zeuthen, speaking about the use of geometric algebra in the conics. However, even though van der Waerden must have read Zeuthen's *Lehre von den Kegelschnitten*,[22] he did not understand what Zeuthen had meant by "geometric algebra". On p. 4 he states that

⟨36⟩ Neugebauer, following in the tracks of Zeuthen, succeeded in discovering the hidden algebraic element in Greek mathematics and in demonstrating its connection with Babylonian algebra.

The idea of a hidden algebraic element had been foreign to Zeuthen, as we have seen; moreover, whereas Zeuthen's "geometric algebra" was a synthesis of application of areas and proportion theory (the condition that he could speak of an analogous function), van der Waerden sees the two techniques as clearly distinct and essentially unconnected (pp. 264, p. 266).

Since van der Waerden had seen Neugebauer's "Apollonius Studien" [Neugebauer 1932b] (p. 259 n. 1 contains a reference), and almost certainly Neugebauer's article on

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[22] Mostly, his fairly copious references to the book are not fully specific, but on p. 259 there is a correct page reference.

geometric algebra from [1936],[23] we may assume that van der Waerden's inspiration when he speaks about "geometric algebra" is Neugebauer.

Neugebauer is also the indubitable source for van der Waerden's view of Greek "geometric algebra" as a translation of Babylonian algebraic knowledge into geometric language necessitated by the discovery of incommensurability.

But van der Waerden is no mere copyist or epigone. He develops Neugebauer's idea into a general interpretive tool for Greek mathematics, used, for instance, to analyze the side-and-diagonal numbers (pp. 126*f*),[24] the Delic problem (p. 161) and Theaitetos (p. 119 and *passim*).

On one point, van der Waerden also disagrees rather openly with Neugebauer. While Neugebauer (admittedly only in [1963: 118] in these words, though with a similar attitude in [1933: 316]) would state that it "hardly needs emphasis that one can forget about Pythagoras and his carefully kept secret discoveries", van der Waerden takes from Proclos/Eudemos that the technique of application of areas (and thus the translation of Babylonian knowledge) should be ascribed to the Pythagoreans (p. 118 and *passim*).

Van der Waerden's book covers precisely the two mathematical cultures that are central to the notion of a geometric algebra as developed by Neugebauer. We may also have a brief look at a few general histories of mathematics and see how they deal with the topic.

First Dirk Struik's *Concise History of Mathematics*, published in [1948] (and often later, with revisions). About the mathematics of the Hammurabi epoch Struik states (vol. I p. 26) that here "we find arithmetic evolved into a well established algebra". There is no explanation of what is meant by that, but since Struik also characterizes the Egyptian *pws*- or *pesu*-problems as "primitive algebra" (p. 22), he is likely simply to see problems which *we* would solve by means of algebraic equations as algebra. In any case, the passage together with what follows immediately after it is inspired from Neugebauer's survey article "Exact Science in Antiquity" from [1941: 28*f*]:

⟨37⟩ Those texts [from the First Babylonian Dynasty] are pure mathematical texts, treating elementary geometrical problems in a very algebraic form, which corresponds very much to algebraic methods known from late Greek, Arabian, and Renaissance times.

In Struik's presentation of Greek mathematics, only three passages are pertinent. On p. 58 it is said that

[23] Apart from the obvious though not specific reference "Neugebauer, following in the tracks of Zeuthen", we may observe that [van der Waerden 1938] was published in *Quellen und Studien* B. Van der Waerden can be presumed to have followed the journal.

[24] This could be a borrowing from Heath, cf. above, p. 600.

⟨38⟩ Among these other texts [by Euclid] are the "Data," containing what we would call applications of algebra to geometry but presented in strictly geometrical language,

and on p. 60*f* that

⟨39⟩ Algebraic reasoning in Euclid is cast entirely into geometrical form. An expression $\sqrt{A}$ is introduced as the side of a square of area A, a product ab as the area of a rectangle with sides a and b. This mode of expression was primarily due to Eudoxos' theory of proportions, which consciously rejected numerical expressions for line segments and in this way dealt with incommensurables in a purely geometrical way.

As we see, the problem arising from incommensurability is mentioned, and underlying algebraic thought is taken for granted. Nothing is said, however, about translating and saving Babylonian insights. In the third revised edition [Struik 1967: 52] the sentence "Linear and quadratic equations are solved by geometrical constructions leading to the so-called 'application of areas'" is inserted before "This mode".

Nothing more is said elsewhere about "translation" of Babylonian results (nor is it indeed told in [Neugebauer 1941]). Babylonian algebra is supposed only to have inspired Diophantos (p. 74):

⟨40⟩ The Oriental touch is even stronger in the "Arithmetica" of Diophantos (c. 250 A.D.). Only six of the original books survive; their total number is a matter of conjecture. Their skilful treatment of indeterminate equations shows that the ancient algebra of Babylon or perhaps India not only survived under the veneer of Greek civilization but also was improved by a few active men.

Joseph Ehrenfried Hofmann knows and refers to the Dutch first edition of *Science Awakening* in his equally concise *Geschichte der Mathematik* from [1953]–1957. Even he, however, avoids the idea of translation. In the discussion of the Babylonians in vol. I he does not refer to algebra at all, and only twice (pp. 13, 14) to "equations".

Even when dealing with the Greeks, Hofmann does not endorse the idea of a "geometric algebra", even though he does mention algebra a few times.

About the *Elements* we find on p. 32 that

⟨41⟩ The second book contains algebraic transformations such as the calculations of $a(b+c)$ or $(a+b)^2$ in geometrical dress. This serves the resolution of the general quadratic equations, which is presented through the example $x^2 = a(a-x)$

(a debatable claim, to be sure). On p. 33, now about *Elements* VI,

⟨42⟩ Of particular importance is the handling of quadratic equations through application of areas (in continuation of II, 4/6), that was taken up by Apollonios and reinterpreted.

Two other references speak of algebra proper. So, on p. 42, about Hipparchos:

⟨43⟩ The Muslims also ascribe to him an algebraic work, in which quadratic equations were perhaps dealt with.

This seems to be a reference to what is found in al-Nadīm's *Fihrist* [ed. trans. Suter 1892: 22, 391] – that Hipparchos wrote a book about algebra, and that Abū'l Wafa made a commentary to it provided with geometric demonstrations.[25]

Finally, on p. 45, Diophantos is spoken about:

⟨44⟩ Completely different is the most significant arithmetical work of Diophantos of Alexandria (c. 250 ad), who contrary to Greek manner took up and continued Egyptian-Babylonian tradition.

So, like Struik, Hofmann distances himself indirectly from the idea that the techniques of *Elements* II and VI should be geometric translations of Babylonian results (such translation being "contrary to Greek manner"/*in ungriechischer Weise*). While Struik may not have known about the thesis in 1948 (but in 1967 he certainly knew), Hofmann is familar with van der Waerden's *Ontwakende Wetenschap*. He refers to [Zeuthen 1886] in his bibliographic notes, but like everybody else since Neugebauer (Thureau-Dangin excepted) he ignores Zeuthen explanation of what *he* meant by a "geometric algebra". At least neither he nor Struik claim to follow Zeuthen.

Carl Boyer's *A History of Mathematics* from [1968] looks much more (on this account) like a diluted version of [van der Waerden 1961]. The Babylonians, we understand, had an algebra that is adequately expressed in symbolic equations, and the "geometric algebra" of the *Elements* was a translation of Babylonian knowledge. On p. 33 we read:

⟨45⟩ One table for which the Babylonians found considerable use is a tabulation of the values of n^3+n^2 for integral values of n, a table essential in Babylonian algebra; this subject reached a considerably higher level in Mesopotamia than in Egypt. Many problem texts from the Old Babylonian period show that the solution of the complete three-term quadratic equation afforded the Babylonians no serious difficulty, for flexible algebraic operations had been developed. They could transpose terms in an equation by adding equals to equals, and they could multiply both sides by like quantities to remove fractions or to eliminate factors.

The beginning of the quotation shows that Boyer knows the Babylonian material only from uncritically absorbed hearsay – there is exactly *one* extant Babylonian problem (a cubic problem) where the table in question is used (referred to by the name "equal, one added" – it was actually understood as a tabulation of $n{\cdot}n{\cdot}(n+1)$). Any mathematical reflection would have shown that it can play no role whatsoever in the solution of second-degree problems, whether understood as algebra or not.

[25] Woepcke, who was the first to point to this passage in [1851: xi], abstained from having an opinion whether this suggests something like al-Khwārizmī's demonstrations. Cantor [1880: 313] dropped the doubts.

On p. 34 this follows:

⟨46⟩ The solution of a three-term quadratic equation seems to have exceeded by far the algebraic capabilities of the Egyptians, but Otto Neugebauer in 1930 disclosed that such equations had been handled effectively by the Babylonians in some of the oldest problem texts.

finally, about the Greeks, pp. 85*f* offers this:

⟨47⟩ The dichotomy between number and continuous magnitude required a new approach to the Babylonian algebra that the Pythagoreans had inherited. The old problems in which, given the sum and the product of the sides of a rectangle, the dimensions were required[,] had to be dealt with differently from the numerical algorithms of the Babylonians. A “geometric algebra” had to take the place of the older “arithmetic algebra,” and in this new algebra there could be no adding of lines to areas or adding of areas to volumes. From now on, there had to be a strict homogeneity of terms in equations, and the Mesopotamian normal forms, $xy = A$, $x \pm y = b$, were to be interpreted geometrically. […] In this way, the Greeks built up the solution of quadratic equations by their process known as “the application of areas,” a portion of geometric algebra that is fully covered by Euclid’s *Elements*. Moreover, the uneasiness resulting from incommensurable magnitudes led to an avoidance of ratios, insofar as possible, in elementary mathematics.

This, and in particular the reference to the Pythagoreans as the translators of “Babylonian algebra”, shows that Boyer has learned from [and watered down] van der Waerden rather than Neugebauer.

Apollonios gets a chapter of his own. There are references to equations, and also a remark on p. 172 on the impossibility to consider negative magnitudes in “Greek geometrical algebra”. In the end there is the rather perspicacious remark that

⟨48⟩ Of Greek geometry, we may say that equations are determined by curves, but not that curves were defined by equations,

which can be read as a suggestion that the “algebraic” reading (whether à la Zeuthen or more modernizing) has its limitations.

Morris Kline’s *Mathematical Thought from Ancient to Modern Times* appeared in [1972], after the attacks against the notion of a “geometric algebra” has set in, but it was relatively unaffected by them. It deserves a look.

“Babylonian algebra” gets a section of its own (pp. 8–11). It is, even on the conditions of the interpretations of the times, rather badly informed. It seems that Kline has taken his information about the topic from Neugebauer’s mathematical explanations of *why things work*, which were never meant to interpret the thinking of the Babylonians; sometimes Kline even misses the numerical procedure – cf. [Høyrup 2010: 12].[26]

[26] According to the bibliography for the chapter, Kline’s sole sources for information about

With the Greeks, Kline shows himself to be a mathematician who understands the sources[27] and sometimes forms his own opinions. On p. 49 he introduces geometric algebra in this way:

⟨49⟩ The Eudoxian solution to the problem of treating incommensurable lengths or the irrational number actually reversed the emphasis of previous Greek mathematics. The early Pythagoreans had certainly emphasized number as the fundamental concept, and Archytas of Tarentum, Eudoxus' teacher, stated that arithmetic alone, not geometry, could supply satisfactory proofs. However, in turning to geometry to handle irrational numbers, the classical Greeks abandoned algebra and irrational numbers as such. What did they do about solving quadratic equations, where the solutions can indeed be irrational numbers? And what did they do about the simple problem of finding the area of a rectangle whose sides are incommensurable? The answer is that they converted most of algebra to geometry.

As we see, here is no hint of a translation of Babylonian knowledge, nor is there much of a role for the Pythagoreans. To the contrary, Kline supposes it to be Eudoxos who made the invention, in opposition to the views of his (Pythagorean) teacher Archytas.

About *Elements* II, Kline has this to say (p. 64; and much more, indeed):

⟨50⟩ The outstanding material in Book II is the contribution to geometrical algebra. […] In Book II all quantities are represented geometrically, and thereby the problem of assigning numerical values is avoided. Thus numbers are replaced by line segments. The product of two numbers becomes the area of a rectangle with sides whose lengths are the two numbers. The product of three numbers is a volume. Addition of two numbers is translated into extending one line by an amount equal to the length of the other and subtraction into cutting off from one line the length of a second. Division of two numbers, which are treated as lengths, is merely indicated by a statement that expresses a ratio of the two lines; this is in accord with the principles introduced later in Books V and VI.

About these principles and book V, this is said on p. 70:

⟨51⟩ We know that we can operate with irrationals by the laws of algebra. Euclid cannot and does not. The Greeks had not thus far justified operations with ratios of incommensurable magnitudes; hence Euclid proves this theorem by using the definitions he has given and, in particular, Definition 5. In effect, he is laying the basis for an algebra of magnitudes.

Babylonian mathematics were Neugebauer's *Vorgriechische Mathematik* and *Exact Sciences in Antiquity*, van der Waerden's *Science Awakening* and Boyer's *A History of Mathematics*.

27 Even here, however, Kline mostly relies on the secondary literature such as Heath's *History of Greek Mathematic*, according to what he tells on p. xi. As a matter of fact, however, he has also looked at Heath's translations of the *Elements* and of Archimedes, Apollonios and Diophantos, and further translations of a few other works.

As Heath’s “geometrical algebra” (and Zeuthen’s, but Kline does not refer to him), that of Kline encompasses the application of areas and other operations from *Elements* II.1–10 as well as the theory of proportions. Kline does not explain what he means by “algebra”, but the phrase “an algebra of magnitudes” shows that his understanding is broader than what is suggested by the reference on p. 176 to “heuristic, empirical arithmetic and its extension to algebra”.

And then, on p. 80, we find

⟨52⟩ Book X of the *Elements* undertakes to classify types of irrationals, that is, magnitudes incommensurable with given magnitudes. Augustus De Morgan describes the general contents of this book by saying, “Euclid investigates every possible variety of line which can be represented [in modern algebra] by $\sqrt{\sqrt{a}+\sqrt{b}}$, a and b representing two commensurable lines.” Of course not all irrationals are so representable, and Euclid covers only those that arise in his geometrical algebra.

This last observation is likely to be inspired by [Heath 1926: 4*f*], where a 13 lines long quotation from [Zeuthen 1896: 56] to this effect (but in very different words) can be found.

On p. 88, a strange claim turns up:

⟨53⟩ Euclid’s *Data* was included by Pappus in his *Treasury of Analysis*. Pappus describes it as consisting of supplementary geometrical material concerned with “algebraic problems.”

What Pappos actually says is that the first 23 propositions (of a total of 90) deal with *magnitudes* [Jones 1986: 84]. The only plausible source for Kline’s invention in the literature listed in the bibliography for the chapter is van der Waerden’s statement [1961: 198] that “The ‘Data’ is a book of great importance for the history of algebra” (but Pappos is not mentioned here, only on p. 200, in connection with Euclid’s *Porisms*). Since Kline obviously did not inspect the Euclidean text, it is doubtful whether anything follows as to what he means by “algebra” and “geometric algebra”.

The use of the techniques of “geometric algebra” by Archimedes and Apollonios is mentioned on pp. 108 and 92, respectively. In Archimedes’ case, solution by means of conic sections is counted as “geometric algebra”, which is definitely a broadening of the meaning.[28] Apart from that, there is no reason to go into details.

Inspiration from the Babylonians *is* mentioned, but in connection with Heron (p. 136 – mainly, as a matter of fact, the pseudo-Heronic compilers of the *Geometrica* collection; Kline is certainly not alone in this confusion) and Diophantos (p. 143).

[28] Kline may have got his idea from van der Waerden [1961: 222], who however does not mention “geometric algebra” in this connection. [Heath 1897: cxxv–cxxvii], apparently van der Waerden’s source, explicitly says that the method in question is *no* generalization of the application of areas (and thus, according to Heath’s understanding, not a case of “geometric algebra”).

All in all we see that the various authors using "geometric algebra" as an interpretive tool share the phrase but do not agree upon what it means (with the exception of Zeuthen/Heath and the partial exception of Neugebauer/van der Waerden). Even their notions of "algebra" point in many directions. So, the words of Goethe's Mephisto, originally referring to theology, are adequate also in the present case: "where concepts are absent, there a word arrives at the due moment".

The decade of objections

The continuation is no less adequate: "with words, quarrel is easily made". *Quarrel* was initiated by Szabó in [1969]. We already discussed him. At present we shall restrict ourselves to summing up that he evidently did not bother to find out what Tannery and Zeuthen had actually thought; he took Neugebauer's pretended borrowing from Zeuthen at face value without control and believed from Tannery's title that he could ascribe priority to him.

Michael Mahoney's essay review from [1971a] of the reprint of [Neugebauer 1934] is more thoughtful, but still plagued by statements adopted from hear-say and embellished by free invention. On p. 371 he writes:

⟨54⟩ The group of theorems that embody the Babylonian importation had already attracted the attention of historians of mathematics long before anyone even suspected the existence of Babylonian mathematics. The term "geometrical algebra" was coined by Tannery and adopted by Zeuthen as both men, in the late nineteenth century, tried to make some sense of theorems that seemed to them incongruous to the *Elements* that contained them. No wonder, ran their answer, these theorems seem out of place. They are not geometrical theorems, but algebraic theorems in geometrical guise, as the theorems of Books VII–IX are geometrically-clad arithmetic. What neither man did, however, was to analyse the reasons for his initial discomfiture. Why should the theorems seem incongruous in the first place? What is it about Greek geometry that would make algebraic theorems stick out like the proverbial sore thumb? Van der Waerden and Neugebauer provide the elements of an answer.

Mahoney has obviously not read neither Tannery nor Zeuthen on this account (this is confirmed by the absence of any precise reference). He borrows from Szabó (who is cited slightly earlier), apparently convicted that nobody would dare write with as strong emphasis and conviction as Szabó if hanging in thin air.

On the whole, however, Mahoney accepts Neugebauer's and van der Waerden's supposed facts: That the Babylonians had created a mathematical discipline that is easily explained by means of algebra; and that this was taken over by Greek mathematicians, more precisely by the Pythagoreans (p. 373), and reshaped. His main objection is directed against the interpretation of either discipline as "algebra".

For this, Mahoney builds on *his* conception of what algebra should be (p. 372):

⟨55⟩ What are the characteristics of the algebraic approach? In its most developed form, that is, in modern algebra, it appears to have three: first, algebra employs a symbolism for the purpose of abstracting the structure of a mathematical problem from its non-essential content; second, algebra seeks the relationships (usually combinatory operations) that characterize or define that structure or link it to other structures; third, algebra, as a mathematics of formal structures, is totally abstract and free of any ontological commitments. The first and second characteristics mark algebra as an operational approach to mathematics; the relations symbolized are combinatory operations and the symbolism expresses the constituents and the results of the operations. Moreover, the second characteristic points to the foundations of algebra in relational logic. The third characteristic emphasizes the purely formal nature of mathematical existence, that is, consistent definition within a given axiom system.

In contrast,

⟨56⟩ And what of Greek geometry? What are its characteristics? It employs no symbols, for it is concerned not with structures formed by relations between mathematical objects, but with the objects themselves and their essential properties. It is not operational, but contemplative; its logic is the predicate logic of Aristotle’s Organon. When Plato castigates mathematicians for speaking as if their task were to do something, he places the Greek seal on geometry.

In quotation ⟨55⟩, firstly, the distinction between a merely “algebraic approach” and its “most developed form” is important; the latter, “modern algebra”, is indeed not only “a creation of the seventeenth century – AD!”, as formulated on p. 375, but at best the teleological interpretation of 17th-century algebra in the light of that “moderne Algebra” which van der Waerden had written about – 17th- to 18th-century algebra was not exactly axiomatic.[29] Secondly, there is a certain (unrecognized) convergence with Zeuthen’s distinction between “Geometry … for its own sake” and geometry “as an instrument for the theory of general magnitudes” (quotation ⟨15⟩), the latter being also, in Zeuthen’s opinion, “totally abstract and free of any ontological commitments”.[30] Neugebauer’s inclusion of geometrically oriented problems under algebra if only they seem to be based on “formal operation with magnitudes” (quotation ⟨15⟩) also comes to mind. Precisely the addition of magnitudes of different dimensions (which was Neugebauer’s main criterion for that) seemed to him as well as van der Waerden to suggest thinking free of ontological commitments.

To quotation ⟨56⟩ it should be observed that preaching against sin is evidence of the existence of sin, not of pervasive virtue; so, if anything, Plato’s reproach is proof that

[29] The “purely formal nature of mathematical existence, that is, consistent definition within a given axiom system” is not even adumbrated in any 17th-century author; it belongs to Hilbert’s times.

[30] Remember that the Euclidean plane is just as categorically distinct from the paper where diagrams are drawn as is the number 3 from a collection of 3 pebbles!

Greek geometry was not pure contemplation in his times (and, after all, four of the five postulates as well as the first three propositions of the *Elements* deal with doing something.[31] Whether even the proportion theory of *Elements* V and the classification of irrationals (and investigation of the relations between these classes[32]) in *Element*s X deal "not with structures formed by relations between mathematical objects, but with the objects themselves and their essential properties" is at least disputable (but perhaps these are not included in Mahoney's concept of "Greek geometry"?); the former was a constitutive part of Zeuthen's "geometric algebra", as we remember.[33]

Instead of exploring such questions in more depth, Mahoney has this preliminary conclusion (p. 373):

⟨57⟩ In characterizing Babylonian mathematics as algebraic, I do not want to confuse the algebra of the seventeenth century ad with that of the seventeenth century BC. A shared typology need not imply shared content or shared concepts. For if it did, then algebra would constitute a bond that links modern mathematics more closely to the Babylonians than the Greeks. If it did, then, at least in the realm of mathematical thought, the mythopoeic mind and the rational mind would not be as far apart as one has good reason to believe they are.

This is dressed as an indirect proof – but since the presumed *reductio ad absurdum* has to be argued unspecifically from "what we have good reason to believe" and from the

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31 Postulate 5 may seem not to do so. However, operating only with the potentially infinite, that is, with the *possibility to produce a line segment indefinitely,* everything in Greek geometry which *we* would spontaneously and naively explain in terms of infinite lines is inherently concerned with "doing".

Mahoney's note 16 also shows his actual knowledge of the matter to be in conflict with his apodictic appeal to Plato's testimony:

> I do not want to overstate the non-operational nature of Greek geometry. Greek geometrical analysis in particular maintained something of the operational approach and shows, in fact, many "algebraic" traits; see my "Another Look at Greek Geometrical Analysis", *Archive for History of the Exact Sciences*, **5** (1968), 318–48.

32 Evidently, Euclid does not speak of "classes" but about unspecified magnitudes possessing a specified character – for instance (X.61, trans. [Heath 1926: III, 135], cf. [Heiberg 1883: III, 186])

> The square on the first bimedial straight line applied to a rational straight line produces as breadth the second binomial.

Euclid, indeed, does not possess a fully developed and flexible language for second-order logic. For the same reason, his definition of equal ratio in *Elements* V.5 becomes ambiguous – cf. [Heath 1926: II, 120]. Attributing "classes" to Euclid is as much an imposition of our language as correcting his definition of equal ratio into "any … any" – but no more.

33 From a different angle: *If* a ratio is an object and not a relation (as *Elements* V, def. 3 says that it is), then the ratios dealt with in *Elements* VII–IX are really "disguised" or "translated" fractional numbers (as can actually be argued to be the case from the naming of such ratios, cf. [Vogel 1936]); and the ratios of *Elements* V are nothing but disguised positive real numbers, or at least their equivalents!

ethnocentric belief in the "mythopoeic mind" of the others, then it must rather be characterized as teleological logic, as a *petitio principii*. If Edward Said had cared about the historiography of mathematics, Mahoney might well have figured in his *Orientalism* [1978].

Sabetai Unguru's "On the Need to Rewrite the History of Greek Mathematics" from [1975] is very different (although it cites Mahoney as well as Szabó with approval), and at least as much convincing by emphasis and scare quotes as Szabó. An example (p. 68*f*), dealing (it must be presumed, but the formulations are conveniently hazy) among others with Tannery and Zeuthen:

⟨58⟩ As to the goal of these so-called "historical" studies, it can easily be stated in one sentence: to show how past mathematicians hid their modern ideas and procedures under the ungainly, *gauche*, and embarrassing cloak of antiquated and out-of-fashion ways of expression; in other words, the purpose of the historian of mathematics is to unravel and disentangle past mathematical texts and transcribe them into the modern language of mathematics, making them thus easily available to all those interested.

Neither Tannery nor Zeuthen have evidently said or done anything like this; I doubt Unguru would be able to find anybody who has. At least, however, he has read in the writings of Tannery and Zeuthen, which nobody else since Heath appears to have done – unfortunately without trying to understand them. So, p. 70, n. 7 he quotes some lines from Tannery's article from 1903 (cf. quotation ⟨4⟩ – square brackets are Unguru's):

⟨59⟩ Indeed, while their algebraic symbolism [sic !] developed painfully, they had already in the fourth century BCE created one for geometry, … That language presented at the same time all the advantages of the use of letters in Viète's analysis [!1, at least for powers 2 and 3.

The "sic" can safely be taken as evidence that Unguru has overlooked that Tannery, when speaking about an algebraic symbolism that develops "painfully", speaks about Diophantos and his predecessors,[34] not about geometry. It is unclear to me whether "[!]" means that it should be illegitimate to speak about "Viète's analysis", (which is after all the term Viète uses in order to avoid the filthy Arabic word "algebra"), or it is deemed illegitimate to compare *the efficiency* of one tool to another one when both investigate similar matters. None of the possibilities makes historiographic sense.

The same note contains quotation ⟨7⟩ from Zeuthen, "In this way …". There is no attempt to find out what Zeuthen means by his words – Unguru knows what algebra *is*, namely from a shorter version of quotation ⟨55⟩ taken from [Mahoney 1971b: 16]. Unguru misses that this is from Mahoney's hand a characterization of that algebraic mode of

[34] Tannery, indeed, is quite aware that there are predecessors – see [Tannery 1887: 51].

thought which emerges in the 17th century and reaches maturity with abstract group theory and perhaps category theory. None of those who have spoken about “Babylonian algebra” or Greek “Geometric algebra” would claim that any of these could be algebra *in that sense.* Unguru also *knows* that equations means algebra (but see note 7, above).

This style goes on. Heath [1926: I, 372] states that

⟨60⟩ Besides enabling us to solve geometrically these particular quadratic equations, Book II gives the geometrical proofs of a number of algebraical formulae.

Then, after listing the algebraic *equivalencies* of II.1-10 and a commentary, Heath goes on that

⟨61⟩ It is important however to bear in mind that the whole procedure of Book II is geometrical; rectangles and squares are shown in the figures, and the equality of certain combinations to other combinations is proved by these figures.

Unguru overlooks that the sequel shows that “gives the geometrical proofs of a number of algebraical formulae” does not claim that Euclid intends this, only that the propositions *can* serve in this way.[35] Instead he accuses Heath of apparently “not grasping the inconsistency involved”.

These few excerpts are characteristic of the whole paper (48 pages). Unguru makes his task easy (for example, pp. 79*f*) by inventing a representative of the stance he tries to refute, having him “grant… (though reluctantly!)” an objection proving only Unguru’s ignorance of matters Babylonian, and which a van der Waerden would never admit; and then, after having this imaginary opponent defend himself, asks “what does one answer to such an interlocutor?”. This is, on the whole, a case of allergy, not argumentation.

Four years later, Unguru published a sequel. I shall not go through it in detail – its eventual influence was quite modest, as was also that of [Unguru & Rowe 1981] (thanks to David Rowe more cautiously formulated). But the beginning of the former of the two [Unguru 1979: 555] is striking:

⟨62⟩ The history of mathematics typically has been written as if to illustrate the adage “anachronism is no vice.” Most contemporary historians of mathematics, being mathematicians by training, assume tacitly or explicitly that mathematical entities reside in the world of Platonic ideas where they wait patiently to be discovered by the genius of the working mathematician. Mathematical concepts, constructive as well as computational, are seen as eternal, unchanging, unaffected by the idiosyncratic features of the culture in which they appear, each one clearly identifiable in its various historical occurrences, since these occurrences represent different clothings of the same Platonic hypostasis.

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[35] That the diagram of II.1 *can* indeed serve to demonstrate that

$$a(b+c+d+\ldots)=ab+ac+ad+\ldots$$

will be familiar to many modern mathematics teachers.

This confirms the allergic interpretation: As soon as Unguru sees the word “algebra”, he stops reading the explanations of the writer. *He*, if anybody, is the Platonist who knows that algebra is eternal and unchanging.

For Unguru, however, this was not the end of the road (we have all been young and sometimes perhaps overly eager). Firstly, he told me around 1995 (thus years before the appearance of [Fried & Unguru 2001]), that so far (by then) the only consistent interpretation of the *Conics* was unfortunately that of Zeuthen. Even later (well after 2001, perhaps in 2011) he told that even he had to start with symbolic algebra in order to grasp Apollonios. Similarly, Szabó was very interested when I told him (in 2002) how the geometric reading of the Babylonian material led to an interpretation of “Babylonian algebra” similar to what the slave boy does (in Plato’s *Menon*) when asked by Socrates to double a square – a story that plays a major role in Szabó’s own scenario.

Objections to the objections – and what followed

Already on the final page of [Unguru 1975] there was an “Editorial Note: A defense of his views will be published by Professor van der Waerden in a succeeding issue”. It appeared as “Defence of a ‘Shocking’ Point of View” [van der Waerden 1976], the tone of which is generally as calm as that of Unguru had been violent.

Commenting upon Unguru’s use of the shorter version of quotation ⟨55⟩, van der Waerden points out (p. 199) that

> ⟨63⟩ If this definition of “algebraic thinking” is accepted, then indeed UNGURU is right in concluding that “there has never been an algebra in the pre-Christian era”, and that Babylonian algebra never existed, and that all assertions of TANNERY, ZEUTHEN, NEUGEBAUER and myself concerning “Geometric algebra” are complete nonsense.
>
> Of course, this was not our definition of algebraic thinking. When I speak of Babylonian or Greek or Arab algebra, I mean algebra in the sense of AL-KHWĀRIZMĪ, or in the sense of CARDANO’S “Ars magna”, or in the sense of our school algebra. Algebra, then, is:
>
> the art of handling algebraic expressions like $(a+b)^2$ and of solving equations like $x^2+ax=b$.

We may remember Thureau-Dangin’s reaction to quotation ⟨28⟩ (Loria).

Slightly later, we find the only sharp formulation, namely in the beginning of a section presenting “Babylonian algebra”:

> ⟨64⟩ UNGURU denies the existence of Babylonian algebra. Instead he speaks, quoting ABEL REY, of an arithmetical stage (Egyptian and Babylonian mathematics), in which the reasoning is largely that of elementary arithmetic or based on empirically paradigmatic rules derived from successful trials taken as a prototype.
>
> I have no idea on what kind of texts this statement is based. For me, this is history-writing in its worst form: quoting opinions of other authors and treating them as if they were established facts, without quoting texts.

> Let us stick to facts and quote a cuneiform text BM 13901 dealing with the solution of quadratic equations. Problem 2 of this text reads:
>
> I have subtracted the (side) of the square from the area, and 14,30 is it.
>
> The statement of the problem is completely clear: It is not necessary to translate it into modern symbolism. If we do translate it, we obtain the equation
>
> $$x^2 - x = 870.$$

Actually Unguru does not even *quote* Rey, whom he mostly treats as a fool because they mostly disagree. He gives a supporting reference to scattered pages for a summary which, as it stands, is adopted as Unguru's own point of view. The very ideas of a "Babylonian algebra" or a "Greek geometrical algebra" are a priori [Unguru 1975: 78]

⟨65⟩ historically inadmissible. There is (broadly speaking) in the historical development of mathematics an *arithmetical* stage (Egyptian and Babylonian mathematics) in which the reasoning is largely that of elementary arithmetic or based on empirically paradigmatic rules derived from successful trials taken as a prototype [first reference to two distinct pages in Rey], a *geometrical* stage, exemplified by and culminating in classical Greek mathematics, characterized by rigorous deductive reasoning presented in the form of the postulatory-deductive method, and an *algebraic* stage, the first traces of which could be found in DIOPHANTOS' *Arithmetic* and in AL-KHWARIZMI'S *Hisab al-jabr w'al muqàbalah*, but which did not reach the beginning of its full potentiality of development before the sixteenth century in Western Europe [a second reference to three places in Rey],

which in view of what was well known in 1975 about Egyptian and Babylonian mathematics could indeed have deserved sharper commentaries than those of van der Waerden.

Returning to quotation ⟨64⟩, van der Waerden's "facts" are of course based on the interpretation of Babylonian mathematics and its terminology that was current at the time; but none of the critics had ever challenged *that*; Mahoney and Szabó accepted it, perhaps with some reticence, Unguru rejected it a *priori* with his reference to Rey, whose ignorance can perhaps be excused by his date (but shouldn't a philosopher stay quiet when he does not know?).

Van der Waerden goes on with further arguments supporting his position, some of them quite interesting and innovative. One may ask why these things were not explained in *Science Awakening*, but without being asked van der Waerden gives the answer on pp. 203*f*:

⟨66⟩ We (ZEUTHEN and his followers) feel that the Greeks started with algebraic problems and translated them into geometric language. UNGURU thinks that we argued like this: We found that the theorems of EUCLID II can be translated into modem algebraic formalism, and that they are easier to understand if thus translated, and this we took as "the proof that this is what the ancient mathematician had in mind". Of course, this is nonsense. We are not so weak in logical thinking! The fact that a theorem can be translated into another notation does not prove a thing about what the author of the theorem had in mind.

> No, our line of thought was quite different. We studied the wording of the theorems and tried to reconstruct the original ideas of the author. We found it evident that these theorems did not arise out of geometrical problems. We were not able to find any interesting geometrical problem that would give rise to theorems like II 1-4. On the other hand, we found that the explanation of these theorems as arising from algebra worked well. Therefore we adopted the latter explanation.
>
> Now it turns out, to my great surprise, that what we, working mathematicians, found evident, is not evident to UNGURU.

A key phrase here is "interesting geometrical problem". Indeed, van der Waerden, in almost Wittgensteinian manner asks not "for the meaning" but "for the use". *In this respect* he is thus almost faithful to Zeuthen when enrolling him (the first to be so since Heath). As we remember, Zeuthen spoke exactly about the Euclidean propositions being *used* in the same manner as algebra is used in latter-day analytical geometry. Both knew from their experience as creative mathematicians that "mathematical entities" do *not* reside ready-made and immutable "in the world of Platonic ideas where they wait patiently to be discovered by the genius of the working mathematician" (cf. quotation ⟨62⟩).[36] Unguru instead, trained as a philosopher and a classical philologist, is an essentialist, convinced that, incorruptibly, algebra is algebra is algebra.

In [1977], Hans Freudenthal published another commentary. He starts with a double motto, Juliet Capulet's "What's in a name", and Mephisto's "Mit Worten lässt sich trefflich streiten". Accordingly, Freudenthal is much sharper than van der Waerden, starting thus (p. 189)

⟨67⟩ Whoever starts reading Greek mathematics is struck by large parts that are overtly algebraic as well as other parts where algebra seems to hide under a geometrical cover. […].

> S. UNGURU has recently challenged this view. All who have written about Greek mathematics have been wrong, he claims. On what grounds? Has he discovered sensational new facts? No, nothing! He has not even interpreted old facts in a new way. He simply says they are wrong, and does so with resounding rhetorical emphasis. If the rhetoric is disregarded, the remainder consists of large extracts from the work of others, decorated with numerous exclamation and question marks, and a few, more concise statements, which can properly be submitted to analysis.

As van der Waerden, Freudenthal asks (among other things) for a reading of the Greek mathematical texts which asks for the *use* of theorems like *Elements* II.5 and VI.28 (pp. 199*f*). Geometrically seen, they are "badly motivated" and "unattractive" in themselves. He goes on, "it appears that these propositions were used as algebraic tools within Greek

[36] Long before "mathematical practice" became a concern for philosophers, working mathematicians of course knew it from the inside even if they did not conceptualize it.

geometry"; that is, without at all mentioning Zeuthen in the article, he too is more or less back at his position. He also asks for discrimination – some renderings of a verbal text with "algebraic" symbols are faithful to it, others are misleading:

⟨68⟩ *Unguru* suggests a quite different origin for the common interpretation of II5 and VI28: people discovered that you can note down these propositions in modern algebraic language, and then concluded that they were algebra, geometrically disguised. This brings us to the question of how Greek mathematics should be edited. In fact, there are various levels on which this can be done. In a plain translation, such as by T. L. HEATH the Greek text (V11) may appear in the version

as A is to B, so let C be to D,
and, as C is to D, so let E be to F.
I say, that, as A is to B, so is E to F.

whereas in a comment or in a summary you might find

Algebraically, if $a : b = c : d$
and c : d = $e : f$
then $a : b = e : f$.

No doubt this is allowable but it would be absolutely inadmissible in the same context to replace propositions like $a : b = c : d$ by their more modern analogues $ad = bc$. It would not only spoil the context but even make nonsense of it. […] Some delicacy is needed to know in any particular case which language is most suitable. […] Anyhow, it is unwarranted to quote modern style algebraical formulas from historians of mathematics without identifying the level of presentation to which they belong and to insinuate that conclusions are drawn from wrong translations.

Freudenthal goes on with discussion of specific points. Of major interest is what he says about Unguru's claim that the Greek texts contain no equations

⟨69⟩ "Equation" has three meanings
formal identity,
conditional equality involving unknowns to be made known,
conditional equality involving variables.
Which meanings are absent in Greek mathematics? Is

$$(a+b)^2 = a^2 + 2ab + b^2$$

not an equation, if it is formulated in words? Are the "symptoms" for circle, parabola, ellipse, hyperbola not equations, just because they are written in the language of rectangles and squares? And finally what about linear and quadratic equations? Of course II5 is not the solution of a quadratic equation, but nobody ever claimed it was. VI28, however, is explicitly formulated as problem-solving, and the problem is a quadratic equation not for a number but for a magnitude,

which gives further substance to the point made above in note 7.

As he is indeed obliged to after attacking Unguru for lack of facts, Freudenthal then goes through a number of textual examples, on which he can illustrate his objections to Unguru's claims.

A final reply was formulated by André Weil in [1978]. It castigates “Z” (Weil’s alias for Unguru) for various misunderstandings and contains a number of interesting observations (and it is certainly worth reading); but it does not add anything very significant to the topic of “geometric algebra”, so I shall not discuss it.

Then, “what followed”?

Unguru’s attack had provoked van der Waerden and Freudenthal to make explicit many things that had been taken for granted by those who spoke about one or the other kind of “geometric algebra”. Without their recognizing it, they had even been induced to return to arguments similar to those of Zeuthen, which nobody had cared about for half a century or more.

Nobody listened, however. Just as Zeuthen’s phrase was taken over by others who did not care to read precisely what Zeuthen had really said, so Unguru’s attack was broadly accepted as a standard reference by a generation of historians who argued in principle for precise reading of the sources, but who did not read their own standard references in a similarly careful manner.[37]

As formulated by David Rowe [2012: 37]:

⟨70⟩ Today it would appear that most historians of mathematics have come to accept this central tenet [of Unguru]. Indeed, at the recent symposium honoring Neugebauer at New York University’s Institute for Studies of the Ancient World, Alexander Jones told me that Unguru’s position could now be regarded as the accepted orthodoxy. Sabetai Unguru, however, begs to differ; he quickly alerted me to recent work by experts on Babylonian mathematics who, in his view, continue to commit the same kinds of sins he has railed about for so long.[38]

In a similar vein but explicitly endorsing the “accepted orthodoxy” (which neither Rowe nor Jones does), Nathan Sidoli [2013: 43] sums up a survey introduction in this way

⟨71⟩ The new historiographic approach that was so hotly debated in the 1970s has become mainstream. There are now almost no serious scholars of the subject trying to determine how Greek mathematics must have originated based on what seems likely from some mathematical or logical perspective, or trying to

[37] Less kindly, Schneider [2016: viii] suggests that

> a group of present-day historians has appropriated Unguru’s request to reinterpret Greek mathematics and thereby tried to get an alibi to ignore, against all scientific probity, results that have been reached earlier.

[38] I suspect that Unguru refers to my intervention at a workshop on the “history of algebra” in which we both participated in 2011. Here I discussed (as asked for by the workshop theme) what the Old Babylonian technique had in common with later algebras (in the plural) and what not, leaving explicitly open whether this would qualify it as an “algebra”. For a Platonist it is obviously a sin to leave things open.

> understand the motivation for methods found in Apollonius or Diophantus using mathematical theories and concepts developed many centuries after these mathematicians lived.

Obviously, no "serious scholars of the subject" ever tried to do so; if anybody has argued *a priori* from a "mathematical or logical" perspective, it will have been Abel Rey – no "serious scholars of the subject" but a philosopher speaking from second-hand knowledge.[39] It has nothing to do with Zeuthen, Neugebauer, or van der Waerden (with whom one may agree or disagree) – but is seems that Sidoli continues the tradition from Neugebauer, inventing the opinions of those whom he cites, or worse, speaking of things he never read or read carefully.

So, even by otherwise good scholars, Unguru's report of the opinion of those whom he attacked have been broadly accepted on faith. Would it not be time to start reading not only the historical sources but also that part of the literature which everybody feels obliged to cite in order to demonstrate appurtenance to the "in-people" – the "superclassics" of Derek Price [1965: 149]?[40] Thus deciding whether it should stay on this privileged shelf or should be gently moved to the archives of the history of historiography?

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[39] Admittedly, as we have seen in quotation ⟨55⟩, Mahoney also explains the aims and motivations of 17th-century algebra from 20th-century views.

[40] Actually, I am not the only one to be of this opinion. After I began writing this, Viktor Blåsjö published online "In Defence of Geometrical Algebra" – now available as [Blåsjö 2016], the aim of which is not "to argue *for* geometrical algebra, but rather to argue against the arguments against it" (p. 327). This "geometric algebra" refers (1) to Zeuthen's claim that "the Greeks possessed a mode of reasoning analogous to our algebra", and (2) the idea that the Greeks "were well aware of methods for solving quadratic problems (such as those exhibited in the Babylonian tradition), and that *Elements* II and VI "contain propositions intended as a formalisation of the theoretical foundations of such methods" (p. 326).

Blåsjö's conclusion (pp. 357*f*) is that

> The geometrical algebra hypothesis has, for the past few decades, been a kind of scapegoat in a war of historiography. As the hallmark of a currently unpopular mode of scholarship, this hypothesis has been condemned with zeal by a new generation of historians. Because of its unfashionable association, the geometrical algebra hypothesis has seen objections of all sorts hurled its way. And with no one to defend it, bystanders are likely to assume that it is justified. But the geometrical algebra hypothesis deserves a fair trial. In this paper I have attempted to address every substantial argument ever raised against the geometrical algebra hypothesis. I have argued that none of them are at all compelling. I urge, therefore, that it is time to take a step back from perfunctory opposition to geometrical algebra and to look at its case afresh with an open mind.

Blåsjö has read the texts, from Zeuthen until Reviel Netz [2004] (whom I do not deal with here), showing that Netz's apparent support for his former teacher Unguru (and for Jakob Klein) actually comes down to a total destruction of their claims about cognitive incompatibility.

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Chapter 23 (Article II.6) State, "Justice", Scribal Culture and Mathematics in Ancient Mesopotamia Sarton Lecture 2008

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

J. Høyrup, *Selected Essays on Pre- and Early Modern Mathematical Practice*,
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Abstract

The functioning of the modern state presupposes a variety of mathematical technologies – accounting, statistics, and much more. Mathematics, on its part, needs the institutions of the state (schools, universities, research institutions, etc.) to secure financing, recruitment and the rearing of competence. At a given moment, the state as well as mathematics largely take the partner “as it is”, and none of them appears to the immediate view to depend for its essence on the other.

At the moment of pristine state formation, the situation was different. Most pristine state structures depended on organized violence, on religious institutions, etc., and mathematics did not enter. At least one major exception to this rule can be found, however: the earliest “proto-literate” state formation in Mesopotamia of the late fourth millennium, intimately connected to a system of accounting that seems to have guaranteed an apparent continuation of pre-state “just redistribution”. Both for its functioning and its legitimization, the state depended on the mathematics of accounting. On its part, the kind of mathematics that was created was totally bound up with its administrative role.

The lecture follows the interaction of state, “justice”, mathematics and scribal profession from the late fourth millennium over the “Ur III” period (21st century BCE, culmination and apparently end of the intertwinement of statal structure and legitimization with mathematics) until the Assyrian empire of the earlier first millennium.

Contents

State and mathematics

The functioning of the modern state presupposes a variety of mathematical technologies – accounting, statistics, and much more. Mathematics, on its part, needs the institutions of the state (schools, universities, research institutions, etc.) to secure financing, recruitment and the rearing of competence. At a given moment, the state as well as mathematics largely take the partner "as it is", and none of them appears to the immediate view to depend for its essence on the other.[1]

At the emergence of the state as a type of social organization, the situation was different. Most statal systems have originated in complex processes, either as "pristine states" via expanding chiefdoms or as "secondary states" in interaction with (often, indeed, as military protection against) existing states. As a rule, the involvement of anything that can be considered as mathematics in such processes has been peripheral, if not totally absent.

In a few exceptional cases, however, mathematical technologies have played a major role in the shaping of the state (and have, in consequence, themselves become more sophisticated in the process, developing into recognizable *mathematics*).

One instance of such an intimate bond is that between the Inca state and its accounting. I know too little about the matter to go into details – I suspect, moreover, that available evidence on the topic is insufficient to trace the connections between the development of the state and that of the *quipu* system.

Possibly, another instance is constituted by the relation (which, however, may be less pivotal) between the Maya states and their "chrono-theology"; even here I abstain from further discussion for lack of deeper knowledge. In any case, the Maya state formation was not pristine.

A third instance, perhaps the most indisputable case and at least the one which is best reflected in the sources (though still indirectly), is offered by the formation of states in southern Mesopotamia from the late fourth millennium BCE onward.

[1] Second thoughts should force us to admit that this "immediate view" may not correspond to the actual situation of the latest four decades or so: without information technology, the immense increase of administrative control of citizens (to mention but that) would never have been possible. Only by discarding computer science from what we perceive as "mathematics" can we claim that the global mathematical enterprise has not been transformed in the same process.

But this is not my topic here. I shall leave it to the reader to ponder after having read the paper whether, paradoxically, the late fourth millennium BCE offers an illuminative model of our own lifetime. That possibility is indeed one of my reasons for choosing the subject I do deal with.

Prolegomena

Before approaching the subject-matter itself, something must be said about what I mean here by "mathematics", and about the notion of a "state".

For the present purpose, the transition to "recognizable mathematics" may be characterized as

> the point where *pre-existent but previously independent* mathematical practices are coordinated through a minimum of at least intuitive understanding of formal relations.

Political anthropologists have discussed the emergence of *statal organization* of society in different terms, not necessarily as mutually exclusive as often assumed in the debate. According to Morton Fried's classic *The Evolution of Political Society* [1967: 235], the state arises as

> a collection of specialized institutions and agencies, some formal and others informal, that maintain an order of stratification,

"stratified society" being a society (*ibid.,* p. 186)

> in which members of the same sex and equivalent age status do not have equal access to the basic resources that sustain life.

This stratification may come about in several steps: in brief, from "big man" practice to chiefdom spurred by warfare, leading to a three-class division *slave owners – commoners – slaves.*

Elman Service's emphasis in the equally classic *Origins of the State and Civilization* was different, seeing [1975: 305] statal organization as the end result of a quantitative and often gradual development from

> relatively simple hierarchical-bureaucratic chiefdoms, under some unusual conditions, into much larger, more complex bureaucratic empires.

The chiefdom itself was understood by Service as a hierarchical organization legitimized by social *functions* wielded by the chief for common benefit[2] in a theocratic frame of reference, where

> economic and political functions were all overlaid or subsumed by the priestly aspects of the organization.

A number of other, less abstract discussions of the early state have been regionally focused (either explicitly or implicitly). In an article on "Population, Exchange, and Early State Formation in Southwestern Iran", Henry T. Wright and Gregory A. Johnson tried

[2] According to Service mostly functions of a redistributive nature; but if we include functions of military leadership the contrast with Fried can be seen to be far from absolute.

to base themselves "on the total organization of decision-making activities rather than on any list of criteria", describing the state [1975: 267] as

> a society with specialized administrative activities. By "administrative" we mean "control", thus including what is commonly termed "politics" under administration. In states as defined for purposes of this study, decision-making activities are differentiated or specialized in two ways. First, there is a hierarchy of control in which the highest level involves making decisions about other, lower-order decisions rather than about any particular condition or movement of material goods or people. Any society with three or more levels of decision-making hierarchy must necessarily involve such specialization because the lowest or first-order decision-making will be directly involved in productive and transfer activities and second-order decision-making will be coordinating these and correcting their material errors. However, third-order decision-making will be concerned with coordinating and correcting these corrections. Second, the effectiveness of such a hierarchy of control is facilitated by the complementary specialization of information processing activities into observing, summarizing, message-carrying, data-storing, and actual decision-making. This both enables the efficient handling of masses of information and decisions moving through a control hierarchy with three or more levels, and undercuts the independence of subordinates.

Though meant to be generally useful, the description *was* specifically geared to what happened when statal systems emerged in southern Mesopotamia and southwestern Iran, for which reason I shall adopt it here.

The West-Asian "token system"

Central to the "control" which Wright and Johnson spoke about is the "token system", an accounting system based on small and less small cones, spheres, discs, tetrahedra, rods etc. made of burnt clay – often (though at first only rarely) provided with markings that define sub-types. The system turns up in Syria and Western Iran around 8000 BCE, concomitantly with the agricultural revolution, spreading over the following millennia to a region reaching from south-eastern Anatolia and Palestine to the Iranian plateau, and remaining alive at least until the early third millennium BCE. Though some suggestions had been made in discussions of late fourth-millennium Iranian material, the discovery of the system and of its chronological and geographical range is unambiguously the merit of Denise Schmandt-Besserat. Her first publication on the topic appeared in [1977]; a complete survey of her results and interpretations is the double volume *Before Writing* from [1992].

According to their use in the fourth millennium and to continuity with proto-cuneiform writing, the various tokens served to represent quantities (presumably standard containers) of grain, oil, etc., and heads of livestock – perhaps also quantities of work.

For a number of reasons, the original social function of the system cannot have been inter-community trade (which did exist, as documented by the spread of obsidian). First of all, any use of quasi-monetary symbols without tangible value (paper money, bills of

exchange) presupposes banks and police forces that can enforce the obligations they represent. Moreover, the tokens were simply thrown out once they had been used, which excludes even a local monetary function.

Instead, the use of prestige versions (made from marble, alabaster, etc.) as grave-goods in high-status graves [Schmandt-Besserat 1988] and the presence of tokens in communal storehouse areas suggest that the tokens functioned as means of accounting in a redistribution system, and that management of this redistribution system carried very high social prestige – cf. Elman Service as quoted above.

In this connection, two observations should be made:

– Redistribution within the community is very common in pre-state societies, but redistribution built on detailed accounting is rather unique. If Inuit hunters kill a walrus and give others access to the meat, this is done from an expectation of reciprocity, and on the part of the more skilled hunters in expectation of prestige; but in neither respect is detailed accounting involved, nor possible.
– Accounting by means of tokens can doubtlessly be characterized as a *mathematical technique*. But we have no evidence for numerically standardized bundling of units (actually there is some counter-evidence from the fourth millennium, cf. below). It is therefore most likely that (e.g.) a small cone corresponded to a specific customary basket containing grain and a small sphere to some larger equally customary container, and that the ratio between the two was not numerically but physically (that is, not precisely) fixed. In other words, the mensuration inherent in the token system appears not to have been coordinated neither with the bundling levels of an oral counting system nor with any other numerical bundling principle; if this is so, the system is hardly an instance of (integrated) *mathematics* in the above sense.

Fourth-millennium developments

In the earlier fourth millennium, the city Susa in a river valley in the Zagros area in southwestern Iran became the centre of a wider settlement system; in this context the redistribution system developed into what looks most of all as payment of tribute or taxes to the central temples of Susa. The tokens were put to new use: enclosed in hollow clay envelopes ("bullae"), they appear to have served as bills of lading for goods delivered from the periphery to the centre. This goes hand in hand with the development and refinement of other bureaucratic devices and procedures – not least the use of cylinder seals as "certifiable signatures" of particular officials or offices. Since the contents of bullae could only be "read" if they were broken (after which they could no longer be controlled), impressions (or representative pictures) of the tokens to be put into them began to be made on their surfaces before they were closed and sealed.

A somewhat similar social development may have started slightly later in Uruk in the Mesopotamian South, but it soon went much further. The background was that a climatic change and lowering of the water level in the Gulf opened the possibility for

irrigation agriculture in the future Sumerian area, allowing a violent growth of agricultural output as well as population – see, e.g., [Nissen 1988: 58–61].

Probably in an initial phase, it was realized that impression or depiction of the tokens on the surface of bullae made it possible to dispense with the contents, and that the bulla itself could then be replaced by a flattened piece of clay as carrier of the impressions/depictions.[3] Very soon (c. 3200 BCE[4]), writing was also invented – *invented* indeed, in one leap or at least in a very speedy process (no "primitive" precursor steps are known).[5]

The "proto-literate" script was ideographic, and used composition in a way that is quite similar to what is found in pidgin and creole languages.[6] Most signs (traced by means of a pointed stylus) were directly pictographic, showing for instance a jar, a head, the mountains to the east, the sun rising between these, etc.[7] Some, however, depict *tokens representing the thing* instead of the thing itself. Quite striking is the sign for a sheep – enigmatic until the discovery of the token system: a circle marked by a cross. Indeed, it does not depict the animal but the token standing for the animal.

In contrast to these drawings of things or tokens, metrological and numerical units were *impressed* by a different stylus, as representations of tokens. This stylus was cylindrical, thick in one end and thin in the other. Impressed vertically it might produce a large or a small circle, oblique impression could represent a large or a small cone.

The proto-literate script did not attempt to render the sentences of spoken language – it was not "glottographic". Some 85% of the surviving texts are accounts made in fixed

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3 These "numerical tablets" provide the evidence referred to above that no arithmetically defined bundling system was yet in existence around the mid-fourth millennium BCE.

4 From this point onward, I follow the "middle chronology", as used, e.g., in [Liverani 1988]. It should be pointed out that dates, even when they can be given exactly *within* this chronology, are not fixed absolutely before the first millennium BCE.

5 Except for what will be said about the possible existence of a creole language, most of what is said in the following about early writing and accounting and their function is explained in much greater depth in [Nissen, Dammerow & Englund 1993]. In general, I draw heavily on the works of Hans Nissen, Peter Damerow and Robert Englund.

The reconstruction of underlying cognitive type and mathematical conceptions are on the whole of my own responsibility.

6 This statement should not be taken as a claim that the inventors of the "proto-literate" script spoke a pidgin – the patterns in sacred architecture show cultural continuity over about 2000 years preceding the invention, and thus continuity of the culturally hegemonic stratum of the area. But the principles of the invention *may* have been inspired by familiarity with a pidgin spoken by enslaved populations – cf. imminently.

7 In the third millennium, the drawings were no longer traced but made by oblique impression of a prismatic stylus; this gave the script its characteristic "cuneiform" character.

The early as well as the developed forms are shown in [Labat 1963].

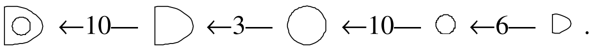
formats, rather to be likened to a statistical table or a ledger than to literary texts; what was written could of course be *spoken of* or *told* in words but it could not be *read*. The remaining 15% are "lexical lists" which served to teach the script.

Whereas writing was thus (to all we know) invented in Uruk, the idea and the bureaucratic use (not the script itself) were soon borrowed into Susa and a number of other Iranian localities which formed a shared cultural system. Until some decades ago the earliest known evidence for Egyptian writing was a century or two later than the earliest Uruk script, and contemporary with artefacts inspired or imported from Mesopotamia (e.g., cylinder seals). It therefore seemed a good guess that even the Egyptian script was inspired by knowledge of the *possibility* to write (whatever that may mean precisely); now, as the earliest beginnings of Egyptian writing has moved back a century, this is much more doubtful [Baines 2001]; independence seems more likely, but partial inspiration going either way cannot be excluded.

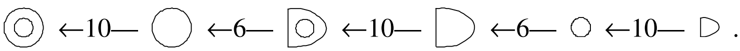
The proto-literate Uruk metrologies

In the numerical and metrological sequences of the Uruk writing system, bundling was numerically determined.[8]

One sequence was used for the measurement of grain, and may reasonably be considered a continuation of the traditional use of tokens. The "basic unit" in this system, depicting a small cone, was . 6 of these became , the picture of a small sphere. 10 small spheres were , the picture of a large sphere. 3 large spheres were bundled as , the picture of a large cone. 10 large cones, finally, became , possibly representing a punched large cone (an existing token), but perhaps a new construction made in parallel with the number sequence. In a notation due to Jöran Friberg, the sequence as a whole looks as follows



Another sequence was used for counting most types of discrete items, and may be regarded as a "number sequence". Whereas the grain sequence is likely to continue an old system in a new medium (though now with arithmetical bundling), the number sequence can be supposed to be new – the representation of pure numbers (that is, numbers abstracted from the quantity they count) by tokens will have had no purpose, at least not before their inclusion in bullae (here, the external official's seal might *in principle* determine which kind of goods was involved). The corresponding diagram is



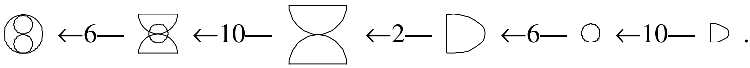
This sequence, in contrast to the preceding one, is highly systematic, and therefore almost

[8] For the following description of the metrological and numerical sequences I build on [Damerow & Englund 1987].

certainly represents a deliberate transformation of the grain sequence made so as to fit an existing oral number system, and perhaps extending it beyond existing spoken numerals. As we see, the signs for 600 ( ) and 36000 ( ) are produced by superposition of 10 ( ) on 60 ( ) and 3600 ( ), respectively, while 60 ( ) is chosen as an "enlarged" unit ( ).

The latter feature suggests that the spoken numeral system treated the step 1→10 differently than the step 10→60 (if not, there would be no reason to invert the order of and in the grain system); 60 must in some way have been understood as a "return of the unit". Evidently, the "second return" of the unit as 3600 could not repeat the visual trick, the "number-and-measure" stylus having only two ends, each of which could be impressed vertically or obliquely. In consequence, the written system gives no clues as to whether 3600 was already a unit in the spoken system.

For specific counting purposes – apparently the counting of bread or grain rations, perhaps also portions of dairy products – a particular "bi-sexagesimal system" with the following structure was in use:

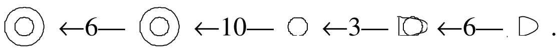


The agreement with the lower orders of the "general" counting system suggests the bisexagesimal system to have been shaped so as to fit particular bureaucratic procedures or habits. Such an adaptation recalls our counting sheets of paper in units of 500, bottles of wine in dozens, etc., sometimes but not always corresponding to standard packages – such adaptations are amply present in the later Mesopotamian record.

We might be tempted to conclude from the divergence of the two counting systems after the level of 60 that the level 3600 did not exist in the spoken number system but was a product of the new bureaucratic device; the existence of the medieval "hundred-weight" and the Germanic *Großhundert*, both deviating from the pre-existing 100 for similar reasons, shows that such a conclusion is not mandatory.

Two other metrological sequences exemplify the converse process, the adjustment of administrative procedures to mathematical structures. One is the area system, the other the administrative calendar.

The structure of the area system in itself shows little mathematical system:



Such lack of mathematical system is in itself an indication that the system is a normalization of a pre-existing system of "natural" (irrigation, seeding or similar) measures – a conclusion which is supported by linguistic arguments [Powell 1972, *passim*]. There is no direct proof of it, but it is a fair assumption that the system (which coincides with what is still known and well documented in much later periods) was already geared to the length metrology (based on the unit nindan or "rod" of c. 6 m) – not least since it is almost certain that the area of slightly irregular rectangular fields was already determined as average length times average width (the "surveyors' formula"), which would make

no point if area units were not derived from length units. (the iku of later times) would then be the square on 10 nindan, a rectangle contained by 10 and 60 nindan.[9] On this foundation we may conclude that the area metrology presents us with a deliberate coordination of several mathematical techniques and with integration of the result in the administrative procedures concerned with the allotment of land in arithmetically determined proportion (which, without this new tool, could not be made, and hardly imagined).

Alongside the true luni-solar calendar with its months of variable length and its insertion of intercalary months when such turned out to be needed (which remained in use for ritual and time-keeping purposes until the first millennium BCE), an administrative calendar was introduced, which counted each month as if it consisted of 30 days, and each year as 12 months[10]. It served for the calculation of fodder to be allocated to herds and, at least in later times, of the work which overseers were to press out of their crew each month irrespective of its length. Even in this case, only the introduction of a mathematical tool made possible the system of intense administrative control of subordinate staff.

Still other metrological sequences were in use – most of them derived from those already mentioned by means of various kinds of extra marks (similar to those that had served in the token system), and serving, for instance, to count malted instead of ordinary grain. There is no need to describe them in detail.

One common feature of all sequences which is worth mentioning is the way they were provided with subunits below . In all cases, the first level of sub-units was obtained by rotating either this sign or a shortened 90° clockwise, and , respectively – standing apparently for a halving (except when a day is seen as a sub-unit of an administrative month), for a division into 5 parts.

It is possible that *one* of these subdivisions precedes writing – could well be a depiction of a hemisphere, one of the old tokens. But , a mere rotation of , can hardly correspond to a particular token, nor can a rotation possibly correspond to any feature of the token system. Globally, the way sub-units are formed thus reflects an underlying general idea of “forming sub-units”.

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9 The definition of area units in terms of linear metrology presupposes a conceptualization of area linked to square and rectangular shapes with measured sides; that this conceptualization was at hand, however, is not subject to doubt. Firstly, a number of prestige buildings in the area from this and earlier periods exhibit clearly rectangular layout – a number of specimens are rendered in [Aurenche 1981]; secondly, the dimensions at least of certain buildings from the proto-literate period are determined in terms of an identifiable length unit – see [Beale & Carter 1983].

This modular-orthogonal architecture represents a kind of “integrated” mathematics beyond the one represented by the state-accounting complex.

10 This calendar and its use until the outgoing third millennium BCE was analyzed in depth by Robert Englund [1988].

Another general feature to be observed has to do with the function of the counting sequence. As observed above, freely movable tokens had to represent both the *kind* of thing they stood for and the quantity involved. In writing, it became possible to separate the two, combining, e.g., the ideogram for a sheep with the number «2» – which was indeed done. The mental habit involved in this splitting of quality and quantity also underlies the way the "lexical lists" were constructed from which the script was learned: in Luria's terminology [1976: 48*ff*], it reflects "categorical classification" and not "situational thinking". A plough will thus appear in a list of wooden objects, not together with the ploughman or the grain. In one list – the "profession list" – the Cartesian product is not only an external condition but also involved in the structure of the list itself, which confronts field of activity with the hierarchy of positions. Even the orderly formats of bureaucratic accounting reflects the same mental habit.[11]

The splitting into a Cartesian product of quantity and quality was not followed rigidly: quality, if determined unambiguously by context, was routinely left implicit – if a number stood for the length or width of a field, the unit n i n d a n was thus omitted. This should not be understood as an indication of "primitivity" but as an instance of economical flexibility of thought: exactly the same thing happened, for instance, when Stevin's decimal fractions came in common use, and his 375⓪7①2② was reduced to 375.72.[12]

Even this principle of economy can be seen in the light of Luria's dichotomy: Situational thinking is the habit of those whose world is largely made up of fixed situations, categorical classification is needed by those whose existence is less predictable – but in situations that *are* predictable, there is no reason they (indeed *we*) should not resort to the simpler pattern. True mental flexibility encompasses the possibility to switch when it is adequate to subordinate patterns which, *if* they were hegemonic, would not be flexible.

Uruk: A "mathematical state"

If the emergence of mathematics proper is understood as the coordination of "*pre-existent but previously independent* mathematical practices [...] through a minimum of at least intuitive understanding of formal relations", there is hence no doubt that mathematics had started its career, if not before, then at least in late preliterate or proto-

[11] According to Mogens Trolle Larsen [1987: 211], the format of the lists "points to a special logic that is additive and aggregative rather than subordinative and analytic", and whatever hierarchy occurs in the list simply reflects "the surrounding highly stratified society". In [Høyrup 1990: 337] I endorsed this view, but second thoughts now suggest to me that the hierarchy of the "profession list" is too regular to represent a spontaneous historical development: it looks like a construction inspired by categorical thought, perhaps fully implemented in real social life, perhaps in part a theoretical construction reflecting how future officials were meant to perceive social reality.

[12] This theme is further explored in [Høyrup 2000] ⟦revised as article II.4⟧.

literate Uruk (and Susa) – nor that it were primarily the needs of the administration of the new social system that asked for the creation or further unfolding of mathematics.

More interesting is perhaps the converse observation. The use of the mathematical tool was no instance of pure "technical rationality", the creation and implementation of means for an already established end which itself is not touched. If we compare the Uruk and subsequent Mesopotamian state formations with other early states, the end itself (the Mesopotamian state) can be seen to have been shaped by the means, no less than the successful appeal to military means may lead to the transformation of the state that appealed to it.[13]

A rash statement of this kind must evidently be explained. Redistributive systems are found in many pre-state societies; they correspond to the need for mutual support, and may thus be said to correspond to a notion of social justice. However, this notion of justice cannot easily be carried over to the proto-statal situation. In Robert Carneiro's words [1981: 58], "what a chief gets from redistribution proper is esteem, not power"; further on (p. 61) Carneiro observes that

> As long as a chief merely returns everything he has been handed, he gains nothing in wealth or power. Only when he begins to keep a large part of it, sharing with his retainers and supporters but not beyond that, does his power begin to augment.
>
> But the power of a chief to appropriate and retain food does not flow automatically from his right to collect and redistribute it. Villagers freely allow a chief to equalize each family's share of meat or fish or crops through redistribution because they benefit from it. But they will not willingly suffer the same chief to keep the lion's share of food for himself. Before doing this, he must acquire additional power, and that power must come from some other source.

Since power only results when redistribution proper (where the chief retains only a small percentage of what passes through his hands) is transformed into *tribute* or *taxation,* where he keeps a large part for himself and for the "core of officials, warriors, henchmen, retainers, and the like who will be personally loyal to him and through whom he can issue orders and have them obeyed" [Carneiro 1981: 61], neither the commoners nor the chief and his circle have any immediate reason to conceptualize the new situation in terms of social justice.

In the Susa-Uruk area, matters were probably perceived differently (at least by upper and middle strata), even though realities may have been similar. As shown by the use of *bullae* and by the accounting tablets, taxation and allocation of resources – be it the fields apportioned to high-ranking temple officials, be it the rations of grain distributed to workers – were made according to mathematically determined rules. In this way, statal power was structured around "just measure" and thus, apparently, legitimized by a

[13] Keeping aloof from real politics we may think of Kästner's nightmarish poem about what would have happened to Germany after WW1 "if we had won the war" [1946: 102*f*].

transformed concept of social “justice”. Since accounts and lexical lists constitute our only written sources, we have no direct evidence for how the situation was conceptualized at the time; but literary evidence from a time when lexical lists from the proto-literate period were still in use indicates that at least the higher literate stratum thought of statal power in such terms.

A striking contrast is offered by early Pharaonic Egypt, the “nearest neighbour” in terms of state formation. All evidence suggests that the Pharaonic state was legitimized by conquest, and (at least in the view of the literate) by a religious guarantee of cosmic order. Already during the First Dynasty, it is true, the yearly level of the Nile was recorded, in all probability in order to allow calculation of the taxation level of the year to come, and a biennial “counting of the riches of the land” was introduced.[14] But a biennial counting certainly does not allow any specific determination of dues and rights, nor is there any evidence that the measured Nile height served such purposes. Social “justice” has no place in the picture of early Pharaonic Egypt.

“Real justice”

“Real socialism” did not coincide too well with what had been proclaimed in programmes, and the real feudalism of the Middle Ages was conspicuously different both from Charlemagne’s blueprint and from Fulbert of Chartres’ theory of the respective roles of the praying, the warring and the labouring order. Likewise, mathematical social “justice” (however much unequal) was certainly not the whole truth about the Uruk state. But it remains an essential part of the truth, and it conditioned Mesopotamian statal structures at least until the mid-second millennium BCE.

That it was only *part* of the truth, belonging rather on the level of hegemonic ideology than on that of social realities, can be seen from the preferred motif of the seals of high officials (found on no less than half of all known early Uruk seals; two specimens are reproduced in [Nissen, Damerow & Englund 1993: 16]): A high official or priest looking on while overseers beat up pinioned prisoners. It is probable that the vehement increase in population did not result from local breeding alone but also from enslavement of significant populations from the mountain areas to the east – the pictograms for male and female slaves are indeed composed of an indication of sex (of a person) with a picture of the mountains[15] – and that this was brought about by the same climatic change as had made possible the irrigation revolution in the lowlands.

[14] Nile observations as well as countings are documented on the Palermo Stone – translated, e.g., in [Clagett 1989: 67–95].

[15] No. 50 and 558, respectively, in Labat’s sign list [1963].

Such a hypothesis is supported by linguistics: many features of Sumerian look like those of languages that over some centuries have developed from pidgins and creoles.[16] Enslaved workers are likely to have had different languages – also in later times, many languages are found in the region. Like the slaves in the West Indian plantations (who were in the same linguistic situation), they can therefore be supposed to have created a pidgin (based largely on the vocabulary of the masters’ language but losing its grammar) which the next generation transformed into a creole.[17] In the absence of a metropole conserving their original language, new generations of masters influenced as children by lower-class nurses and servants will also have adopted the creole over some generations (probably without perceiving the shift as a change of language) – the final outcome (after centuries) being Sumerian.[18]

The Early Dynastic and Sargonic periods

The proto-literate period may have lasted from c. 3200 BCE to c. 2900 BCE (falling in two distinct sub-periods, “Uruk IV”, 3200–3000, and “Uruk III” or “Jemdet Nasr”, 3000–2900). It was followed in the Sumerian area (now beyond doubt Sumerian) by the “Early Dynastic Phase” (subdivided into ED I, ED II and ED III), c. 2900–2750–2600–2350 BCE.

In this phase, what appears to have been a social system with one major centre (Uruk) changed (collapsed?) into one consisting of competing city states; and what looks like a state centred around a staff of high temple officials developed into states rules by a king (though still heavily influenced by the temple institution).

From ED I we have no written sources, and from ED II very few. In ED III, their number proliferates; the continued use of the old lexical lists demonstrates continuity not only of the writing system but also of the school tradition. In the 26th century, however, a new phenomenon can be observed. Writing was now in wider use, serving also, e.g.,

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16 This theme is explored in depth in [Høyrup 1992].

17 For this process, see for instance [Mühlhäusler 1986] or [Romaine 1988].

18 Since the proto-literate script was ideographic and indicated neither grammar not the word order of full sentences, we have no means to identify the language spoken by its inventors. One or two cases of possible use of homophones corresponding to later Sumerian (“rebus principle” writing) decide nothing, since a pidgin and the creole it engenders borrow most of their vocabulary from the language of the masters – cf. the derivation of the language name *Tok pisin* from (the pronunciation of) *talk pidgin.*

⟦A personal note: for eight months around my three-years birthday, I was in Hongkong with my parents. My mother certainly believed she took care of me, but she also had a Chinese *amah.* From the things I said around our return which have been quoted to me, I know that c. 80% of my vocabulary was English (in Cantonese pronunciation), only 15% in my Danish mother (and mother's) tongue.⟧

for the stipulation of private contracts; at the same time, and in consequence, the circle of the literate became broader; in John Baines' terms [1988], a transition from "very restricted" to "restricted literacy" took place. The group of scribes (dub.sar) turns up for the first time as a distinct *profession* in the city-state Shuruppak [Visicato 2000: 4, 12–23].

Also at the same time, and in all probability as a further consequence of this, the script was put to new uses. We find the first literary texts – a proverb collection and a hymn – and the first instances of "supra-utilitarian" mathematical school problems (problems that are not directly connected to practice even though they are formulated as if they were). In contrast, all mathematical texts from the proto-literate period that can be identified as school texts are "model documents", distinguishable from real administrative texts only by the absence of an office seal and by the occurrence of numbers that are suspiciously round or nice and at times suspiciously large.

Literary texts as well as supra-utilitarian mathematics were probably meant to probe and make manifest the reach of the two professional tools – writing and computation – and thus as expressions of professional pride. This agrees well with the appearance of many of the so-called "schooltexts" from Shuruppak (edition in [Deimel 1923]): empty corners may be filled out by nice drawings, and according to the judgement of Aage Westenholz (personal communication) the tablets may indeed be *de luxe* versions made for mature scribes looking back at the real or imagined pleasure of their school time, emblem of their present professional identity and social position.[19]

Rising city walls show clearly that warfare was an endemic condition of the ED-period, and that the king was a military leader; Shuruppak itself was completely devastated in a military attack, following upon a general mobilization [Visicato 1995: 144*f*]. The many killed servants that followed their master to the underworld in the Royal Cemetery of Ur (initial ED III) also demonstrate that the king had given up any idea of being the servant of society – he was its overlord, and society a means for his greatness. None the less, only the very end of the ED period gives us written evidence, if not of the ritual slaughter of servants then at least of military activities; until then, even royal inscriptions show the king solely as the benefactor of temples and provider of agricultural prosperity (in strong contrast to early Pharaonic documents). Literacy, so it appears, only reflects the functional and pseudo-just characteristics of the state; those features of the state which had been irrelevant for the invention of writing and bookkeeping remained outside the perspective of writing. In this respect, ED Sumer was a *dual society*, one of whose faces was still "mathematical".

From c. 2350 to c. 2200 BCE, the "Old Akkadian period", the Sumerian area (and soon the whole of Mesopotamia and even more) was united into a single territorial state;

[19] Cf. Giuseppe Visicato's work on third-millennium scribes [Visicato 1995].

after an initial short-lived centralization around a Sumerian city-king, the centre was the Akkadian “Sargonic” state[20]< (Akkadian is a Semitic language, of which the later Babylonian and Assyrian languages are dialects, Sargon the founder of the dynasty; the school language remained Sumerian).

“Literature”, at first apparently a free creation of the scribe school and a means for scribes to probe and demonstrate their professional identity, was soon taken over by the Sargonic rulers as propaganda (hymns being written so as to serve the new dynasty [Hallo 1976: 186]). While mathematical administration certainly expanded [Foster 1982], the utilization of supra-utilitarian problems in mathematics teaching was continued; there is no reason to presume that they fulfilled, or could fulfil, any role outside the school.

Already during the ED phase (documented in ED III) but accelerating during the Sargonic period, metrologies were adjusted with concern for mathematical regularity as well as administrative convenience. The former concern (mathematical regularity) is especially visible in the weight system, apparently a fresh development of the ED phase, where the step factor 60 was given a prominent position (only one factor had to be 3×60 in order to accommodate the “natural” measure of a barleycorn). But other metrologies too were extended upwards and downwards with this step factor.

The concern for administrative convenience, at times but not always in conflict with the former, asked for the adaptation to administrative procedures or technical practice, for instance in the definition of a Sargonic “royal” g u r (“tun” – the largest capacity unit) and in the creation of particular brick metrologies geared to the various standard bricks; cf. [Visicato 2000: 5] and [Powell 1976].

All in all, the relation between the state and its mathematics seems to have developed during the later ED and the Sargonic period along lines known from other societies provided with an accounting or otherwise mathematically organized administration: mathematics was taught in a way which was needed by future staff, but it was also allowed a certain autonomy in the school. It was certainly not taught by “mathematicians” – but even when teachers are supposed to teach for practice, teaching will normally be affected by the fact that the practice which teachers are *really* familiar with is the practice of teaching. Thus also here, according to the meagre evidence at our disposition.

The Janus-faced innovations in metrology correspond to this tension in the situation of mathematics: sexagesimalization is likely to have been driven by a preference for intra-mathematical coherence, the other innovations by the links to extra-mathematical practice, in particular to the administrative procedures of the state.

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[20] Given the travelling times and the plurality of languages it is even justified to speak of an “empire”, as indeed often done.

The Neo-Sumerian state

Around 2200, the Akkadian territorial state or "empire" lost most – in the end all – of its territory, and smaller states reemerged, of which only Gudea's Lagash (2141–2122 BCE) has left sources that might be considered relevant for our topic – inscriptions telling in meticulous accounting what he has given to the temple, and how he laid out the geometric plan for sacred buildings (texts with translation in [Edzard 1997: 69–101], see in particular pp. 72–82). From 2112 BCE onward, however, the Third Dynasty of Ur established a new "Neo-Sumerian" territorial state or empire, mostly referred to as "Ur III".

The early decades of this dynasty present us with nothing spectacular. In 2074 BCE, however, king Šulgi undertook a military reform, which was immediately followed by an administrative reform. From this point onward and until the collapse of the empire, scores and scores of thousands of accounting tablets inform us about the details of the administration (and, indirectly, about its governing principles).

At least in the Sumerian South, the larger part if not the overwhelming majority of the working population in both agriculture and handicraft production seems to have been submitted to conditions close to those of slavery,[21] working in crews under scribal overseers who were responsible for the work performed, reckoned in units corresponding to $^1/_{60}$ of a working day (i.e., 12 minutes).

The accounts of the overseers are extremely meticulous, converting all outputs into a common unit,[22] taking illness, death and absence as well as workers lent to or borrowed from other overseers into account. The old administrative calendar was still in use – Ur III is the epoch in which sources show that the overseer scribes were to press out of their crew 30 days' work each month irrespective of whether its actual length was 29 or 30 days. As shown by Robert Englund [1990: 46*f* and *passim*], the yearly deficits of an overseer scribe were accumulated, and at his death the family was held responsible for it (if needed by being drawn into the enslaved crews) – at least in private discussion, Englund would speak of the system as a *Kapo* economy.

For use in this immensely expanded accounting, two decisive mathematical innovations appear to have been introduced.

One is the accounting system itself, with built-in automatic controls (in this respect an analogue of what was brought about in the later Middle Ages by the introduction of

21 A survey of the debate about how to interpret the sources on this account is given by Robert Englund [1990: 63–68]. The system appears to have been established during what was originally a state of emergency declared at the same occasion as the military reform and which was soon made permanent (*ibid.* p. 57).

22 Often weight of silver, but barley was another possibility – see [Englund 1990: 18–20].

double-entry bookkeeping). This was taken over in the subsequent "Old Babylonian" period, during which it was also used for private large-scale accounting – after which it was forgotten.

The other was the sexagesimal place-value system. This was a floating-point system, serving equally well for integers and for fractions. It was used for intermediate calculations, of which relatively few traces remain; in mathematical school texts, where orders of magnitude could be presupposed, could be remembered, or were immaterial; and in the late astronomical tables, where the tabular format helped to determine orders of magnitude.

Neither school texts nor astronomical tables can have been the original purpose for which the system was introduced – the latter already for chronological reasons. Nor did it ease additive and subtractive computations (which anyhow appear to have been performed on some abacus-like device, see [Høyrup 2002b] ⟦= article I.2⟧ and [Proust 2000]). What it did facilitate was multiplication and division – but only if multiplication tables and tables of reciprocal numbers were available or learned by heart, along with tables permitting the translation of metrological units into sexagesimal multiples of a standard unit.[23] The production and teaching of such tables, on the other hand, had no point before the place-value system was in use.

This observation leads to a striking conclusion: The important step was not the *invention* of the new notation – which, by the way, was in the air since centuries, as shown by Marvin Powell [1976], and may even have been invented well before Ur III without leaving any traces in tablets that happen to have survived and to have been read by Assyriologists. What was decisive will have been a *political decision to implement it* – a decision which could only be effectual in a centralized system like Ur III.

We have no direct evidence for the taking of such a decision nor for where it was taken;[24] but we may safely assume that the planning was made in a scribe school

23 I borrow the following explanatory example from [Høyrup 2002a: 18], adapting it slightly:

If a platform had to be built to a certain height and covered by bricks and bitumen, a "metrological table" could be used to transform the different units of length into sexagesimal multiples of the nindan and kùš ("cubit", $^1/_{12}$ of a nindan), allowing the determination of the surface and the volume in the basic units sar [square nindan] and [volume] sar [an area sar provided with a height of one kùš]. A list of "constant coefficients" (igi.gub) would give the amount of earth carried by a worker in a day over a particular distance, the number of bricks to an area or volume unit, and the volume of bitumen needed per area unit – all expressed in basic units (if no transformation into basic units had taken place, different coefficients for the bitumen would have had to be used for small platforms whose dimensions were measured in kùš and for large ones measured in nindan). With these values at hand the number of bricks and the amount of bitumen as well as the number of man-days required for the construction could be found by means of sexagesimal multiplications and divisions – once again facilitated by recourse to tables, this time tables of multiplication and of reciprocal values. Finally, renewed use of metrological tables would allow the calculator to translate the results of the calculations into the units used in technical practice.

24 Until recently, direct evidence for use of the notation during Ur III was itself extremely scarce

environment that was closely connected to the royal administration. Similarly, F. R. Kraus [1973: 24–27] concludes that official year names, royal inscriptions and royal hymns were produced in the subsequent Old Babylonian period (see presently) in an institution which at one and the same time served as "palace school" and as "court chancery", and that this institution went back to some similar Ur III institution.

That king Šulgi himself (or at least those who produced propaganda in his name) saw the school as an essential tool for his project is obvious from one of the so-called Šulgi hymns,[25] according to which the king was taught from an early age in the "tablet-house", learning the art of writing together with addition, subtraction, counting and accounting under the protection of the scribal goddess Nisaba; later we hear that his praise is sung in the same tablet house.

Considering the marvellous feats of which Šulgi boasts elsewhere in this and other hymns we may wonder at the level of his mathematical curriculum, far below the actual level of mathematical competence of which the texts of the Old Babylonian age bear witness – even multiplication goes unmentioned, at most it may perhaps be presupposed as an auxiliary technique in accounting (but why then mention addition?). Actually, however, this fits what can be derived from the absence of all mathematical school texts apart from model documents, in particular when viewed in the light of evidence offered by the terminology of the Old Babylonian period. It appears that *problems*, well represented in the (meagre) corpus of mathematical texts surviving from ED III and the Sargonic period, were banished from the Ur III school: it looks as if even the modicum of independent thought needed when students have to find and not just follow a prescribed way was considered a threat to their docility.[26]

If any ruler ever *was* the state, the deified Šulgi was. The various Šulgi hymns and the prologue of the law-code he produced[27] are therefore informative about the official ideology of the state. Šulgi is not only a potent military leader and pitiless avenger of wrongs (which, conveniently, permits him to provide slaves) but also a "good shepherd" and exceedingly just (dual society, passed away in late ED III, had not been resurrected). However, only one feature of his "social justice"[28] goes beyond verbatim repetition

(and not fully compelling), in particular because of the uncertainty of palaeographic dating of tablets containing only numbers (that is, of mathematical tables and scratch pads for computation). A few years ago, however, Eleanor Robson [personal communication] discovered tables of reciprocals found in dated contexts.

25 Hymn B, l. 13–19 [ed. Castellino 1972: 31*f*]. Castellino's translation and commentary miss the mathematical points.

26 The full argument for this in unfolded in [Høyrup 2002c] ⟦= article II.7⟧.

27 At first ascribed by Assyriologists to his father Ur-Nammu and hence known as the Ur-Nammu laws. The law-code is published with translation in [Roth 1995: 14–21], hymns B and C in [Castellino 1972], hymns A, D and X in [Klein 1981].

28 Social justice should be distinguished from "judicial justice", punishing enforcement of the laws

of the trite commonplaces of the preceding centuries (protection of orphans from wealthy and widows from mighty men), and only one thus rings true: metrological reform.

All in all, Ur III enhances features which already appeared to characterize proto-literate Uruk: the management of the state was meticulously planned and controlled. This meticulous planning and control had several effects:

- In mathematics, important innovations were introduced – one of them still important for us, given that the sexagesimal place-value system may possibly have provided part of the inspiration for the Indian invention of the decimal place-value system and was certainly the direct inspiration for the introduction of decimal fractions. Free supra-utilitarian developments, on the other hand, appear to have been blocked.
- Socially and ideologically, the fact that the extremely oppressive policies of the system were metered out according to mathematical rules permitted that these policies could be seen by those in power – and probably even by the overseer scribes – as embodiments of *justice.*

The undernourished workers, however, fell ill or ran away the best they could – even this can be read from the accounting texts; after all, they had not been brought up in the scribal school and may have had other opinions about social justice if at all caring about such questions.[29] This is likely to be one of the reasons that the Ur III state did not outlast the third millennium. All in all, this early instance of immoderate Taylorism seems to have provoked a reaction similar to what British trade union activist of the twentieth century CE responded to the “scientific management” of their own days: “time and motion studies means that motion stops and time is wasted”.

The Old Babylonian period and the culmination of Mesopotamian mathematics

Already around 2025, the periphery rebelled, and the Ur III state lost its character of an empire. A few decades later, even the centre dissolved into small states. Gradually, some of these absorbed the others, and in the 18th century BCE Hammurabi of Babylon managed to subdue the whole Mesopotamian south and centre. From then on, this region can be spoken of as “Babylonia”. The centuries from 2000 BCE to 1600 BCE are known

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which follow after the prologue.

[29] It appears that some of them did. An Old Babylonian epic which seems to reflect Ur III experience and not Old Babylonian conditions (*Atra-ḫasīs,* ed., trans. [Lambert & Millard 1969]) transposes a strike into the realm of the gods. After the creation of the world, An takes possession of the heavens, Enlil of the earth, and Enkidu of the waters below the earth – and the minor gods are put to work, digging Euphrates and Tigris. After toiling for forty years they revolt, set fire to their spades and prepare an attack on Enlil's abode. So much in the account reflects the psychology of real wildcat strikes (Enlil asking who is the instigator, the mutinees answering that everyone is the instigator) that we may safely assume that the story builds on historical experience.

as the "Old Babylonian period"; it produced the most sophisticated mathematics we find in ancient Mesopotamia.

This culmination arrived when the mathematical Taylorism of Ur III had disappeared. The period is characterized by individualism, both in the economic structure (even though it would be a mistake to speak of a general market economy) and on the level of ideology or culture [Klengel 1977]. Land, even when owned by the Crown, was often rented under contract. Private correspondence turns up (a large number of letters are published with translation in [Kraus 1964]). The letters were often written by free lance scribes (a category we do not know from Ur III). The Ur III accounting system was now used in private business, handled by privately employed scribes. The seal, so far a symbol of office, now belonged to the individual. We may speak of the rise of an ideology of *personal identity.*

This ideology also affected scribal culture, in a way that is reflected in the texts used in school to inculcate understanding of what should characterize a *real* scribe – so-called "examination texts", cf. [Sjöberg 1971; 1975; 1976].

The Sumerian language was dead by now, and Babylonian could be written adequately with a phonetic syllabary of 70 signs or less; a *true* scribe, however, would also use a large number of word signs, borrowed from the Sumerian script but now meant to be pronounced in Babylonian. This, however, was not a sufficient demonstration that the scribe was somebody special. He should also be able to read, write *and speak* Sumerian – a feat only other scribes would be able to appreciate.[30] He should know everything about bilingual texts, he should be familiar with all the significations of the cuneiform signs (each single sign would have one or several phonetic and one or several logographic meanings – to which comes further occult meanings which we do not understand). He should know about music, and about mathematics. He whole complex was called "humanism" (true! – namely nam-lú-ulu, Sumerian for "the condition of being human"). Quite adequately, lú corresponding to Latin *vir,* another literal but still adequate translation would be "virtuosity".

The texts from which we know this do not specify *which* kind of mathematics would count as "humanist". Training tablets which carry a Sumerian proverb on the obverse often have quite simple calculations on the reverse. Elementary mathematics was thus taught at a rather advanced level, and most scribes presumably never went further. On the other hand, however, very sophisticated supra-utilitarian mathematics was also produced, and it is a fair guess that this (as useless as spoken Sumerian) was the really "humanist" level of mathematics.

[30] A real feat: Sumerian and Babylonian are as different as, for instance, Basque and Spanish. Quite apart from the vocabulary, the scribes should thus understand a grammar based on principles totally different from those of their mother tongue. Without the lexical lists and explanations prepared by the Babylonian masters for their students, 19th-century scholars would have been unable to decipher the Sumerian texts.

What we find together with Sumerian proverbs are simple numerical multiplications, area determinations and such things. Before that, future scribes copied metrological tables and tables of reciprocals and multiplication – probably so often that they learned them by heart. All of this was useful training for future professional practice, and hence not supra-utilitarian.

At the sophisticated, supra-utilitarian level we still find numerical problems – for instance, an intricate technique for finding reciprocals of numbers not listed in the standard table nor easily derived from it by successive halving and doubling. The favourite genre, however, was what has been interpreted as "algebra" of the second (at times the third) degree. Nominally, these "algebraic" problems deal with areas of rectangles or volumes of excavations and their sides, at times combined for instance with the wage to be paid for the excavation; the substance of the problems, however, is entirely artificial, and the "algebraic" technique that is taught is completely useless for professional practice. This "algebra" is thus truly supra-utilitarian.[31]

Its inspiration had probably come from a riddle tradition carried by "lay", that is, non-scribal (whence fully or almost illiterate), Akkadian-speaking surveyors [Høyrup 2001 ⟦=article I.3⟧]. These riddles (as they can be reconstructed from consideration also of their appearance in much later surveying texts) were of this kind:

> "I have added together the side of a square and its area, and the outcome was 110".
>
> "I have added together the four sides of a square and its area, and the outcome was 140".
>
> "I have added together the length and the width of a rectangle, and the outcome was 14, while its area is 48".
>
> "the diagonal of a rectangle is 10, and its area is 48".
>
> "I have added together the perimeter, the diameter, and the area of a circle, and the outcome was 115".

Others probably concerned differences between square area and one or all four sides, the sum of or difference between areas and sides of two squares. The total number of the riddles will not have exceeded ten to fifteen.

As mentioned above, mathematical *problems*, and *a fortiori* supra-utilitarian problems, appear to have been totally absent from the Ur III school. As the Old Babylonian scribe school developed, its "humanist" pretensions appear to have induced it to adopt these riddles; in the context of the school, however, a handful of standard riddles could not do: the riddles became the starting point for a genuine mathematical discipline, with rich variation and exploration of the possibilities offered by the technique – for instance, letting

[31] Not only the terminology but also the technique of the "algebra" in question is geometric – see [Høyrup 2002a].

the sides of a rectangle represent a number or a price, or even a square area or the volume of a cube (the latter in a problem of the eighth degree, resolvable as a bi-biquadratic). Rich variation had the added advantage of allowing copious training of sexagesimal arithmetic.

Beyond their "humanism", scribes were (supposed to be) proud, if not of being leading officials of the state (few of them of course were), then of belonging to a group from which leading officials came. This state was still supposed to represent social justice, and serving it could hence be a reason for pride. That can be seen in one of the texts used to form the self-image of scribes in the Old Babylonian school, known as "Lipit-štar, King of Justice, Wisdom and learning".[32] The king was taught the scribal art by Nisaba, the goddess of scribal wisdom – consisting, the text reveals, in writing and use of "the measuring rod, the gleaming surveyor's line", and she bestowed upon him "the cubit ruler which gives wisdom". The praise goes on

> […] you are Enlil's son;
> Truth and justice you make manifest;
> Lord, your goodness covers even the horizon.
> King Lipit-eštar, councellor of great judgment,
> (Whose) word never falters, wise one (whose) decision provides justice for the people;
> Great mind, knowing all things deeply,
> In order to lay down the law for all foreign countries [...
> ...] you rage against the enemies,
> From evil and oppression you know how to save people
> From sin and destruction you know how to free them.
> The mighty do not perpetrate robbery,
> And the strong do not make the weaker ones into hirelings –
> Thus you established justice in Sumer and Akkad.

The mathematical scribal arts and justice are neighbours, as we see, but the only link beyond this vicinity is indirect, the common reference to generic wisdom.

One step further, the statal social justice of which Hammurabi proclaims himself the supreme protector in the introduction of his famous "law-code"[33] is not mathematical at all but a continuation of commonplaces going back to the outgoing Early Dynastic epoch (Hammurabi is still the protector of orphans and widows); beyond that his justice is judicial (some of his legal decisions, however, concern metrology and punish metrological fraud). One of his successors also issued a decree "re-establishing justice to the country", prescribing a debt cancellation [Bottéro 1961; Edzard 1974: 151–153], reminiscent of the Old Testament jubile (Leviticus 25:11–15) but apparently a once-only measure meant

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[32] Lipit-eštar had been king of Isin, one of the smaller states emerging from the collapse of Ur III. The text was published with translation by H. L. J. Vanstiphout [1978].

[33] Text in [Roth 1995: 76–140]. Actually, the text presents itself not as a law-code but as Hammurabi's (presumable paradigmatic) judicial verdicts, cf. [Renger 1975: 228*f*].

to palliate the threat to general economical stability resulting from a debt crisis and crushing interest rates – in any case a cancellation of the very idea of that "mathematical justice" where everyone receives and contributes his exactly calculated due (indeed the kind of "justice" which had led to the crisis).

Accounting, as mentioned, was still around, but even when done for the state its role was that of a subservient tool. The relation between the state and mathematics had become accidental, not constitutive for either part. Mathematical "humanism" should probably be understood as an *alternative* legitimation rather than as a continuation of the ancient pattern.

Disappearance of a pattern

The final dissolution of the pattern state—social justice—accounting mathematics arrived with the collapse of the Old Babylonian state. After a Hittite conquest of Babylon and ensuing social chaos, power was taken by the Kassite tribes, already present in Babylonia as mercenary soldiers. The ratio between town and countryside dwellers fell to fifth-millennium levels, and the role of scribal administration and culture – always the carriers of ideas of the just state – was not only strongly reduced but also appears (to the extent the extremely meagre written evidence from the period allows us to distinguish) to have lost its ideological hegemony. As writing once again became copious in the late second and the first millennium with the expansion of the Assyrian city-state into a territorial state and finally an empire, we find scribes in central positions at court or somehow working for the court – but now as producers of an ideology emphasizing the king and the empire as creators and upholders of order [Liverani 1979; Garelli 1979; Reade 1979; Parpola 1999], and as omen priests and astrologers protecting the king [Reade 1979; Koch-Westenholz 1995];[34] the huge libraries of the Assyrian royal palaces are also evidence of the activity of learned court librarians and copyists. These scribes working for or corresponding with the court were certainly also proud of their professional status, but even those of them who may have worked on incipient mathematical astronomy identified themselves as "writers" of omen series, exorcists etc.; mathematics was

[34] The letters from these scholar-scribes are collected in [Parpola 1970]. Apart from giving technical advice, they mostly wish the king good health and vigour. One exorcist needing to flatter Assurbanipal – who, in contrast to his predecessors, was pleased to take up themes from earlier epochs – praises him for having brought prosperity to the land (Assurbanipal does boasts of that himself, as Hammurabi had done 1100 years before) and for distributing particular favours; the exorcist also states (*ibid.* p. 91) that "the King, my lord, has revived the one who was guilty (and) condemned to death; you have released the one who was imprisoned for many years" (metaphorically, no actual event is meant). Even when trying to appear in the light of age-old traditions, the Assyrian king could only taint Iron-Age despotism with commonplaces of (mainly judicial) justice in Old Babylonian style.

peripheral to their professional self-esteem.[35] Ordinary daily administration was probably taken care of in Aramaic alphabetic writing, and not in cuneiform on clay tablets, for which reason the evidence has disappeared together with traces of the clerks who took care of it.[36]

* * *

To sum up: During the late fourth and the third millennium, "writing" was in power; but "writing" in this respect was first of all accounting and management of resources, somehow connected to the pre-historic redistributive structures. However, during ED III we find the first evidence of literary writing and supra-utilitarian mathematics as evidence of professional self-esteem of scribes, and soon afterwards the use of literature as state propaganda.

During the Old Babylonian period, the role of professional self-esteem becomes much more conspicuous in scribal culture; concomitantly, the legitimization of the state, though still referring to "justice", is decoupled from accounting.

After the Kassite interlude (the "Babylonian Middle Ages"), "justice" however meant does not characterize the role of the state; activities of importance for professional self-esteem of cuneiform scribes were predominantly literary, divinatory and theurgical.

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[35] This is no less true in the Seleucid era (third and second century BCE), the epoch where mathematical astronomy attained maturity.

[36] Contracts on clay tablets are revealing in this respect. Belonging to a legal genre, they were mostly written in cuneiform Assyrian; but often they carried a resume of some lines in Aramaic – see the specimens in [Fales 1986].

Some of the contracts, though legal stuff, are in Aramaic, and carry no resume in Assyrian. Contracts on parchment or papyrus, if they existed, will also have been in Aramaic only, the support not being suitable for cuneiform.

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Chapter 24 (Article II.7) How to Educate a Kapo: Reflections on the Absence of a Culture of Mathematical Problems in Ur III

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Small corrections of style made tacitly
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Translations, if not otherwise identified, are mine

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Abstract

The article presents in detail the mathematical terminology of Old Babylonian procedure texts, those for mathematical operations as well as those belonging to the metalanguage needed to formulate *problems* and structure the prescriptions. In particularly it investigates which terms are properly Sumerian or, if Akkadian, loanwords; which may be written in syllabic Akkadian or by means of logograms (though within a framework of Akkadian syntax), according to the general stylistic ideal of the text; and which as a rule are necessarily expressed in syllabic Akkadian. The general outcome offers strong evidence that the culture of mathematical *problems* (well presented in the material known from earlier epochs) had been eliminated from the mathematics education of scribes during the Ur-III epoch (21st century BCE), confirming the picture of Ur-III society as extremely oppressive.

Contents

TO BOB (ENGLUND) and HANS (NEUMANN)

Personal prehistories

Firstly:

Some fifteen years ago, when Robert Englund had recently changed his dissertation theme from “Ur III-Fischerei” to “Organisation und Verwaltung der Ur III-Fischerei”, he told me (halfway jestingly) to be alarmed, discovering himself to have become a Stalinist: namely because analysis of the sources obliged him to conclude that Ur III was a slave society. This agreed better than he liked with the orthodox Stalinist version of historical materialism, according to which “slave society” follows after “primitive communism”, without any intervening “Asiatic mode of production”.

I suggested he should not worry too much. In the present case, the orthodoxy had little to do with the contents or style of Stalinist policies, apart from its being *made* an orthodoxy. Stalin had just happened to listen to the best authority on the question he could find: V. V. Struve, who, when becoming curator of the Hermitage cuneiform collection, had started reading Ur III accounts and had been led to the same conclusion as later Englund.[1]

Further on, Englund sharpened his views considerably: in view of the strict regime to which not only the workers but also the overseer-scribes were submitted he would now speak of a “*Kapo* economy” – a *Kapo* being (in case anybody should not have heard about the system) the KZ prisoner responsible for the work or organization of a group of fellow prisoners, in constant danger of being reduced to the status of an ordinary prisoner as soon as he did not fulfil his tasks in a way that contented the SS. And, as formulated in the concluding words of the published version of the dissertation [1990: 316], the understanding of working conditions conveyed by the administrative texts

> kann vielleicht helfen, sich in den historischen Darstellungen des 3. Jahrtausends v. Chr. die Kosten der babylonischen Paläste und Statuen plastischer vorzustellen. ⟦[2]⟧

In a volume published in the same year[3] we find a more detailed formulation in a commentary to the accumulated deficit in the yearly balance of an overseer-scribe:

> Aus anderen Texten wissen wir, welche ernsten Konsequenzen solch eine lückenlose Überwachung der anwachsenden Fehlbeträge für den Aufseher und seinen Haushalt mit

[1] See [Diakonoff 1969: 5].

[2] ⟦may perhaps help to imagine with more plasticity in historical descriptions of the third millennium the costs of the Babylonian palaces and statues.⟧

[3] [Nissen, Damerow & Englund 1990: 89, cf. *id.* 1993: 54]] – this chapter signed by Peter Damerow and Robert Englund.

> sich brachte. Die Fehlbeträge mußten offenbar um jeden Preis beglichen werden. Verstarb ein Aufseher, so wurde sein Nachlaß herangezogen, die Schuld zu Tilgen. Das bedeutete in der Regel, daß die verbleibenden Haushaltsmitglieder selbst in die staatlichen Arbeitertrupps eingegliedert wurden, die die von den Aufsehern überwachten Arbeiten zu verrichten hatten.
>
> Das waren die Arbeitsbedingungen der Aufseher, über die in den Verwaltungstexten allein Buch geführt wurde. Über das Schicksal der Arbeiterinnen und Arbeiter sind dagegen kaum Informationen überliefert. Sogenannte "Musterungstexte" berichten regelmäßig über in großer Zahl entflohene Arbeiter. Man kann sich angesichts der totalen Überwachung aller Leistungen der Trupps, in die sie eingegliedert waren, die Gründe leicht ausmalen. ⟦[4]⟧

Secondly:

Already before I started my conceptual analysis of the Old Babylonian "algebraic" texts in 1982 I suspected these to represent a *new* genre, irrespective of current opinions. In [1980: 20–22] I had thus written that[5]

> the influence of the school on Sumerian mathematics was (as far as we know it from published material[6]) restricted to the *systematization of applied mathematics*. […] Sumerian mathematical texts are concerned with *real* "real-world-problems"; this does *not* imply that they are always realistic: One school text from the Sargonic epoch[7] deals with a field as long as 1297.444 km (given to that precision […]). […] In historical retrospect, this characteristic is typical of the teaching of even practically oriented

[4] ⟦From other texts we know what drastic consequences such continuous control of deficits meant for the foreman and his household. Apparently, the debts had to be settled at all costs. The death of a foreman in debt resulted in the confiscation of his possessions as compensation for the state. One consequence of such a confiscation was that the remaining members of the household could be transferred into the royal labor force and required to perform the work formerly supervised by the deceased foreman.

Such were the working conditions of the foremen, solely accounted for in the administrative texts. So-called inspection texts regularly report on large numbers of escaped laborers. In view of the total control the laborers were subjected to, it is not difficult to imagine why they tried to flee.

Translation [Nissen, Damerow & Englund 1993: 54]⟧

[5] I change the numbering of notes and the format for the references but leave my original text intact (though truncated, and with omission of a number of notes) in other respects.

Evidently, I would now formulate much of what is said in different terms (not least avoiding the notion of an Old Babylonian "pure mathematics"). To the extent one can distinguish substance from formulation, I believe most substantial points remain valid.

[6] […] comparison with the distribution of literary texts seems to indicate that Sumerian precursors of later Babylonian pure mathematics are really non-existent: Indeed, even if most literary texts are known only from later versions although they have Sumerian or Sargonic origins, a reasonable number of literary tablets are known illustrating the tradition all the way back to 2500 B.C. [Hallo 1976, *passim;* Alster 1974: 7]. *No* such precursors for Babylonian pure mathematics exist.

[7] See [Powell 1976: 428*f*].

mathematics when this teaching has been *institutionalized* and thereby has become the task of a partly closed milieu. […].

The practical even if sometimes abstract character of Sumerian mathematics is in perfect harmony with what little we know about the curriculum of the Ur III school: It was purely utilitarian, and had no room for *l'art pour l'art*[8].

Babylonian culmination

The practical fixation of Sumerian mathematics and mathematics teaching may perhaps be regarded as a consequence of the integration of the Sumerian school and the scribal profession in the state administration. We may guess that the unequivocal public attachment of the scribal function may have restricted the ideological autonomy of the scribal school and thereby its institutional independence.

This is only speculation, and we may leave it as it stands. […].

Individualism in the Old Babylonian society was not confined to the commercial sphere. […].

In this situation, the scribal profession seems to have become more independent as a social body; at least, it became less unequivocally attached to the public authority and function. […]. At the same time[9], a genuine pure mathematics was developed […], based in part on *methods* with no relevance for down-to-earth practical tasks – derived, truly, from practitioners' methods, but transformed and developed by the contact with the theoretically generated problems.

As I engaged afterwards in my close reading of the "algebraic" texts, I found confirmation in the predominance of Akkadian terminology; in the Akkadian language structure even of almost exclusively logographic texts; and, not least, in the fact that the only terms that occur exclusively or almost exclusively in Sumerian (or as loanwords provided with Akkadian declination) are those that belong unambiguously to the sphere of practical computation: u š, s a g̃, a . š à, i g i, i g i . g u b, í b/b a . s i $_8$, a . r á .

Late in the 1980s I got the hunch – by then still built on very tenuous evidence – that a tradition of surveyors' recreational riddles might be "older than – perhaps even a source for – Old Babylonian scribe school 'algebra' " [Høyrup 1990a: 275], but invested no more than this single line in the hypothesis.[10]

8 [Cross reference to an earlier note, in which is found:]
According to hymns made in the name of king Šulgi, the curriculum of the Ur III school contained writing, arithmetic, accounting, field measuring, agriculture, construction, and a few subjects the names of which are not understood; cf. [Sjöberg 1976: 173*f*].

9 The precise chronology of the process is at least for the moment not to be known. Still, the decisive steps must be placed in the earlier part of the period, since a number of characteristic texts dating from c. 1800 B.C. have been found in Tell Harmal […].

A limit *post quem* is obtained from the observation that the problem type most characteristic of the new pure mathematics – the "equation" of the second degree – is intertwined with the use of the full potentiality of the sexagesimal number system; it seems […] that this type of mathematics can only have been developed after the use of sexagesimals had been generalized.

[…] The language was Akkadian, the language of the new literary creativity.

10 The manuscript for the publication in question was finished in 1987; in [Høyrup 1990b: 79*f*],

I had no suspicion by then that the various strands of what precedes might end up being intertwined. That they are only struck me at my third or fourth return to the general structure of the Old Babylonian mathematical vocabulary in 1998. The present paper is meant to tell that story.

The Old Babylonian mathematical operations and their vocabulary

The (more or less) technical terms used in Old Babylonian mathematical texts fall in two main groups: (1) terms for operations, and (2) the metalanguage which allows the formulation of problems and the higher-level explanation of the procedures by which they are solved.

The former group can be subdivided thus:

Additive operations

Two operations belong to this group. One is the "identity-conserving" addition in which an extra piece is joined to a quantity; it is always concretely meaningful. It is designated by the Akkadian term *waṣābum,* with the Sumerogram d a ḫ, for both of which I shall use the standard translation "to append".[11] In agreement with the "identity-conserving" character of the operation, no particular term for the corresponding "sum" appears to exist.

The other operation is symmetric, and does not presuppose the addition to be concretely meaningful. It heaps or "accumulates" (the measuring numbers of) two or more addends, connecting them with the word *u* ("and"), and may thus be regarded as a genuine arithmetical operation. The main Akkadian term is *kamārum,* to which correspond the Sumerian term g̃ a r . g̃ a r and the unexplained logogram UL.GAR. The corresponding sum has a name: *kumurrûm*, with logograms g̃ a r . g̃ a r and UL.GAR. Occasionally, other nouns derived from *kamārum* are used for the sum, most remarkable among which is the plural *kimrātum*, apparently a reference to the sum as consisting of still identifiable constituents. On some occasions, *u* alone serves in the same function; in one text (BM 85200+VAT 6599) an abbreviated form of the term used for the sum total in accounting (NIGIN) turns up twice; in both cases, two *numbers* – *viz* a pair belonging together in the table of reciprocals – are added.

written in 1989, the argument is elaborated and some supplementary evidence is cited.

[11] For convenience, I make use of a system of fixed "standard translations" in the following: it is easier to insert the conjugated forms of "append" in an English phrase than those of *waṣābum;* for non-Assyriologist readers it may also be easier to connect them to the common root. The ones I use here differ slightly from the set used in my [1990a] but coincide with those of [Høyrup 2002a].

For the reasons that lead to the interpretation of the terms I shall only refer to my earlier publications, in particular to [Høyrup 1990a] and [2002a].

Subtractive operations

Two "subtractions" occur in the texts, *removal* and *comparison.* Both are always concrete.

Removal may be seen as the inverse of appending. The main term is *nasāḫum*, "to tear out", with Sumerogram zi. It can only be used when the subtrahend is part of the entity from which it is subtracted. Most texts from early 18th-century BCE Eshnunna and some from Goetze's equally early "group 1"[12] prefer the Akkadian *ḫaraṣum*, "to cut off", with no proper Sumerogram;[13] in contrast, zi is used frequently even in predominantly syllabic texts.[14]

Comparison indicates how much one magnitude *A* exceeds another magnitude *B which it does not contain.* It is no inverse of *kamārum,* and cannot be the reversal of any addition (since the sum always contains the addends).[15] The phrase in use states that *A eli B d itter/īter,* "*A* over B, *d* it goes/went beyond" (from *eli … watārum,* "go beyond", "be(come)/make greater than"), with the Sumerographic equivalent *A* u g u *B d* d i r i g.[16]

"Multiplications"

Four different operations can in some way be understood as "multiplications". One is the multiplication of number by number as found in the tables of multiplication, to which corresponds the Sumerogram a . r á (from RÁ,[17] "to go") and the phrase *a* a.r á *b*, meaning "*a* steps of *b*". The term has no Akkadian counterpart but gives rise to the loanword *arûm*<**ara-um*.[18]

12 Goetze, in [MCT, 146–151]]. For the chronology of the group, see [Høyrup 2000: 149]; when speaking in what follows of text groups I refer to the new delimitations of Goetze's original groups established in this latter publication.

13 k u d, used in the late Old Babylonian TMS XXVI, may stand for *ḫaraṣum* but also for *nakāsum,* "to cut down", or *haṣābum,* "to break off" – or, not to be excluded, for *nasāḫum.*

14 For the use of *tabālum,* "to withdraw", and *šutbûm,* "to make leave", for specific removals, see [Høyrup 1993].

15 Because of the symmetric character of accumulation, its actual inverse is the *splitting into* or *singling out of* components (*bêrum,* with no Sumerographic counterpart in the mathematical texts). It may be no accident that the term is used in AO 8862, the very text that speaks of the accumulation as a plural *kimrātum.*

16 The relatively rare comparison made the other way round, the statement of how much *B* falls short of *A* (using the verb *matûm,* "to be(come) small(er)", with Sumerogram l a l), is discussed in [Høyrup 1993].

17 The verb takes on a number of different forms depending on grammatical number and aspect [SLa, §268]: g̃en and du (singular perfective and durative), re_7 and su_8.b (plural ditto). Since both g̃en and du are written DU = RÁ, it is convenient to write the verb as RÁ in order to keep present the relation with the term a.r á (the pronunciation of which is certified by the loanword *arûm*).

18 Admittedly, the metaphor of "repeated going" expressed in Akkadian (*alākum*) and used in a

The second is the determination of a concrete magnitude by means of a multiplicative operation. It is used in multiplications by technical constants and metrological conversions; it serves when volumes are determined from base and height and in the calculation of areas that are not implied by the construction of a rectangle (that is, areas of triangles, of trapezia and trapezoids, and of rectangles which *are already there*). The core term is *našûm*, "to raise", with the Sumerogram íl. Another Sumerogram used in the same function is n i m, logographically connected to *elûm* and *šaqûm* and their various derivations (both "to be/become/make high")".[19]

The third multiplicative operation is the repetition of a concrete magnitude an integer number of times ("until *n*", $2 \leq n \leq 9$). It is spoken of in Akkadian as *eṣēpum*, "to double", with the Sumerographic equivalent t a b (whose original sematic range is wider, for which reason Seleucid mathematical texts could readopt it as a logogram for the identity-conserving addition).

The fourth operation is better dealt with under the following heading.

Rectangularization, squaring and "square root"

This operation, indeed, consists primarily in the "building" (*banûm*) of a rectangle and is only a multiplication in so far as the computation of the appurtenant area is treated as inherent in or implied by the construction. The central term is *šutakūlum*, "to make [two segments *a* and *b*] hold each other" (*viz* as sides of a rectangle – at times grammatical constructions are used which rather imply that *a* together with *b* contain or "hold" the rectangle). ì.g u ₇.g u ₇ (from g u₇, "to eat") is used as a logogram, probably because of the phonetic near-identity between *šutākulum*, "to make eat each other", and *šutakūlum.*[20]

general way (for multiplication as well as repeated "appending") is found in various Old Babylonian texts from Susa. *The pattern of thought* behind the term a.r á thus found expression in Akkadian; but it did not produce an Akkadian equivalent of *the term.*

[19] The original mathematical use of the term is connected to the determination of volumes. In these, indeed, the base is invariably "raised" to the height; in all other cases, the order of the factors is random from a mathematical point of view, and depends first of all on stylistic criteria – as a rule, it is the quantity that has been computed in the preceding operation that is "raised" to the other factor, irrespective of its meaning or role in the computation – cf. [Høyrup 1992: 351*f*].

As is well known, volumes were measured in area units, which implies that these were thought of as provided with a virtual standard thickness of one k ù š (cubit). In consequence, determination of a prismatic volume meant that this virtual thickness was "raised" to the real height. Raising multiplications are thus operations of proportionality, and may hence be said to be "category-conserving".

[20] Traditionally, most workers (Thureau-Dangin being the chief exception), have taken the logogram as an argument that the term should be read *šutākulum*, "to make [*a* and *b*] eat each other", which fits the cuneiform orthography just as well. However, certain texts refer to a segment that has been submitted to the operation with the relative clause "which you have made hold/eat", while others use a noun *takīltum* with precisely the same function; the latter term can only derive from *kullum*

Beyond ì.gu$_7$.gu$_7$ and the abbreviation i.gu$_7$, several other logograms are used: UL.UL (probably to be read du$_7$.du$_7$, for *nitkupum*, "to butt each other"); UR.UR; and NIGIN, which can be interpreted as a contracted LAGAB.LAGAB. In all four cases, the reduplication is probably not to be understood as a genuine Sumerian grammatical form but rather as a way to render in pseudo-Sumerian the reciprocity of the process that the Akkadian language presents by means of the Št and Gt-stems (*šutakūlum* and *nitkupum*, respectively).

Several texts play with repeated numerical multiplication, but no specific term for powers *of numbers* exists. *Squaring*, as a specific process, is *geometric* squaring.

The Akkadian term for the square configuration is *mitḫartum*, a term which refers to a confrontation of equals (*viz*, equal sides), and which when expressed as a number coincides with the side of the square.[21] The word derives from the verb *maḫārum*, whose (causative-reflexive) Št-stem *šutamḫurum*, "to make *s* confront itself", designates the construction of a square with side *s*. Often, however, one of the terms for rectangularization is used instead. With respect to one side, the other side meeting it in a common corner is considered its *meḫrum* or "counterpart", sometimes replaced by the Sumerogram gaba.[22]

Some texts use LAGAB and NIGIN as ideograms (not necessarily logograms, the sign LAGAB *being* a square) in the same functions as *šutamḫurum* and/or *mitḫartum.* Some series texts ⟦a particular genre from the outgoing 17th century BCE⟧ use íb.si$_8$ in the same function;[23] however, the normal function of this important term is different.

Originally it is a Sumerian finite verb form, and it often occurs as a verb in phrases "*Q.e s* íb.si$_8$", where *Q* and s are numbers, s = $\sqrt{Q}$./.e/ is the "ergative" or "locative-terminative" suffix", /íb./ combines a mark of finiteness /ĭ/ with the "inanimate pronominal element" /b/ – *s*, indeed, is no person. In total, the phrase therefore has to be translated

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and mean "which is made hold". Of course, rebus- or pun-like substitutions like that of *šutākulum* for *šutakūlum* constitute the very fundament for the cuneiform writing system and should not surprise us.

21 Its primary reference is thus the square frame, parametrized by the side, not as with Euclid that (area) which is contained by the boundary.

22 *meḫrum* is derived from the same verb as *mitḫartum* etc. gaba, on its part, is wholly independent, meaning rather "identical copy". Anybody who has tried to translate a technical terminology from one language into another will have observed that etymologically related terms often end up having unrelated translations, whereas it is a rare luck to find etymologically related translations for original terms which, though semantically related are etymologically unrelated. Anticipating what is to follow we may conclude that the *mitḫartum/šutamḫurum/meḫrum*-terminology is likely to have originated in Akkadian.

23 A single procedure text, moreover (YBC 6504), uses íb.si$_8$ in parallel with du$_7$.du$_7$, in a construction where it might function logographically for *šutamḫurum.* In BM 15285, a catalogue text concerned with the subdivision of squares into other geometric figures, it alternates with *mitḫartum* as a term for the square configuration.

"by Q, s is equal".[24] The meaning is that when the area Q is laid out as a square, it is flanked by s as side of this square (equal of course to the other sides).

Other texts have left behind the etymology, and use í b . s i 8 as a noun, the name for this side (with Jöran Friberg we may call it "the equalside") – so to speak the geometrical equivalent of a square root.[25]

Quite a few of the texts that use the term as a noun employ the homophonic (unorthographic) writing í b . s i. Other texts use b a . s i or b a . s i 8 (the latter form also occurs as a verb); the shift s i 8>s i simply shows that the etymology is no longer thought of, but the pronunciation still Sumerian;[26] /b a/ is an alternative prefix which probably contains a locative element /a/, indicating that the process takes place "(out) there".[27] At times, the noun appears as a loanword *basûm,* further proof that the pronunciation remained Sumerian.[28]

The function of the í b/b a . s i 8 is not restricted to the case of squares. It may also refer to the side of a cube, which should not disturb us – even the sides of a cube are equal and close to the volume they contain. But true generalizations also occur, somewhat similar to our reference to the "root(s)" of an equation, derived in tangled ways from the concept of a square root.

24 In earlier publications I have blindly accepted the interpretation of .e as an ergative suffix, which would give the phrase the meaning "(caused) by Q, s is made equal". This was originally proposed by Thureau-Dangin, and was indeed the only possibility within the arithmetical reading – *the number* 9 cannot meaningfully be claimed to be close to *the number* 81.

25 Even when no declination elements allow us to distinguish cases, the word order shows whether a verb or a noun is meant, both Sumerian and Akkadian being verb-final. e n . n a m í b . s i 8 thus means "what is equal?", whereas í b . s i 8 e n . n a m means "the equalside, what?".

26 Thureau-Dangin, in contrast, believed í b . s i 8 to be a logogram that was pronounced *mitḫartum;* in a few cases (*viz* when the reference *is* the square configuration) this was certainly the case, but mostly not.

27 The prefix /b a./ is often used regularly with the identity-conserving subtraction z i, "to tear out"; in contrast, the identity-conserving addition d a ḫ, "to append", is often preceded by /b i ./, which suggests a "here", a closer contact.

In the texts from early 18th-century BCE Eshnunna, some texts use the b a - and some the í b-form for the quadratic case (the b a-form always as a noun). b a . s i8 . e is also used in the quadratic case (but as a verb) in two atypical texts from early Old Babylonian Ur (UET V, 859 and 864). Elsewhere, the b a-form is only used for the cubic and for generalized cases (cf. imminently).

28 Some publications use the transliteration í b . s á instead of í b . s i 8. Later lexical lists, indeed, render the pronunciation of the Sumerian verb as *sa-a;* the preference of most editions of mathematical texts for s i 8 is supported by the occasional homophonic shifts to í b . s i and i b . s í, which however might as well be í b . s e respectively i b . s é. Since we also find the syllabic writing b a . s e . e (which can *not* b e ba . s i . i) for *b a . s i8* (IM 52301 rev. 7, 9), the real phonetic value might be between *-se* and *-sä.*

Division, parts and i g i

Division, as we use the word, is both *a problem* – to solve the equation $bx = a$ – and an operation. The familiar assertion that division does not exist in Babylonian mathematics refers to the absence of division as an arithmetical operation of its own.

As is well known, the way a division *problem* is dealt with depends on whether the divisor b is regular or not, that is, whether its reciprocal has a finite expression in the sexagesimal system. The reciprocal of such a number n is called i g i n g̃ á l.bi ("[of 1,] its i g i n g̃ á l"[29]), often shortened to i g i n g̃ á l or i g i n. When a division by such a number n is to be performed, the texts ask for i g i n to be "detached" (*patārum*/d u$_8$), after which the dividend is "raised to" the i g i. In modern terms, division by n is thus performed as multiplication by $^1/_n$. A couple of mathematical texts from Eshnunna replace i g i with *pa-ni,* "in front of",[30] which suggests a reference to the table of reciprocals, where i g i n is indeed present "in front of" n – a folk etymology close at hand, not least because g̃ á l means "to be/place (somewhere)", "to be at disposition". However, since texts from Lagash from c. 2400 BCE speak of $^1/_3$, $^1/_4$ and $^1/_6$ as i g i n g̃ á l, it is clearly nothing but a secondary folk etymology, the opinion of E. M. Bruins ([1971: 240] and elsewhere) notwithstanding. We also find an Akkadian loanword *igûm.*

If the divisor is not (or cannot easily be seen to be) regular (a recurrent situation in mathematical procedure texts though hardly in practical computation, all relevant technical coefficients being chosen to be regular), the text takes note of the non-existence of the i g i and then formulates the division as a problem – "what may I posit (*šakānum*/g̃ a r) to b which gives *(nadānum/*sum*)* me a" – and states the answer immediately, "posit p; raise p to b, a it gives to you" (or some slight variation on this pattern). Mathematical problems being constructed backwards from the solution, this could always be done without difficulty.

Bisection

When dividing by a number which, so to speak, is 2 only "by accident", our texts find "i g i 2" = 30′ and "raises" to that number. Beyond that, a particular sign and a corresponding set of words (*mišlum/*š u . r i . a) for the half exist. They are used, for instance when the width of a rectangle is said to be half its length; if one entity is said to exceed another by its half; or to indicate a measure ("half a barleycorn"). Such relations and measures are "accidental": they might as well have been slightly different.

[29] Strictly speaking, early tables of reciprocals show that the meaning is "[of 1`,] its i g i $n$ g a l". These tables, indeed, list $^2/_3$, $^1/_2$, $^1/_3$, $^1/_4$, etc. of *sixty* = 1` – see Steinkeller [1979: 187]. In later times, genuine reciprocals in our sense may have been thought of.

[30] For instance Haddad 104. The lexical form *pa-nu* occurs in a text from Sippar (BM 96957+VAT 6598, in [Robson 1999: 231]) – but as an interlinear gloss, which suggests that it may have been intended as an indication of pronunciation and nothing more.

Besides this accidental half, however, a different, "necessary" half (invariably half *of something,* never a number – namely of something which naturally or due to the nature of the case falls in two halves) is used in the texts, the *bāmtum* (no Sumerogram seems to exist[31]). It occurs in places where something is bisected into two necessarily equal parts: for instance when the base of a triangle is bisected for the purpose of an area calculation; when the same is done to the sum of opposing sides in a trapezium; and when the radius is found from a circular diameter.

The "necessary half" is invariably found by "breaking" (*ḫepûm*/g a z). This verb, on the other hand, has no other function in the mathematical texts; it always goes together with *bāmtum* (or with ½ or š u . r i . a in the rare texts where these are used logographically for *bāmtum*). In the mathematical texts (but only here), "breaking" is thus the same as *bisection.*

Structure and metalanguage

These mathematical operations are used within texts that are organized with a particular higher-order structure – enunciation and exposition of the procedure, hypothetical-deductive arrangements, relation between "true" entity and representative – and which in order to express that structure make use of what we may call a metalanguage – names for unknown quantities, terms that indicate equality, terms that delimit expressions (corresponding to the brackets of our algebraic expressions), terms that announce results, etc. Of importance for the present argument are the following categories.

Structuration

The mathematical procedure texts are arranged into statement and prescription, with a corresponding distribution of grammatical person and tense. At face value, the structure corresponds to the scene of the scribe school as known from other sources: the master states a problem ("I have done so and so"), next the instructor or "big brother" explains in the present tense or the imperative what "you" (the student) should do, referring occasionally to what was said by "him" (the master). A group of texts from Eshnunna

[31] Non-presence of a term in the extant text material is in general a weak argument. But absence from texts where it *should* have occurred is strong. Such texts are Str 366, 367, 368, all of which insist on writing everything with Sumerograms (except grammatical particles that do not exist in Sumerian), although in clearly Akkadian sentence structures: they eschew the word (Str 366), or they use the number sign ½. This is also done in VAT 7532 and VAT 7535. The text YBC 6504 (already mentioned for its unique use of í b . s i$_8$ in the function of *šutamḫurum,* "to make confront itself") has recourse to š u . r i . a.

BA and BA.A, used in the function of *bāmtum,* are taken in [MKT] to be Sumerograms; actually, we are confronted with elliptic writings or, more likely, with irregular assimilations to the pronominal suffix *-šu*, *bāššu* < **bāmat-šu;* the noun **bûm* [MCT, 161], constructed backwards from similar forms, should also be *bāmtum.*

from the early 18th century BCE, however, start by asking “If somebody has asked you thus, ‘I have done so and so’ ”, suggesting instead that the format belongs originally with mathematical riddles. Early texts from the south, on the other hand – both many of those which belong to group 1[32] and those from early Old Babylonian Ur – deviate from the pattern that prevails elsewhere. All in all, the format thus seems not to have originated as a portrait of the scribe school but to have been imported into it; below (the quotation on p. 680) this conclusion will find further confirmation.

Within this format, the logical structure of the texts and the way to compose fairly unambiguous mathematical expressions is also standardized, though not uniformly in all text groups.

Occasionally, the hypothetical-deductive structure of the problem is made explicit by an introductory *šumma*, “if” – also familiar as the opening of the protases of omina (“if the liver looked so and so”, etc.). Since it is found in most of those texts from Eshnunna that do not carry the full “if somebody has asked you thus” (and in this function almost exclusively in texts stemming from the same northern part of Babylonia), the initial *šumma* may originally have been a vestige of that phrase. In one text from Eshnunna (Haddad 104), however, as in certain other texts, *šumma* is used to introduce variations of an exemplar, carrying thus the meaning “if (instead the situation is as follows:)”. *šumma* may also serve to introduce a smaller piece of deductive reason inside the prescription from already established foundations (“if (as you have now established) …”), or to open the proof; it can be found in either function both in texts from Eshnunna and other localities in the periphery and in texts from the former Sumerian heartland.[33]

inūma, “as”, is used in a couple of texts from the periphery to mark a piece of deductive reasoning on already established foundations. *aššum*, “since”, has the same function in some texts from the periphery and in some from the core (in the periphery specimens it mainly serves as the opening phrase of the procedure prescription).[34]

It may also be used in connection with quotations from the statement which serve to justify the pertinency of single steps in the procedure. The whole quotation is then contained in the phrase *aššum ... qabûku/iqbû*, “since it is said to you”/“since he has said”.

In certain cases, the statement falls into two sections, the first of which contains general information – the value of a technical constant to be used, the rent to be paid

[32] Thus AO 6770, AO 8862, YBC 7997, YBC 9856, YBC 9874.

[33] A division of the text corpus into “northern” and “southern” types was originally undertaken by Goetze, who based himself on orthographic criteria (almost all mathematical texts known in 1945 came from illegal diggings and were thus of unknown origin). In my [2000] the inclusion of the text groups from Eshnunna and Susa led me to replace Goetze’s division by a distinction between the (former) “Sumerian core” and its “periphery”.

[34] Not, however, in texts beginning “if somebody …”.

per b ù r of a field, etc. – whereas the second presents the actual problem. The second section may then be introduced by the word *inanna*, "now".

Even the prescription may be divided explicitly into subsections. Such divisions may be marked by the verbs *saḫārum* "to turn around", *târum*, "to turn back", or n i g̃ í n(. n a), found in the late group 6 (from Sippar) and probably meant as a logogram for *târum* (this term occurs in related texts but *saḫārum* not). At least in the text AO 8862, *saḫārum* appears to be used in the statement about a quite concrete walk around a field which has just been marked out, and *târum* about a return to the starting point. *târum* is used in a similar way in texts from several text groups; it seems no unreasonable guess that this concrete application may have been the origin of the abstract usage as a textual delimiter in the prescriptions.

Many problems – particularly those of "algebraic" character – are shaped as *equations*, statements that (the measure of) a more or less complex quantity equals a number; also present though less common are equations that declare that the (measure of) one quantity equals (the measure of) another quantity. In the former case, the equality is mostly implied by the enclitic particle *-ma* on the verb. In the latter case, the term *kīma*, "as much as", may be found; in the series text but nowhere else it is written logographically as g i n $_7$. (n a m), the Sumerian equative suffix.

A term for equality that may serve as a kind of bracket when complex quantities are constructed verbally is *mala*, "so much as". It is found in the expression "so much as *a* over *b* goes beyond", meaning (*a*–*b*). In the series texts it is replaced by the Sumerian interrogative pronoun a . n a.

The numerical value of a quantity *Q* may be asked for in two ways, either by the question *minûm Q,* "what (is) *Q*", with logographic equivalent en . nam, or by the question *Q kī masi,* "*Q* corresponding to what?".[35] Collective questions for each of several values may be asked for with the question *kiyā*, "how much each".

"Variables"

What allows us to speak of an Old Babylonian "algebra" is the existence of a *standard representation*, a mathematically structured domain onto which problems dealing with entities belonging to other domains but entering in mathematical relations that are homo- or isomorphic with those of the standard domain may be mapped and then solved by analytic procedures (implying that in practice the standard domain is reduced to its mathematical structure, and that it is thus functionally abstract). In the Old Babylonian case, the standard domain is that of squares and rectangles[36] with measured or measur-

35 In the division question, the accusative *mīnâm* is used, "what may I posit to …". Similarly when a geometric square root is asked for by the verbal construction, "by *Q*, what is equal". When the "equalside" is regarded as a noun, it may be asked for by the nominative *mīnûm*, or the student is asked to make it come up", *A basâšu šuli*, or to "take" it (*laqûm*).

36 Certain problems dealing with prismatic excavations are of the third degree. However, as their

able sides and corresponding areas. In a number of problems, these sides represent prices (better, inverse prices, quantity per monetary unit), workers and working days, numbers, or even areas or volumes;[37] as in more recent school algebra, however, most extant problems deal with the basic representation itself.

The names of the entities belonging to the standard domain are, for rectangles, u š ("length"), s a g̃ ("width"), a . š à ("surface"), and for squares *mitḫartum* ("equalside") and a . šà ("surface").

Outside their function as standard representation, u š and s a g̃ are logograms which may be replaced by the syllabic equivalents *šiddum* and *pūtum* (and, in other connections, by other words – s a g̃ thus by *rēšum*, "head"). With exceedingly few exceptions, this does not happen when they serve within this function. Similarly, a . š à is never replaced by the syllabic *eqlum;* in this case, however, the logogram may be provided with Akkadian phonetic elements.

In the case of square configurations and sides, the situation is different (their areas are still a . š à). The configuration *may* be referred to as LAGAB or í b . s i ₈, but *mitḫartum* occurs quite as often. As to the value of the side itself it may also be spoken of as *mitḫartum;* as LAGAB provided with a phonetic complement that identifies the Akkadian pronunciation; or it may said that *s* "confronts itself" (*imtaḫḫar*).[38] A set of curiously related catalogue texts from Eshnunna, Susa and elsewhere[39] refer to the side of the configuration as u š, but with a phonetic complement that identifies it as *pūtum.* Three texts[40] in contrast, speak of the s a g̃ or *pāt,* plural "widths", of square configurations, but at least the first two may be argued to speak of "real", not standard fields.

In modern letter-based algebraic computations it is often mnemotechnically convenient and therefore customary to label derived variables which somehow fulfil the same function as the initial ones by some kind of marking – $\tilde{x}$ for x, etc. The Babylonians used several

constituent elements never serve to represent entities of other kinds, they do not belong to the standard domain; instead, those excavation problems which are of the second or first degree are *represented* themselves within the standard representation

37 A distinction between "true width" and "width" in the Susa text TMS XVI could also mean that the width 20 of a real field (*viz* 20 nindan) is represented by a standard-domain width 20 (*viz* 20′ n i n d a n, fit for the dimensions of the school yard). But this remains a hypothetical interpretation of a passage which seems not to be explainable in any other way. In general, there is no positive evidence that the distinction between "standard-domain" and "real" fields, fairly well-respected in the practice revealed by the texts, was also formulated as a principle.

38 Or, instead, asked "how much, each, confronts itself", with the particular interrogative particle *kiyā.*

39 IM 52916, IM 52685+52304 (Eshnunna), TMS V, TMS VI (Susa), CBS 154+921 (unknown provenience).

40 UET 864, from early Old Babylonian Ur; BM 13901; and CBS 19761, from Nippur.

similar tricks – distinguishing for instance between entity and "true" (*kīnum*/g i.n a) entity or between entity and "false" (*sarrum*/lul) entity, or referring to the way the new entity is produced. Even within groups of closely related texts, however, no uniform pattern for doing so can be identified, nor are "true" and "false" used exclusively in this function within the texts. All devices of the kind were clearly as much *ad hoc* as our picking of X, ξ, u or $\tilde{x}$ for "new x".

Recording

Numbers occur as data in the statement, as intermediate outcome of calculations, and as final results. Several terms and phrases may be used in this connection.

Of particular importance is the verb *šakānum*, "to posit", with logogram g̃ a r. The term appears to designate various kinds of material recording – "putting down" in a computational scheme, writing the value of a length or an area into a diagram, etc. Its is mainly used in two functions: to take note of data in the beginning of the prescription, and thus to prepare their use; and in the formulation of the division problem, "what may I posit to b which gives me a", with the answer "posit p; raise p to b, a it gives to you" (with minor variations). In the latter case, insertion into a computational scheme is likely to be meant.[41] Some texts also "posit" an "equalside" and its "counterpart", i.e., two sides of a square; the same process is indicated in other texts by the verbs *lapātum*, "to inscribe", or *nadûm*, "to lay down". In the geometric text BM 15285 there is no doubt that the actual sense of the latter term is to lay down *in drawing;* most likely, its general meaning in mathematical texts is "to lay down in writing or drawing". *lapātum* is also used regularly about numbers that afterwards serve in additive and subtractive operations; since these seem to have been performed on a counting board and not in clay [Høyrup 2002b], it may also have referred to recording on such a device, which is still in agreement with the general meaning of the term, "to grasp/take hold of"; "inscription" of coefficients, on the other hand, most likely refers to their being written down on a tablet for rough work, cf. [Robson 1999: 30].

A specific phrase for recording an (invariably intermediate) result is *rēška likīl*, "may your head hold (it)". It seems to be reserved for numbers that are not to be inserted in a fixed scheme and therefore are not "posited".

The appearance of a result may be announced in several ways. It may be said that a number *illiakkum*, "comes up for you", from *elûm*, or that a calculation "gives" a certain result (*nadānum*, s u m); alternatively, the text may state that "you see" the result (*tammar*, from *amārum,* "to see"). Very often, the calculation is simply followed by the enclitic particle *-ma* and the number, or by nothing but the naked number.

[41] In YBC 6504, g̃ a r is used to take note of both intermediate and final results.

Third-millennium terminology

We have few mathematical texts from the third millennium, and our direct evidence for the corresponding technical terminology is correspondingly weak. We know that the verb si$_8$ was used at least since c. 2600 BCE to express that a segment λ was the side of the corresponding square area; uš, "length", sag̃, "width" and ašà$_5$ (=GÁN = IKU), "surface", can be followed back to 2400 BCE;[42] the use of the phrase igi *n* g̃ál is also documented for *n* = 3, 4 and 6 since c. 2400 BCE (cf. above).

The only mathematical documents from the Ur III period that contain terms for mathematical *operations* are the tables of reciprocals and of multiplication, of which the former use igi *n* g̃ál and the latter a.rá, "steps of".[43] The stable and invariably Sumerian terminology of the tables of square and cube roots, íb.si$_8$ and ba.si$_8$, allows us to conclude that these terms, too, will have been used already in Ur III.[44]

The other mathematical documents from the epoch, accounts and model documents, only give results, and tell neither the details of calculations nor the terminology in which these were spoken about.

A hymn in the praise of King Šulgi relates that the scribe school is a place where zi.zi.i g̃á.g̃á are learnt together with šid, "counting", and níg.šid, "accounting".[45] The use of the reduplicated forms g̃á.g̃á and zi.zi.i may perhaps depend on the context (description of a habitual practice and not of the single operation). g̃á.g̃á is the *marû* (approximately = imperfective/durative) stem of g̃ar, "to place" [SLa, 305], later used logographically for *šakānum*, "to posit"; in good agreement with the meaning of *kamārum* for which g̃ar.g̃ar is used logographically in the Old Babylonian age (namely "to place in layers, to accumulate"), g̃á.g̃á may thus be understood as "ongoing placing" – but also as "habitual placing". zi.zi is the *marû* stem of zi, "to rise, to stand up" [SLa, 322],

42 Texts in [Allotte de la Fuÿe 1915: 124–132]. For non-rectangular fields, these surveying texts distinguish uš and uš 2.kam, "2nd length", and sag̃ an.na and sag̃ ki.ta, "upper" and "lower width". The equality of (e.g.) lengths is expressed uš si$_8$.a.šà is used about the area in Sargonic texts [Whiting 1984: 69].

43 Until very recently it was impossible to establish with certainty that any of the extant specimens were really of Ur III date; I have now been told by Eleanor Robson (personal communication) that at least tables of reciprocals have been found in dated UR-III contexts.

44 The aberrant use of ba.si$_8$ in Eshnunna and Ur, it is true, could suggest that the distinction which all other text groups uphold between íb.si$_8$ and ba.si$_8$ is a secondary development, and perhaps (namely if we believe that early Old Babylonian Ur is a better witness of Ur-III usages than Eshnunna, only submitted to Ur III until 2025) that the form originally connected with the function as a verb was ba.si$_8$. In Eshnunna, it might then have displaced íb.si$_8$ even when used as a noun; elsewhere, the term of the tables might have got the upper hand.

45 "Šulgi-Hymn B", l.17, ed. [Castellino 1972: 32].

and may perhaps be understood as "take up from" – not too far removed from *nasāḫum*, "to tear out", for which zi is used logographically in Old Babylonian texts,[46] nor however too close. g̃ar or g̃á.g̃á and zi or zi.zi may therefore have been the standard terms for addition and subtraction in the Ur III school; it is not to be excluded, however, that the reference is more specifically to "placing" on the counting board and "taking up" from it. ⟦In consequence of the conclusions drawn in the last section of the article one may even ask whether the Ur III school operated with any general concepts of addition and subtraction, beyond what was done on the counting board.⟧

Other terms are not mentioned in the text, not even a term for multiplication, although multiplication was certainly a cornerstone in the accounting system. However, the relation between syllabic and logographic writings of technical terms in Old Babylonian mathematical texts may permit us to approach the question from the opposite side.

First there are the terms that are written invariably (or almost so) with Sumerograms: uš, sag̃ (including sag̃ an.na and sag̃ ki.ta) and a.šà, when the "lengths", "widths" and "surfaces" of quadrangular and triangular fields are meant;[47] and a.rá. To these come those which occur alternatingly as Sumerograms and as Akkadian loanwords (which indicates that they were really spoken with the Sumerian phonetic value): igi/*igûm* (with the cognates igi.bi/*igibûm* and igi.gub/*igigubbûm*); and íb.si$_8$/ib.sí/ba.si$_8$/ba.se.e/…/ *basûm* when not used logographically for the square configuration. Both of these categories, though used in the algebraic texts, have their roots in a much simpler and much more utilitarian calculational practice.

Then there are terms that may appear in syllabic Akkadian as well as in logographic writing: *wasābum*/daḫ, "to append"; *kamārum*/g̃ar.g̃ar/UL.GAR, "to accumulate", with *kumurrûm*/*kimrātum*/g̃ar.g̃ar/UL.GAR, "accumulation"; *nasāḫum*/zi, "to tear out"; *eli*... *watārum*/ugu … dirig, "over … go beyond"; *našûm*/íl/nim, "to raise"; *esēpum*/tab, "to double"; *šutakūlum*/ì.gu$_7$.gu$_7$/du$_7$.du$_7$/UR.UR/NIGIN, "to make hold each other"; *šutamḫurum*/NIGIN, "to make confront itself", with *mitḫartum*/LAGAB/NIGIN/ìb.si$_8$; *patārum*/du$_8$, "to detach" (an igi); *šakānum*/g̃ar, "to posit", with *nadānum*/sum, "to give", in the answer to the "division question"; *mišlum*/šu.ri.a, "a half" (together with other terms for simple fractions); *ḫepûm*/gaz, "to break", together with *bāmtum,* the "natural half", for which the logogram for ½ or šu.ri.a appear in a small number of texts that insist on writing as much as at all possible with logograms.

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46 zi.zi is used in the unpublished text IM 121613 (courtesy of Jöran Friberg and Farouk al-Rawi) but does not appear in any published Old Babylonian text. ⟦The whole text has now been published with analysis in [Friberg & al-Rawi 2016: 149–188].⟧

47 In contrast, *šiddum* may replace uš when the length of a wall or a carrying distance is meant; similarly, sag̃ may occur as *rēšum*, "head", when an initial value is intended, and is mostly substituted by the latter when an intermediate result is to be kept in memory. This excludes that the logogram is used simply because it is more easily written.

Finally there is a cluster of terms that occur only in Akkadian, or where logograms are either late or arguably not Sumerograms proper but artificial constructions which may have been recognized as such at the time.

Only one term for an operation belongs to this cluster, namely *ḫarāṣum*, "to cut off". It is more common than *nasāḫum,* "to tear out", in the early Akkadian groups ("group 1" and the Eshnunna group) but appears to have fallen in disuse afterwards, perhaps because it occupied the same conceptual niche as *nasāḫum* which was well provided with a standard logogram (zi). In contrast, most terms belonging to the metalanguage are of this kind (those designating variables not, as discussed above).

According to Old Babylonian lexical lists, *šumma*, "if", could be replaced by u_4.da ("at the day when") or tukun.bi, of which at least the former is no more difficult to write than the syllabic writing; but in all its mathematical functions, the word always remains syllabic.

inūma, "as", might also have been replaced by u_4.da; *aššum* might have been mu, as it actually happens in the Late Babylonian but pre-Seleucid mathematical text W 23291; but in Old Babylonian mathematical texts neither is ever written logographically.

inanna, "now", used to divide general from specific information in the statement, could have been i.ne.šè – but it always remains syllabic. So do the terms that serve to demarcate divisions within a prescription *(saḫārum*, "to turn around", and *târum*, "to turn back"), except in the late texts from Sippar. Lexical lists give nigĩn as well as nigĩ́n for either.

-ma, sometimes used to form "equations" where the right-hand side is a number, has no proper Sumerian equivalent, and often appears within text that are otherwise written in more or less grammatical Sumerian. *kīma*, "as much as", the indication that two non-numerical quantities are equally great, is replaced in the late series texts but nowhere else by gin_7.(nam), the Sumerian equative suffix. Also in the series texts but nowhere else, the "algebraic bracket" *mala*, "so much as", appears as a.na.

Among the interrogatives, *mīnûm*, "what", is written logographically as en.nam in the early Ur texts UET V, 121, 859 and 864; in several later texts (groups 3 and 6, and some scattered texts) the same equivalence is used; it is absent from Eshnunna. For the accusative *mīnâm*, UET V, 859 uses a.na.àm once, and so does IM 55357 from Eshnunna (asking for "what is equal", that is, for the side of a square). a.na.àm is also given by lexical lists, and is regular Sumerian meaning "what is it" ([SLa, §120] quotes it from *Inanna's Descent)*; there is hence no need to postulate any direct link between the two appearances. en.nam, as far as I have been able to find out, has no antecedents before the Old Babylonian epoch; moreover, [AHw, 656a] only records it as used in mathematical texts. All in all, it seems to be an *ad hoc* construction made in the context of early Old Babylonian mathematics teaching, probably in Ur[48]. *kī maṣi* has no logographic

[48] Since it occurs together with a.na.àm in UET V, 859 but in a distinct function, it is unlikely

equivalent in the mathematical texts, even though the similar phrase *mala maṣi* used in legal texts [AHw, 621b][49] looks like an Akkadian translation of a.na.àm. *kiyā*, the question for several values, is similarly deprived of logographic equivalent (but seems to be so in general, not only within the mathematical corpus).

Since the semantic boundaries between *šakānum*, *lapātum* and *nadûm* is difficult to establish, the apparent absence of logographic writings for the last two terms may not be significant – we cannot exclude that g̃ar is used more broadly than *šakānum* (though no positive evidence suggests it to be). More interesting are the terms that announce results.

Of these, *nadānum*, "to give", was already mentioned for using the logogram sum in connection with the "division question". In this function, the term is used in all text groups, either in logographic or in syllabic writing; as a general marker of results it is only found in group 3 and (as *nadānum*) in AO 6770 (group 1) and in the two texts YBC 4662 and 4663, in which some slips indicate that it has been introduced as a substitute for a phrase *tammar* used in an original from which they were copied with terminological revisions. In [Høyrup 2000: 165] I summed up the conclusions that can be drawn from this contribution as follows:

> The origin of *nadānum* as a marker of results is with multiplications in the sexagesimal system (and thus with the post-Šulgi tradition); the main use of the term in texts where it does not serve for results in general is in the division question; second comes the indication of the result of "raising" multiplications; thirdly that of final results. It is probably significant that even group 3 tends to use the syllabic forms in this original domain.
>
> It is also significant that the use of *nadānum* goes together with the only systematic breaking of the normal switch between grammatical persons.[50] This switch between the statement formulated by an "I" and a prescription of what "you" should do (at times involving quotes from what "he" has said) becomes a general characteristic of mature Old Babylonian mathematics, the only texts that have some difficulty with the principle being indeed those of group 1. But if it is systematically broken in connection with a term pointing towards the post-Šulgi school tradition, it must come from elsewhere – and the format of [many of the Eshnunna] texts shows how it fits the riddle tradition: The "he" of the prescription is indeed the "someone" who has posed the problem and the "I" of the statement, whence different from the speaking person of the prescription who explains the procedure.

elûm, "to come up", has no Sumerographic equivalent at all when used about results (when used instead of *našûm*, "to raise", it is usually written nim.[51] This is quite

to be a mere phonetic writing of that term.

[49] It is also found (in the subjunctive form *mala maṣû* in the mathematical text VAT 7531.

[50] [Namely the question "what may *I* posit to *b* which gives *me a*", with answer "posit *p;* raise *p* to *b*, *a* it gives to *you*"].

[51] The only Old Babylonian text which has a Sumerographic writing of "coming up" is the Nippur

striking: in Eshnunna, where it is used alternatingly with *tammar*, it is indeed predominantly linked with such computations as point toward the Ur-III scribal tradition. In view of the invariably Akkadian writing we can be fairly certain that the idiom of results "coming up" is a post-Ur III contribution to the scribal computational tradition – probably made in the periphery or the northern part of the core (Nippur) since the term is absent from all texts from the core area with the exception of three group-1 texts and the Nippur texts.

tammar, "you see", *may* on rare occasions appear in logographic writings: pàd ("to see") in early Ur, igi.dù in IM 55357, igi.du$_8$ ("to look at") in the series texts YBC 4669 and 4673, igi in VAT 672. Apart from the appearance in Ur and the apparent slips in YBC 4662 it is totally absent from the core area whereas it dominates the periphery (in Eshnunna used alongside with "coming up", "seeing" belonging predominantly in problems of riddle character, "coming up" as stated above rather with the traditional utilitarian scribal type). The absence from the core is noteworthy, since the parlance can be seen to have been known even there: Str 366 and VAT 7531, both from group 3 (the group in which the "giving" of results was adopted generally) refer to it obliquely in phrases "in order that I see [as result]", how much "do I see".

The fluctuation of the logographic equivalents for *tammar* is best explained if we suppose the Akkadian phrase to be primary and the logograms to be translations from this. One thing must be observed, however: results are already "seen" in a group of Sargonic school texts about rectangles and squares – see [Whiting 1984: 59 n.2]). In these, the word is pàd. This is no proof (and hardly circumstantial evidence) that the use of pàd is continuous: pàd simply happens to be the most precise Sumerian translation of the Akkadian verb *amārum;*[52] moreover, *if* pàd had been carried by the tradition, there would have been no reason to invent a Sumerian substitute in Eshnunna. But it is fairly hard evidence that the Akkadian and the Old Babylonian parlance is linked in one of the two languages, and thus probably through an Akkadian (whence necessarily non-scribal) tradition – which fits the apparent bond between "seeing" and the riddle tradition perfectly well.

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text CBM 12648, which appears to construct Sumerograms for everything, and which has íb.si$_8$ *x* e$_{11}$.dè, "make the equalside of *x* come up".

[52] By the same argument, the unorthographic igi.dù, the corresponding orthographic igi.du$_8$, and the ellipsis igi – less adequate and all presumably from the northern periphery – are more likely to be connected.

Educating the Kapo

I shall not repeat the arguments I have presented elsewhere in favour of the thesis that the particular pattern of Old Babylonian mathematics arose from a synthesis between a tradition of scribal computation deriving from the Ur-III (and, only rarely to be distinguished, pre-Ur-III[53]) scribal computation and one or more traditions of non-scribal practitioners' mathematics – the latter contributing with mathematical riddles that might serve the display of professional proficiency, in agreement with the ideals of Old Babylonian scribal "humanism" (n a m . l ú . u l ù). The above presents only a small part of the evidence; for further elaboration of the argument I shall only refer to my [2000; 2001 ⟦= article I.3⟧; 2002a]. Instead I shall concentrate on what vindicates Robert Englund's unpleasant view of Ur III – a view so unpleasant that many Assyriologists prefer not to believe in its veracity.

Let us first look at what happened outside Ur when the pattern of Old Babylonian mathematics was formed. Apart from some modest vacillation in Eshnunna it appears to have been accepted by everybody that the standard mensurational terms (u š, s a g, including "upper" and "lower", and a . š à) and the core vocabulary for using the place value system (a . r á, i g i, i g i . g u b) referred to legitimately Sumerian practices and should hence be expressed in Sumerian; even in Eshnunna, most deviations from the norms prevailing elsewhere take the shape of unorthographic Sumerian or loanwords, syllabic Akkadian is much rarer.

As far as mathematical operations are concerned, it depends on the general style of the text whether they are spoken of in syllabic Akkadian or logograms. In a number of cases, logograms appear to be fresh inventions, either based on puns (e.g., ì . g u $_7$. g u $_7$), on geometric (e.g., LAGAB) or semantic-grammatical similarity (reduplication of the sign rendering reciprocity); in others, traditional Sumerian names for operations were employed, even though their non-technical meaning did not coincide precisely with that of the Akkadian terms for which they served as logograms, at times in spite of disagreement with the preferences of lexical lists (z i for *nasāḫum*, g̃ a r . g̃ a r for *kamārum).* In some cases, logograms were chosen for which we have no evidence (nor, however, counter-evidence) that they had been used in earlier times as mathematical terms, but where the choice is semantically meaningful (d a ḫ for *waṣābum*, t a b for *eṣēpum*, g a z for *ḫepûm*[54]). Some schools may have had syllabic writing as their stylistic preference

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[53] As I have argued elsewhere, the use of í b / b a . s i $_8$ as a verb is likely to derive from pre-Ur-III usages, while the treatment of the term as a noun reflects its appearance as an "object" in the tables that became increasingly important after the Ur-III introduction of the place value system.

[54] But lexical lists also give the equivalences g a z ∼ *nasāḫum*, t a b ∼ *waṣābum*, which is no less meaningful.

and others may have favoured logographic writing. Their way to speak of mathematical operations was then governed by this general canon of style; the texts betray no tendency overriding general preferences to favour or to avoid referring to the operations in Sumerian.

When turning to the metalanguage we encounter a wholly different situation. None of the "logical operators" *šumma*, *inūma* and *aššum* are ever written logographically; of the terms that allow to formulate a specific question on the background of general information or to structure a complex procedure (*inanna*, *saḫārum* and *târum*), only the third may be replaced by a logogram, and this only happens in the late group 6. Similarly, those which allow the formulation of an equation (*-ma*, *kīma* and *mala*) always remain syllabic outside the late group of series texts, written in very compact pseudo-Sumerian. Among the interrogatives, *kī/mala masi* and *kiyā* always remain syllabic, and *mīnûm* always so except for one appearance of a.na.àm in Eshnunna and from its replacement in the late texts from group 6 by what may at the time have been recognized as a specifically mathematical pseudo-Sumerogram. Finally, among the terms that announce a result, only the one connected to place-value multiplications (that is, to a practice with a clear Sumerian history) is ever written logographically, except for the utterly scarce appearances of igi.du$_8$ for *tammar*, all but one of which, moreover, are late.

Since logograms for most of these terms were available in lexical lists (whence familiar to the authors and users of the mathematical texts), we must conclude that they were consciously evaded. But these are the terms that are needed if mathematics teaching is to go beyond the inculcation of routines, that is, if it is to be based on problems. We must conclude that the authors of the mathematical texts chose to demonstrate through the way they formulated problems that *problems were a new genre* and no inheritance from the Sumerian scribe school. And problems, as is well known, constituted the core and most of the body of Old Babylonian mathematics.

So far I have left out early Old Babylonian Ur from this analysis. Ur, the former queen of Ur III, of course was not likely the emphasize the break with the past, even though the sparse problem texts that have been found do suggest that they come from a borrowed tradition.[55]

If is therefore not astounding that the metamathematical vocabulary of these texts is in Sumerian, but noteworthy that it is meagre and not very consistent: en.nam (probably

[55] In UET V, 864, everything is in Sumerian except the key technical terms *dakāšum* and *dikištum;* the topic was thus something which could not be discussed in Sumerian, obviously because it had not been done before; UET V, 858 presents us with the problem of a bisected trapezium, but in a trite version that suggests that the author had heard that the transversal 13 of a trapezium with parallel sides 17 and 7 divides the height in ratio 2:3 but either did not know *that* or did not know *why* this transversal bisects the area. That it did was known in Sargonic surveying and constituted stock knowledge in Old Babylonian mathematics in general; it can only have been found out by somebody who also knew an argument *why*.

a new pseudo-Sumerogram, cf. above) is used in UET V, 121, 864 (in varying spellings) and 859, and a.na.àm once in UET V, 859. Results are "seen" (pàd) in UET V, 859 and 864, whereas they carry the Sumerian enclitic copula -àm, "it is",[56] in UET V, 858. The prospective prefix ù.ub., "and then", appears to be a fresh translation of the Akkadian suffix *-ma*. It is indeed hyperorthographic, and thus not traditional – íb.tag$_4$ does not appear in the perfectly parallel form ì.ib.tag$_4$. All this certainly does not look like a continuation of a stable tradition, rather as a tentative exploration of an unknown terrain.

Before drawing the final conclusions we might ask whether Old Babylonian mathematics teachers are really likely to have expressed any ideas or ideals through their shaping and use of a technical terminology. The answer is emphatically affirmative – examples of very conscious adoption of a characteristic terminological canon and of elimination of unwanted terms abound. One example was mentioned above: the elimination of the familiar phrase *tammar* from group 3 and YBC 46627, 4663 and its replacement by *nadānum*/sum (elsewhere exclusively used in connection with sexagesimal multiplication). We may also mention AO 8862 from group 1, which asks questions about the dimensions of fields by *mīnûm*, whereas *kī maṣi* is used when brick calculations are concerned. Most striking of all is perhaps a group of 9 tablets found in a single room in Tell Harmal, Eshnunna, which invariably couple results "coming up" with the question *mínûm* and the "seeing" of results with *kī maṣi* (a system which is found nowhere else and even broken in a tablet coming from the neighbouring room – see [Høyrup 2000]: 122 n.9, 126).

Deliberate shaping of the vocabulary beyond what was required by the mathematics it was meant to express was thus not alien to the mind-set of Old Babylonian mathematics teachers, and we may trust that they had a reason for doing so when requiring one category of terms to be written invariably with Sumerian logograms, allowing others to appear in syllabic or logographic writing according to the local stylistic canon, and still others to be written always in syllabic Akkadian. Seeing that those which are invariably in Sumerian are legitimately so from the historical point of view, we may conclude that those which are not allowed to appear in Sumerian – those that allow the formulation and solution of *problems* – were supposed to represent something new; the meta-terminological vacillations of the problem texts from Ur indicate that the mathematics teachers of the remaining Old Babylonian orbit were not mistaken on this account. The whole *discourse of problems* must have been absent from the legacy left by the Ur III school.

Mathematical problems, of course, were not a fresh invention of the Old Babylonian school. Students of the Old Akkadian schools had learned their mathematics from problems, and "seen" results; problems are also to be found among the school texts from Shuruppak.

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[56] Never used elsewhere in this function in Old Babylonian mathematics!

What will have been new is instead a mathematics teaching *not* based on problem solution, a mathematics teaching deliberately eliminating even the slightest appeal to independent thought on the part of the students. It is also unique in history: even the school of the Third Reich went no further in its control of the students' minds than to having problems deal with "artillery trajectories, fighter-to-bomber ratios and budget deficits accruing from the democratic pampering of hereditarily diseased families" [Grunberger 1974: 367].

In that school which came out of an administrative reform which completed a military reform, Ur III seems to have aimed at exactly what Orwell's [1954: 241] *Newspeak* was meant to effectuate: "to make all [unauthorized] modes of thought impossible" – at least among those miserable and terrible Kapo–overseer-scribes who constituted the "Outer Party" of king Šulgi's statal system.

List of tablets referred to

AO 6770. Published in [MKT II 37f, cf. III 62*ff*].
AO 8862. Published in [MKT I 108–113, II Taf. 35–38; III 53].
BM 13901. Published in [Thureau–Dangin 1936].
BM 15285. Published in [MKT I 137]; with an additional fragment in [Saggs 1960]; with yet another in [Robson 1999: 208–217].
BM 85200+VAT 6599. Published in [MKT I 193*ff*, II Taf 7–8 (photo), Taf 39–40 (hand copy)].
BM 96957+VAT 6598. Published in [Robson 1999: 231–244].
CBM 12648. Published in [MKT I 234*f*], much improved in [Friberg 2001: 149].
CBS 154+921. Published in [Robson 2000: 40].
CBS 19761. Published in [Robson 2000: 36*f*].
IM 52301. Published in [Baqir 1950b].
IM 52916+52685+52304. Published in [Goetze 1951].
IM 55357. Published in [Baqir 1950a].
IM 121613. Preliminary publication in [Friberg & al-Rawi 1994].
Str 366. Published in [MKT I 257, III 56], hand copy [Frank 1928: #9 = Taf vii].
Str 367. Published in [MKT I 259*f*], hand copy [Frank 1928: #10 = Taf viii].
Str 368. Published in [MKT I 311], hand copy [Frank 1928: #11 = Taf ix].
TMS V. Published in [TMS 35–49, Pl 7–10].
TMS VI. Published in [TMS 49–51, Pl 11–13].
TMS XVI. Published in [TMS 91*f*, Pl 25].
TMS XXVI. Published in [TMS 124*f*, Pl 39].
UET V, 121. Published in [Vajman 1961: 248], cf. [Friberg 2000: 35*f*].
UET V, 858. Published in [Vajman 1961: 251], cf. [Friberg 2000: 38*f*].
UET V, 859. Published in [Vajman 1961: 254*f*], cf. [Muroi 1998: 201*f*] and [Friberg 2000: 39].

UET V, 864. Published in [Vajman 1961: 257*f*], cf. [Muroi 1998: 200] and [Friberg 2000: 37]
VAT 672. Published in [MKT I 267, II Taf 43].
VAT 7531. Published in [MKT I 289*f*, II Taf 46; III 58].
VAT 7532. Published in [MKT I 294*f*, II Taf 46; III 58].
VAT 7535. Published in [MKT I 303–305, II Taf 47].
W 23291. Published in [Friberg 1997].
YBC 4662. Published in [MCT 71*f*, Pl 8].
YBC 4663. Published in [MCT 69, Pl 7].
YBC 4669. Published in [MKT I 514, III 27*f*, Taf 3].
YBC 4673. Published in [MKT I 507*f*, II 508, III 29–31, Taf 3].
YBC 6504. Published in [MKT III 22*f*, Taf 6].
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Chapter 25 (Article II.8) A Hypothetical History of Old Babylonian Mathematics − Places, Passages, Stages, Development

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

Most general standard histories of mathematics speak indiscriminately of "Babylonian" mathematics, presenting together the mathematics of the Old Babylonian and the Seleucid period (respectively 2000−1600 and 300−100 BCE) and neglecting the rest. Specialist literature has always known there was a difference, but until recently it has been difficult to determine the historical process *within* the Old Babylonian period.

It is still impossible to establish the details of this process with certainty, but a rough outline and some reasoned hypotheses about details can now be formulated.

Contents

WILBUR KNORR
in memoriam

Non-history of Mesopotamian mathematics in general histories of mathematics

General histories of mathematics often begin with or contain a chapter on "Babylonian mathematics", or perhaps a chapter mixing up Babylonian matters with early literate mathematics in broader generality. I shall discuss only two examples, both of them much appreciated and more serious than many others: Uta Merzbach's *A History of Mathematics* from [2011], a revised version of [Boyer 1968]; and Victor Katz' *A History of Mathematics: An Introduction*, the second edition of which appeared in [1998] (I have not seen the third edition from 2009 but do not expect it to be fundamentally different from its predecessor in this respect ⟦[1]⟧).

Merzbach's changes of Boyer's original text about Mesopotamia are modest – most of the chapter is taken over verbatim, including outdated interpretations, gross historical blunders, and fantasies. Some pertinent publications from recent decades are listed in the bibliography, but they seem not to have been consulted. In the preface to the new edition (p. xiv) the late Wilbur Knorr is praised for "refusing to accept the notion that ancient authors had been studied definitively by others. Setting aside the '*magister dixit,'* he showed us the wealth of knowledge that emerges from seeking out the texts"; but in spite of that Merzbach seems to have thought that regarding Mesopotamia there was nothing to add to or correct in the words of the master who wrote the prototype of the book.

And what were then the words of this master concerning the historical development of mathematical thought and techniques in the area? It is acknowledged that the place value system was an *invention,* which is supposed to have taken place some 4000 years ago [Boyer 1968: 29; Merzbach & Boyer 2011: 24]. It is also recognized that "most of [the source material] comes from two periods widely separated in time. There is an abundance of tablets from the first few hundred years of the second millennium BCE (the Old Babylonian age), and many tablets have also been found dating from the last few centuries of the first millennium BCE (the Seleucid period)" – thus [Merzbach & Boyer 2011: 24].[2] But neither edition sees any difference between what was done in the two

[1] ⟦This guess turns out to be wrong. In [Katz 2009], the chapter on "Ancient mathematics", in which ancient Mesopotamian, Egyptian, Indian and Chinese material is dealt with pêle-mêle (see imminently) has been replaced by one on "Egypt and Mesopotamia", treated separately, while the chapter on "Medieval China and India" has become two chapters, on "Ancient and Medieval China" and "Ancient and Medieval India", respectively. The changes, as told in the preface [Katz 2009: xiv], are inspired by the editorial work for [Katz 2007].⟧

[2] In [Boyer 1968: 29], instead of "many tablets have also been found dating" we find "there are many also". Actually, that "many" Seleucid mathematical texts have come down to us is only true if we count "one, two, many", or if we count astronomical texts (which are not mentioned in the

periods, even though some of the mathematical formulae Boyer and Merzbach extract from the texts belong to the Old Babylonian only and others exclusively to the Seleucid period – apart from one contribution “not in evidence until almost 300 BCE”, namely the use of an intermediate zero in the writing of place value numbers.[3] The “Orient”, as we have often been told, is eternal, be it in wisdom or in stubborn conservatism – and much more so in the periphery of Orientalism than according to those scholars whom Edward Said [1978: 18] justly praised in spite of what he could say about their ideological context.

Merzbach and Boyer at least know that Pharaonic Egypt, Mesopotamia, pre-Modern China and pre-Modern India have to be treated separately and not (as in [Katz 1998]) pêle-mêle as the mathematics of “ancient civilizations”, structured only according to mathematical topic – namely “counting”, “arithmetic operations”, “linear equations”, “elementary geometry”, “astronomical calculations”, “square roots”, “the Pythagorean theorem”, and “quadratic equations”. Katz, like Boyer/Merzbach, recognizes that the great majority of Mesopotamian mathematical tablets are from the Old Babylonian period, a few being of Seleucid date (and still fewer from other periods, not always correctly identified in the book) – but even he sees no change or development beyond the introduction of the intermediate zero, similarly believed to be a Seleucid invention.

Before leaving this section I shall point out once more that I did not choose these two books because I find them particularly faulty but because I regard them as better than most on the Mesopotamian topic.

What is known to those who care about the historical development of Mesopotamian mathematics

The earliest formation of something like a state in Mesopotamia took place in the later fourth millennium BCE around the city Uruk, as a bureaucratic system run by the high priests of the temples.[4] The organizational innovation was intertwined with the creation of a script and the creative merging of earlier mathematical techniques into a coherent system of numeration, metrologies and computational procedures (procedures which we know next to nothing about beyond the results they yielded). Writing and computation served solely in accounting, which is amply represented in surviving clay tablets; there is no trace whatever of interest in mathematics going beyond that, nor of the use of writing for religious, literary or similar purposes. The only thing we need to

book).

[3] Actually, this “zero” (which often replaces not a missing sexagesimal place but a missing level of ones or tens) is already used in a few late Old Babylonian texts from Susa.

[4] Sources for this ultrashort summary, as well as a somewhat broader exposition, can be found for instance in [Høyrup 2009] ⟦= article II.6⟧.

know (because of its importance below) is that the number system had separate notations for 1,10, 60, 600, 3600, and 36000; we may characterize it as "sexagesimal" (that is, having base 60) or as alternatingly decimal-sexismal (as the Roman system can be considered decimal or alternatingly quintal-dual). Metrologies were not sexagesimal, but their step factors were compatible with the sexagesimal system.

Around the beginning of the third millennium BCE, the Uruk state gave place to a system of city-states competing for water and other resources and mostly ruled by a military leader (a "king"). For a couple of centuries, writing and computation disappear from the archaeological horizon, but they return forcefully around 2550,[5] at a moment when a scribal *profession* begins to separate from the stratum of priestly managers. Slightly earlier already, the first short royal inscriptions turn up, and in the cities Shuruppak and Abu Ṣalābīkh we find the earliest instances of literary texts (a temple hymn, and a collection of proverbs) as well as the earliest specimens of "supra-utilitarian" mathematics − that is, mathematics which formally looks as if it could serve in scribal practice, but whose substance goes beyond what could ever be needed. One problem deals with the distribution of the barley contained in a silo of 1,152,000 "litres" (40×60 "tuns", each containing 480 "litres") in rations of 7 "litres" to each worker. The resulting number of 164571 workers exceeds the population of the city state. More telling, a "divisor" 7 would never appear in genuine practice; its merit here is exactly that it is *not* compatible with the metrologies involved, and that the solution builds on a technique which a working scribes would never need to apply.

The use of writing for the recording of "literature" is equally supra-utilitarian; the appearance of both genres can be presumed to depend on the professional (and ensuing intellectual) semi-autonomy of the scribes. Temple managers could be proud of being powerful and associates of the gods; the scribe had to be proud being a scribe, that is, of mastering the two techniques for which he was responsible, and his professional pride was best served if he mastered them with excellence, that is, beyond what was needed in trite daily practice.

But the *raison d'être* of the profession of scribes was of course this daily practice, and its expansion beyond its earlier scope. Really utilitarian mathematics did not disappear, and throughout the third millennium we see an expansion of metrologies. As Sargon of Akkad (a city located somewhere in the vicinity of present-day Baghdad) united southern and central Mesopotamia into a single territorial state around 2350 (expanding into a genuine empire under his successors), literature – in the shape of rewritten versions of the mythology fitting the new political conditions – came to function as propaganda. Supra-utilitarian mathematics could offer no similar service, but from the Sargonic school we still know a number of supra-utilitarian problems dealing with rectangles and squares (for

[5] As all dates in what follows BCE, and according to the so-called middle chronology.

instance, stating the area and one side of a rectangle and asking for the other side – a problem no real-life surveyor of tax collector would ever encounter). Concomitantly, a new "royal" metrology for use in interregional affairs was introduced (probably neither replacing earlier local metrologies nor meant to do so).

Third-millennium upward and downward extensions of metrologies were made sexagesimally (and a new weight metrology was almost fully sexagesimal from the start). Other changes were apparently made so as to facilitate administrative procedures, and were therefore not made according to the sexagesimal principle. That contradiction was overcome during the next centralization, following after the demise of the Sargonic empire around 2200 and a decentralized interlude lasting until c. 2100. The new centralized state, called "Ur III" (short for "Third Dynasty of Ur"), introduced a military reform around 2075, and immediately afterwards an economic and administrative reform.

The gist of this reform was the organization of large part of the working force of the country in labour troops supervised by overseer scribes. These were responsible for the produce of their workers according to fixed norms – so many bricks of a certain standardized type produced per day, so much dirt carried a certain distance in a day, etc. The control of all this involved an enormous amount of multiplications and divisions, as can be imagined. The way to make this work was to have tables of all the technical norms involved, to translate (using "metrological tables") all traditional measures into sexagesimal numbers counting a standard area, a standard length, etc. (corresponding to the translation of 2 yards, 2 feet and 3 inches into 99 inches). These sexagesimal numbers were of a new type, written in a floating-point place-value system (absolute orders of magnitude thus had to be kept track of separately); with these, multiplications and divisions (the latter as multiplication by the reciprocal of the divisor) could be performed by means of tables of reciprocals and multiplication tables. Finally, the resulting place-value outcome could be translated back into current metrologies (e.g., weight of silver, as a value measure) by means of a metrological table.

The place-value *idea* may be older, but only the integration in a system of arithmetical, metrological and technical tables made its implementation worthwhile. This implementation presupposed the whole system to be taught in scribal training. However, it appears that rank-and-file scribes were taught no mathematics beyond that – the whole tradition of mathematical *problems,* not only of supra-utilitarian problems, appears to have been interrupted; beyond training (and learning by heart) of the tables belonging to the place-value system, mathematics teaching seems to have consisted in the production of "model documents", emulating such real-life documents as the scribe would have to produce once on his own. Mathematics teaching was apparently directed exclusively at the drilling of routines – even the modicum of independent thought which is required in order to find out how to attack *a problem* was apparently unwanted.

Old Babylonian political history in rough outline

Around the time of the military reform, Susa in the Eastern periphery and the area around Eshnunna in the north-to-east had been conquered by Ur III; in 2025 they rebelled, and around 2000 – the beginning of the "Old Babylonian period" – even the core disintegrated into smaller states, dominant among which was first Isin, later Larsa.[6] In both, but most pronouncedly in Larsa, the politico-economical structure was gradually decentralized. In the north, the cities Sippar and Eshnunna became important centres, and in the early 18th century Eshnunna had subdued the whole surrounding region. In the north-west, Mari (which had never been subject to Ur III) was the centre of a large territorial state. Between Sippar and Eshnunna to the north and Larsa to the south, Amorite chiefs had made Babylon the centre of another state.

In 1792, Hammurabi became king of Babylon. A shrewd diplomat and warrior, he took advantage of existing conflicts between the other powers in the area to subdue Larsa and Eshnunna in c. 1761, and Mari in c. 1758.[7] Isin had already been conquered by Larsa in c. 1794, which had taken over from Isin the control of Ur already before 1900.

With Hammurabi's conquests, southern and central Mesopotamia thus became "Babylonia", and it remained so for some two thousand years (Assyria, in northern Mesopotamia, is a different entity which we do not need to take into account here).

None the less, Hammurabi's Babylonia was not politically stable. In 1740, ten years into the reign of his son, Larsa revolted, and the first emigration of scholar-priests from the south toward Sippar began. The revolt was suppressed (possibly with great brutality), but twenty years later the whole south seceded definitively, and formed "the Sealand", where scribal culture appears to have become strongly reduced.[8]

After another century of increasing internal and external difficulties, Babylon was overrun by a Hittite raid in 1595, and afterwards the Kassite tribes (already familiar in the preceding century, both as marauders and as migrating workers) took over power. This marks the end of the Old Babylonian period, which thus roughly spans the four centuries from 2000 to 1600.

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6 This section synthesizes information that is more fully documented in [Liverani 1988], [van de Mieroop 2007], and various articles in [RlA].

7 I repeat that these dates are according to the "middle chronology". According to the "high" and the "low" chronologies, they fall c. 60 years earlier respectively later. This has no importance for the present argument.

8 A newly published collection of texts [Dalley 2009] shows that literacy did not disappear completely (which would anyhow be difficult to imagine in a situation where statal administration did not vanish) – but the writing style of the same texts shows that the level of erudition was low (p. 13).

Old Babylonian mathematics, and mathematics teaching

Administrative and economic records from this as well as other epochs present us with evidence of computation and area determination, but mostly tell us little about the mathematical procedures involved (even the place value system, serving only for discarded intermediate calculations, is absent from them). Our entrance to mathematics as a field of *practised knowledge* thus goes via texts which, at least by their format, can be seen to be connected to scribal teaching.

Apart from a huge lot found *in situ* in Nippur (since the third millennium an important temple city, but in the northern part of what was to become the Sealand), a rather large lot (badly) excavated in Susa and smaller batches from Ur, from Mari and from various towns in the Eshnunna area, almost all Old Babylonian mathematical texts have been "found" on the antiquity market – that is, they come from illegal diggings in locations which dealers did not identify or did not identify reliably. However, orthographic and terminological analysis allows to assign most of the important texts to a rather small number of coherent groups and to determine their origin.[9]

The Nippur corpus gives us a detailed picture of the general scribal curriculum, literary as well as mathematical [Veldhuis 1997; Proust 2004; Proust 2008]; the texts must predate 1720, where Nippur was conquered by the Sealand and soon deserted, and they are likely to be from c. 1739 [Veldhuis 1997: 22]. As far as the mathematical aspect is concerned, it shows how the Ur III curriculum must have looked (not our topic here), but it contains only three verbal problem texts (the ones which allow us to discern historical change) – too little to allow us to say much.[10]

There are, moreover, problem texts almost or totally deprived of words, of the types "*a*, *b* its reciprocal" or "*s* each square-side, what its area? Its area *A*", or simply indicating

[9] A first suggestion in this direction was made by Otto Neugebauer [1932: 6*f*]. This division into a "northern" and a "southern" group was refined by Albrecht Goetze [1945], who assigned most of the then-known texts containing syllabic writing (which excludes the Nippur corpus, whose origin was anyhow well known) into six groups, four "southern" (that is, from the former Ur III core) and two "northern".

Since then, the texts from Susa, Ur, Mari and Eshnunna have been added to the corpus. The picture which now presents itself is described in [Høyrup 2002a: 319−361]. Apart from the inclusion of new text groups it largely confirms Goetze's division though with some refinements due to analysis of terminology (Goetze had considered orthography and occasionally vocabulary, but mostly without taking semantics into account).

[10] Similar texts, though never copious enough in one place to allow us to identify deviations from the Nippur curriculum (but showing that such deviations will not have affected the fundamental substance), are found in various other places – see not least [Robson 1999: 272−277; Robson 2004; Proust 2005]. Exceptionally, some of the texts in question from Larsa are dated, in part to c. 1815, in part to c. 1749 [Robson 2004:13, 19].

the linear dimensions of a triangle and writing its area inside it [Robson 2000: 22, 25, 29]. They are student exercises, and correspond to those written in the Sargonic or Shuruppak schools. Changes in vocabulary (more precisely the appearance of the word EN.NAM[11] meaning "what", a pseudo-Sumerogram invented in the Old Babylonian period) shows them not to be a direct continuation of Ur III school habits.

While showing the characteristics of general scribal education, the Nippur corpus is not of much help if we want to build at least a tentative diachronic *history* of Old Babylonian mathematics. For this, we need genuine word problems.

The earliest Old Babylonian mathematical text group containing genuine word problems comes from Ur (if anything, the core of Ur III). The pertinent texts belong to the 19th century – since they have been used as fill, nothing more precise can be said [Friberg 2000: 149*f*].[12] Many are simply number exercises, and thus reflect training in use of the place-value system. But there are also word problems, which provide evidence for an attempt to develop a *problem format:* the question may be made explicit (depending on grammatical case by a regular Sumerian A.NA.ÀM or by the pseudo-Sumerogram EN.NAM, both meaning "what"); a few times results are "seen" (PÀD or PAD[13]). PÀD is regular Sumerian, and is already used in certain Sargonic problem texts; the phonetically caused "misspelling" PAD speaks against transmission via the Ur III school, whose Sumerian was highly developed.

The themes of the word problems all fall within the sphere of Ur III scribal practice, even though some of them are certainly supra-utilitarian. In this respect they do not point toward the imminent developments that have come to be known as "Old Babylonian mathematics".[14] That, on the other hand, shows that the new *attitude* toward mathemat-

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[11] For simplicity, I write all logograms, whether identified with Sumerian words or not, in SMALL CAPS (when philological questions are dealt with, Sumerian is often written in s p a c e d w r i t i n g and "sign names" as SMALL CAPS or even CAPITALS); this should be superfluous here.

Syllabic spellings of Akkadian conventionally appears in *italics.*

[12] A few mathematical texts from Ur – found at different location, namely in a house that served for scribal training in a reduced curriculum – belong to the 18th century [Friberg 2000: 147*f*]; they are of the same types as those found in Nippur and confirm the general validity of the Nippur curriculum.

[13] Accents and subscript numbers in transliterated Sumerian indicate different signs used for homophones – or at least for terms which were homophones after Ur III, when Sumerian was a dead language taught at school to Akkadian speakers. Since Sumerian was a tonal language but Akkadian not we cannot be sure whether homophony is original or the result of phonetic impoverishment called forth by the transfer to a new linguistic environment.

In the actual case, while PÀD means "to see", PAD originally stood for a small portion of nourishment, or for "to bite". We do not know whether tone, some other small difference in phonetic quality or only contexts allowed the Sumerians to distinguish.

[14] This claim seems to be contradicted by three texts, UET 6/2 274, UET 5, 858 and UET 5, 859

ics, emphasizing the role of genuine and often supra-utilitarian problems, preceded the substantial expansion of mathematical interests, know-how and know-why.[15] Moreover, it suggests that the change in attitude was a driving force behind the expansion.

The new attitude is not an isolated phenomenon characterizing only the culture of mathematics teaching. It corresponds to a general change of cultural climate at least at elite level, emphasizing so to speak individual self-conscience, expressed also by the appearance of private letter writing (when needed being served by free-lance scribes) and of personal seals (as opposed to seals characterising an office or function).[16] Within the scribal environment, it expressed itself in an ideal of "humanism" NAM.LÚ.ULU, literally "the condition of being human"), which a scribe was suppose to possess if able to exert scribal abilities beyond what was practically necessary: writing and speaking the dead Sumerian language, knowing rare and occult meanings of cuneiform signs, etc. The texts which explain this ideal (texts studied in school and thus meant to inculcate professional ideology in future scribes) also mention mathematics, but give no particulars.

None the less, this ideology illuminates not only the strengthened reappearance of mathematical problems but also the new kind of mathematics which turns up in the various texts groups from the 18th and 17th centuries.

Before discussing these text groups, a short characterization of this mathematics will be useful. If counted by number of problem statements, with or without description of the procedure, the dominant genre is the so-called "algebra", a technique which allows to solve problems involving the sides and areas of squares or rectangles by means of cut-and-paste and scaling operations.[17] We may look at two simple specimens, both

[Friberg 2000: 113, 142, 143]. The first contains only numbers, but Jöran Friberg has shown that they correspond to finding the sides of two squares for which the sum of their areas and the ratio between the sides is given. This problem turns up in several later text groups in the context of metric "second-degree algebra", but it is much simpler than the basic stock of this discipline. The second deals with the bisection of a trapezium by means of a parallel transversal, something which already Sargonic surveyor-calculators had been able to do correctly; but here the problem is reduced to triviality, since the ratio between the two parts of the divided side is given. The third asks for the side of a given cubic volume. Since tables of "square" and "cube roots" (better, inverse tables of squares and cubes) were already part of the Ur III system, this does not fall outside Ur III themes – and since the solution ends by transforming the resulting place value number into normal metrology (doing so wrongly!), we get extra confirmation of this.

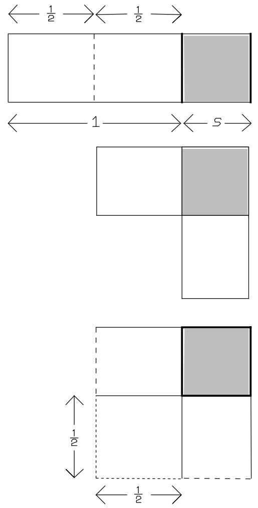
15 Since no development of autonomous *theory* but only of well-understood techniques and procedures takes place, these terms seem adequate.

16 Elaboration, documentation and further references in [Høyrup 2009: 36*f*].

17 A detailed analysis of this technique based on close reading of original sources is found in [Høyrup 2002a: 11–308]. A shorter (but still extensive) popular presentation in French can be found in [Høyrup 2010] ⟦now also available in English, as [Høyrup 2017]⟧.

borrowed from the text BM 13901, which contains 24 “algebraic” problems about one or more squares.[18]

Problem #1 from the text states that the sum of a square area and the corresponding side is $^3/_4$. In the adjacent diagram,[19] the area is represented by the grey square $\square(s)$, while the side is replaced by a rectangle $\sqsubset\!\sqsupset(s,1)$. The composite rectangle $\sqsubset\!\sqsupset(s+1,s)$ thus has an area $^3/_4$.



The procedure used to solve BM 13901 #1

As a first step of the procedure, the excess of length over with is bisected, and the outer half moved around so as to contain together with the half that remains in place a square $\square(^1/_2)$, whose area is evidently $^1/_4$. Adding this to the gnomon into which the rectangle $\sqsubset\!\sqsupset(s+1,s)$ was transformed gives an area $^3/_4+^1/_4 = 1$ for the large square, whose side must thus be 1. Removing the $^1/_2$ which was moved around leaves $1-^1/_2 = ^1/_2$ for the side s.[20]

#3 of the same text is “non-normalized”, that is, the coefficient (“as much as there is of it”, as expressed in a different text) is not 1. This asks for application of the “scaling” technique. The statement explains that $^1/_3$ of a square area (dark grey in the diagram) is removed, and $^1/_3$ of the side (represented by a lighter grey rectangle $\sqsubset\!\sqsupset(^1/_3,s)$ in the diagram) is added. Instead of a square area $\square(s)$ we thus have a rectangle $\sqsubset\!\sqsupset(^2/_3\ s,s)$, but changing the scale in the vertical direction by a factor $^2/_3$ gives us a square $\square(\sigma)$, $\sigma = {}^2/_3\ s$. In the same transformation the rectangle $\sqsubset\!\sqsupset(^1/_3, s)$ becomes $\sqsubset\!\sqsupset(^1/_3,\sigma)$. Thereby the situation has been reduced to the one we know from #1, and σ is easily found. Finally, the inverse scaling gives us s.[21]

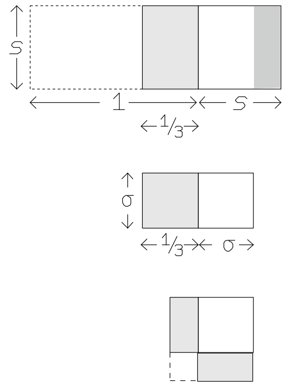
18 First published in [Thureau-Dangin 1936], here following the analysis in [Høyrup 2002a: 50−55]. For simplicity, I translate the sexagesimal place value numbers into modern fraction notation.

19 These diagrams are never drawn on the clay tablets, which would indeed not be adequate for deleting and redrawing. We may imagine them to be made on a dust-board (as were the diagrams of working Greek geometers) or on sand spread on the floor of the court-yard. The diagrams sometimes drawn on the tablet serve to make clear the situation described in the statement.

20 Whoever is so inclined may translate the procedure into algebraic symbols and thus discover that the steps agree with those that occur when we solve an equation $s^2+s = {}^3/_4$. This explains that the technique is commonly spoken of as “algebra” (even though better reasons can be given).

On the other hand, the transformations of the diagram correspond rather precisely to those used in *Elements* II.6, which explains why Euclid’s technique is sometimes spoken of as “geometric algebra”.

21 Translated into symbolic algebra, this corresponds to a change of variable.



The procedure used to solve BM 13901 #3.

The procedure of #1 can also be used to solve problems where a multiple of the side is subtracted from a square area, or where the difference between the sides of a rectangle is known together with its area. The problem where a rectangular area is known together with the sum of the sides is solved by a different but analogous procedure (the diagram of *Elements* II.5 gives the gist of it). Taken together, the two techniques permit the solution of all "mixed second-degree" problems about rectangular and square areas and sides. Moreover, the method allows, and was actually used for, *representation.* If (for example) a number of workers carrying bricks according to a fixed norm per day is identified with one side of a rectangle and the number of days they work with the other,[22] then the number of bricks they bring is a known multiple of the area. From the ratio between the number of workers and that of days together with the sum of workers, days and bricks (evidently another supra-utilitarian problem, never to be encountered in scribal real life), all three entities can thus be determined. In other problems we see lines representing areas or volumes (which allows the solution of bi- and bi-biquadratic problems), as well as inverse prices ("so and so much per shekel silver") or number pairs from the table of reciprocals.

The oldest text group where "algebraic" problems turn up is from the Eshnunna area (Eshnunna itself and a few neighbouring sites). Before it fell to Hammurabi, Eshnunna appears to have been the cultural centre for central and northern Mesopotamia – Mari at a certain point undertook a writing reform and adopted the orthography of Eshnunna [Durand 1997: II, 109; Michel 2008: 255], and Eshnunna produced what seems to have been the earliest Akkadian law code in the early 18th century.

The earliest problem text from Eshnunna, IM 55357 [Baqir 1950] from c. 1790, exhibits a problem format which is still rudimentary but none the less slightly more elaborate than what we know from Ur − the prescription is introduced by the phrase ZA.E AK.TA.ZU.UN.DÈ, "You, to know the proceeding". Beyond that, like one of the texts from Ur it asks the question "what" (in accusative) A.NA.ÀM, and it "sees" results – not using PÀD, however but IGI.DÙ, an unorthographic writing of IGI.DU$_8$, "to open the eye". All in all, it obviously shares some inspiration with the texts from Ur but does not descend from them.

This text makes heavy use of Sumerian word signs, even though the structure of phrases shows that they were meant to be read in Akkadian (corresponding to a reading of *"viz"* as "namely" and not as Latin *videlicet*). The other texts from the area (written

22 AO 8862 #7, ed. [MKT I, 112*f*].

between c. 1775 and 1765) are predominantly written in syllabic Akkadian. They make an effort to develop problem formats, but disagree on how this format should look. Clearly, they stand at the beginning of a tradition where conventions have not yet been settled.[23]

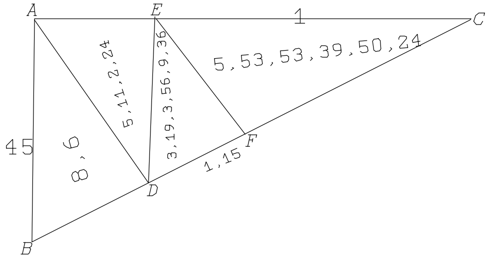


Diagram illustrating the problem of IM 55357 (as drawn on the tablet, numbers translated)

Some of the Eshnunna problems – for instance 10 problems solved on a tablet published in [al-Rawi & Roaf 1984], found in Tell Haddad (ancient Me-Turan) and most likely from c. 1775 – consider practical situations which already Ur III scribes would have had to deal with. But supra-utilitarian problems of various kinds dominate, many of them "algebraic". Largely overlapping with the latter category, many deal with artificial geometric questions. IM 55357, presented above, thus deals with the cutting-off from a right triangle of similar subtriangles. The ratio of the sides in all triangles is 3:4:5 – that is, they are Pythagorean, but that is not used; the solution applies the scaling operation, but has no use for the cut-and-paste technique. However, the text Db_2-146 [Baqir 1962], found at Tell Dhiba'i together with texts dated 1775, contains a partial quote of the "Pythagorean rule" (that is, the rule corresponding to the Pythagorean theorem) in abstract terms in its proof; the general rule (and not just the 3:4:5-triangle) was thus familiar. The question asked and solved in the latter text is to find the sides of a rectangle from the area and the diagonal.

Other supra-utilitarian geometric problems apply the "algebraic" technique to trapezia or triangles divided by parallel transversals. Beyond that, the "Tell Harmal compendium"[24] list a large number of "algebraic" statement types about squares (there are no prescriptions, and even the statements leave the numerical parameters undetermined). So, the "basic representation" for the "algebraic technique" was well known as such, even though the full problems only show it at work in more complex problems. On the other hand, *representation* never occurs in the Eshnunna texts.

Many of the problems open as riddles, "If somebody has asked you thus: …". This betrays one of the sources from which the "new" Old Babylonian school drew its "humanist" mathematics, namely the mathematical riddles of non-scribal mathematical

23 The texts belonging to this group were published singly or in smaller batches in journal articles. A preliminary treatment of all the texts from Tell Harmal (apart from the "Tell Harmal compendium") is given in [Gonçalves 2012].

24 Three badly broken tablets left on the ground as worthless by illegal diggers [Goetze 1951]; being found under such circumstances they are obviously undated, but certain aspects of the terminology shows them to be early [Høyrup 2002a: 324] – probably from the 1770s.

practitioners – not only surveyors (certainly the most important source, according to the statistics of surviving problems[25]) but also, it appears, travelling traders (the grain filling Eshnunna problem IM 53957[26] is obviously related to problem 37 from the Rhind Mathematical Papyrus – see [Høyrup 2002a: 321]).[27]

The finding of texts in elaborate problem format and often with intricate supra-utilitarian contents in several sites suggests that the Eshnunna region as a whole was the hotbed where the new type of mathematics developed. That this new type arose in continuity with and as a graft upon the heritage from Ur III is obvious: everything makes use of the place value system, and the Tell Harmal compendium contains long sections with technical constants.[28]

We may compare Eshnunna with Mari to its west (close enough to prompt repeated military conflict). Mari had never been part of the Ur III empire, though certainly kept for a while under political control. None the less, a batch of mathematical texts from between 1800 and 1758 [Soubeyran 1984] shows that many of the place-value techniques were adopted. We also find a reflection of the general interest in supra-utilitarian mathematical skills. One text, indeed (not written in problem format), calculates 30 consecutive doublings of a grain of barley. There can be no doubt that this is the first known version of the "chess-board problem" about continued doublings of a grain of

[25] Comparison with later sources allows us to identify a small set of surveyors' riddles that survived until the late first millennium CE or later, and left an impact on Greek ancient mathematics (theoretical as well as supposedly practical), in medieval Arabic surveying texts, and even in Jaina mathematics − see [Høyrup 2001; 2004] ⟦= articles I.3 and I.4⟧.

The earliest members of the set appear to have dealt with rectangles whose area was given together with (1) the length, (2) the width, (3) the sum of length and width, or (4) the difference between these. (1) and (2) were already adopted into the Sargonic school, as we remember. The trick to solve (3) and (4), the geometric quadratic completion, appears to have been discovered somewhere between 2200 and 1900, after which even they, and a few other simple problems that could be solved by means of the same trick (e.g., known sum of or difference between a square and the corresponding side, cf. above), were taken up by the Old Babylonian school, where they provided the starting point for far-reaching further developments.

[26] [Baqir 1951: 37], revision and interpretation [von Soden 1952: 52].

[27] The kinship only concerns *the question* − the solution in the Rhind Papyrus builds on orthodox and highly sophisticated use of the Egyptian unit fraction system and its algorithms, while the Eshnunna solution is no mathematical solution at all but a mock reckoning which takes advantage of the known result – a type which is not rare in collections of practitioners' riddles. Since these are meant to dumbfound the non-initiates and not supposed to represent a category of "mathematics", it is not strange that they may distort usual procedures as much as the riddle of the sphinx (even more clearly a "neck riddle") distorts the normal sense of words.

[28] Certain linguistic particularities may suggest direct continuity with pre-Ur-III mathematics, but local writing habits (in particular the frequent use of unorthographic Sumerograms) might also have induced a local transformation of the Ur-III heritage.

barley, widely circulating in subsequent millennia.[29] The diffusion in later times coincides with the Eurasian caravan trading network [Høyrup 1990: 74]; this, as well as the theme, indicates that the Mari scribes have borrowed it from a merchants', not a surveyors' environment.

Eshnunna was conquered and destroyed by Hammurabi (in contrast to texts on papyrus, vellum, palm leaves and paper, those written on clay are best conserved when the library burns). We know that the conqueror emulated the idea of the law code; whether he brought captive scholars to Babylon we do not know – the Bronze Age strata of that city are deeply borrowed under the remains of the first-millennium world city. What we do know is that another text group ("group 1" according to [Goetze 1945]) carrying all the characteristics of a tradition *in statu nascendi*[30] can be located in Larsa, and that one of the most important texts belonging to it (AO 8862) is very similar to another one containing tables of squares, inverse squares and inverse cubes and dated to 1749, both being written not on flats tablets but on clay prisms [Proust 2005, cf. Robson 2002].[31] Literary school texts from Larsa inculcating the ideal of scribal humanism written on similar prisms are dated to 1739.

The Larsa text group, though still in search for a definite style or canon, differs from the Eshnunna group in one important respect: it uses the "algebraic" area technique for *representation* – the above-mentioned problem about workers, working days and bricks comes from the prism AO 8862.

We do not know whether the Larsa tradition ever reached maturation; the texts we have were found by looters and acquired by museums on the antiquity market, and what looters find is of course rather accidental. In any case, the time for development will have been restricted – in 1720, as told above, the whole southern region seceded as the Sealand, and scholarly culture withered away.

Two text groups from Uruk (Goetze's "group 3" and "group 4") did reach maturity before the collapse. They betray no groping similar to what we find in the Larsa group, and are therefore likely to be somewhat later in time. Striking is, however, that while each of the groups is very homogeneous in its choice of canonical format and terminology, the two canons differ so strongly from each other that intentional mutual demarcation seems the only explanation [Høyrup 2002a: 333–337] – we may imagine two teachers or schools in competition, but there could be other reasons. None of them can descend from the other.

29 Until the diffusion of chess, other variants also have 30 doublings. After that, 30 and 64 compete.

30 Vacillating format, vacillating terminology, an experiment with an abstract formulation of a rule, etc. See [Høyrup 2002a: 337−345].

31 As mentioned in note 10, other mathematical texts − but exclusively arithmetical tables in Ur III tradition – from Larsa can be dated to c. 1815. This does not contradict the impression coming from the problem texts from c. 1749 that these represent the very beginning of a tradition.

Goetze's "group 2" [Høyrup 2002a: 345−349] can only be located unspecifically in "the south". It consists of texts containing either a long sequence of complete problems (statement+procedure) dealing with related topics (mostly right parallelepipedal "excavations" (KI.LÁ) or "small canals" (PA$_5$.SIG)[32]), or similar sequences of problem statements only. They show the impact of another characteristic of later Old Babylonian scribal scholarship: a quest for systematization.

All southern texts share one striking characteristic: they do not announce results as something "seen", even though an oblique reference in one text[33] and a few slips in another one[34] shows the idiom to have been familiar. An ellipsis in a kind of folkloristic quotation (BM 13901 #23) also shows that the "riddle introduction" "If somebody …" was known, but apart from this single instance it never appears in texts from the south. Instead, these state the situation directly in the first person singular, "I have done so and so", and the speaking voice is supposed to be the teacher (while the voice explaining what "you" should do is the "elder brother", an instructor well known from ideology-inculcating texts about the school).

Hammurabi is likely to have carried Eshnunna scribal culture to Babylon at the conquest, and chronology suggests that the new mathematical style of the south was sparked off by this migration of knowledge or scholars. However, the deliberate avoidance of two marks of northern and lay ways suggests that the schoolmasters of the south tried to demarcate themselves from the conquering Barbarians from Babylon.

Others, as we remember, were more efficient in demarcating the south from the Babylonian centre – so efficient that scholarly high culture appears to have disappeared from the south in 1720. A number of temple scholars are also known to have gone north already from 1740 onward. From 1720 onward, advanced mathematical activity is thus restricted to the northern part of the area.

Goetze assigns two text groups to the north. His "group 5" is too small to allow any conclusions – it consists of one complete and fairly well-preserved text, a fragment and a heavily damaged text. Group 6 is larger and much more informative. Already Goetze thought it might come from Sippar, a hypothesis which can now be considered well-established [Høyrup 2002a: 332, reporting Friberg; Robson 2008: 94]; the very homogene-

32 The text YBC 4612 [MCT, 103*f*] dealing with simple rectangle problems is written more coarsely than the indubitable members of the group, but it is likely to belong to the same family.

The textBM 13901, which supplied the two problems used above to demonstrate the "algebraic" techniques, was assigned to the group by Goetze for reasons which he himself characterized as insufficient and "circular" [Goetze 1945: 148 n. 354]. While remaining southern it must now be excluded from the group (and from the other established groups as well).

33 YBC 4608, "group 3", asks what to do "in order to see" a certain result.

34 YBC 4662, from "group 2", [MCT, 72]; three times, intermediate results are "seen" here, not "given" as in the rest of text (and the group as a whole).

ous core of the group can be dated to c. 1630. Goetze [1945: 151], at a moment when the Eshnunna texts were not yet known, supposed that the "6th group comprises northern modernizations of southern (Larsa) originals". This hypothesis can now be discarded. The texts of the group consistently "see" results, and other characteristics too shows it to descend from a "northern" mathematical culture of which the Eshnunna texts are the first known, and probably first, representatives.

But another text group may have been affected by the emigration of southern scholars, the so-called "series texts", written almost exclusively with logograms and therefore not considered in Goetze's orthographic analysis.[35] The texts consist of lists of statements only, or statements and solution. Often, long sequences of problems can be obtained from a single one by systematic variation on up to four points in "Cartesian product" (e.g., subtraction instead of addition, a denominator 19 instead of 7, length instead of width, a member being taken twice instead of once, see [Høyrup 2002a: 203]); the writing is utterly compact, often only the variation is mentioned, and in consequence the meaning of the single statement can only be grasped when those that precede are taken into account.

The style must be the endpoint of a long development, and the texts in question are thus likely to belong to the late phase, and already for that reason to be northern. Christine Proust [2010: 3, cf. 2009: 195] gives evidence that "the structure of the colophons might speak in favour of a connection between the mathematical series texts and a tradition which developed in Sippar at the end of the dynasty of Hammurabi" – more or less as the same time as "group 6" was produced in the same city. Much in the terminology excludes, however, that the series texts came out of the same school; Friberg [2000:172], moreover, has shown that the use of logograms is related to what can be found in groups "3" and "4". All in all, it seems plausible that the texts were produced within a tradition going back to scholars emigrated from the south ⟦though in interaction with the local environment⟧.

The last group to consider consists of texts that were excavated in Susa (published in [TMS]). Since the expedition leader did not care much about stratigraphy (cf. [Robson 1999: 19] and [MCT, 6. n.28]), we only know (and mainly from the writing style) that they are late Old Babylonian [TMS, 1]. Like "group 6", they clearly belong to the northern tradition first encountered in Eshnunna. On one hand, their results are always "seen". On the other, the two texts TMS V and TMS VI, lists of problem statements about squares, are clearly related in style both to the Tell Harmal compendium and to the two texts CBS

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35 The term was introduced by Otto Neugebauer [1934: 192 and *passim;* MKT I, 383*f*]. In [MCT], he and Abraham Sachs discarded the term because the Old Babylonian mathematical series do not correspond to the ideal picture one had at the moment of canonical series from later times (Neugebauer had not compared the two types when he introduced the term, just observed that the single tablets were numbered as members of a series). Since other Old Babylonian series are no more canonized, there are good reasons to retain the term. Cf. [Proust 2009: 167–169, 195].

43 and CBS 154+921 [Robson 2000: 39*f*], possibly from Sippar (Eleanor Robson, personal communication).[36] The latter texts, like the Susa catalogues and in contradistinction to the Tell Harmal compendium, specify numerical parameters, but all refer to the side of a square as its "length" (UŠ).

The Susa group contains some very intricate problems. For instance, TMS XIX #2 determines the sides of a rectangle from its diagonal and the area of another rectangle, whose sides are, respectively, the (geometrical) cube on the length of the original rectangle and its diagonal. This is a bi-biquadratic problem that is solved correctly.[37] Even more remarkable are perhaps some texts that contain detailed didactical explanations (the above-mentioned term for a coefficient, "as much as there is of it", comes from these).[38] They make explicit what was elsewhere only explicated orally (sometimes with set-offs in the prescriptions which only comparison with the Susa texts allow us to decode). We may assume that the peripheral situation of Susa invited to make explicit what was elsewhere taken for granted.

The end

In my part of the world, the most beautiful moments of summer may occur in September. However, they are invariably followed by real autumn, and then by winter.

Not so for the Indian summer of Old Babylonian mathematics. Very shortly after the bloom represented by the series and Susa texts, winter set in directly. After the Hittite raid in 1595 and during the ensuing collapse of the Old Babylonian political system, even "humanist" scribal culture disappeared; or at least, what survived as its textual vestiges within the scholarly "scribal families" encompassed literature, myth, ritual, and omen science − but not mathematics.

Shortly after the Kassite take-over in Babylonia, there was also a dynastic change in Elam, to which Susa belonged. The details and even the precise moment are not clear [Potts 1999: 188*f*], but in any case the Babylonian cultural influence (and *a fortiori* the influence of the Old Babylonian cultural complex, already extinct in its homeland) was

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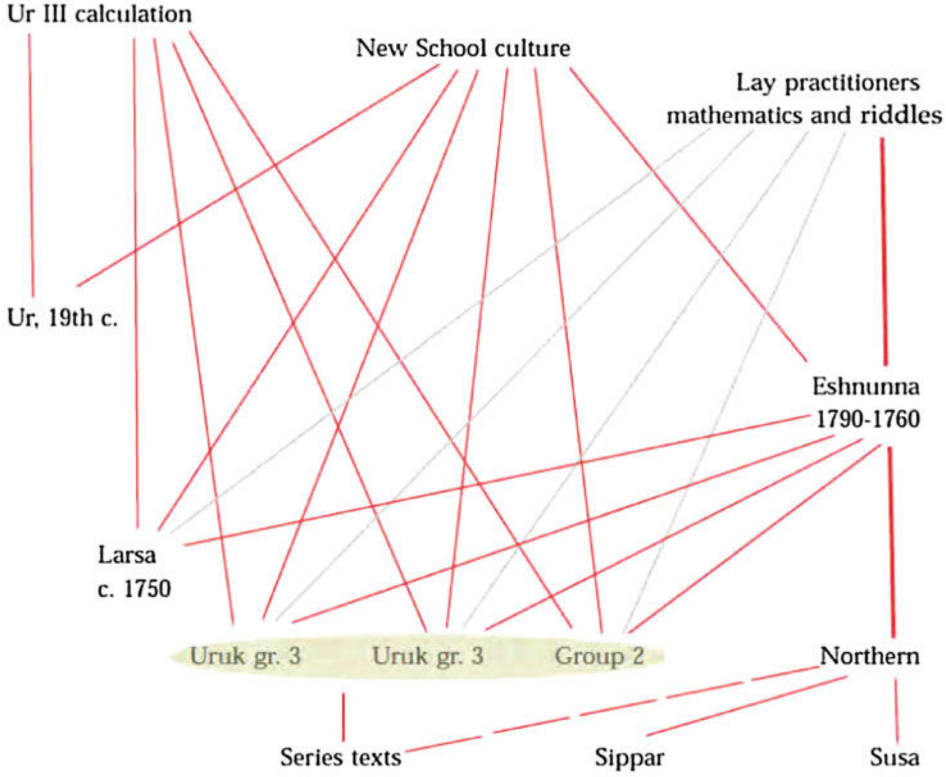
[36] This assumption is supported by the way square sides are asked for, "how much, each, stands against itself?", which is routinely used in "group VI". However, the folkloristic quotation in BM 13901 #23 also refers to a square side as what is "standing against itself', so the phrase may belong to the parlance of Akkadian lay surveyors (which would still suggest the texts to be northern but nor necessarily from Sippar).

[37] See [Høyrup 2002a: 197−199]. The solution is only correct *in principle*. There are indeed some numerical errors due to the misplacement of counters on the reckoning board [Høyrup 2002b: 196*f*] ⟦= article I.2⟧, but since square root extractions are made from the known end result, these mistakes are automatically eliminated.

[38] See [Høyrup 2002a: 85−95] [see also article II.3⟧.



strongly reduced. Even here, the text group just discussed marks a high point as well as the end.

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Chapter 26 (Article II.9) Written Mathematical Traditions in Ancient Mesopotamia: Knowledge, Ignorance, and Reasonable Guesses

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Abstract

Writing, as well as various mathematical techniques, were created in proto-literate Uruk in order to serve accounting, and Mesopotamian mathematics as we know it was always expressed in writing. In so far, *mathematics generically regarded* was always part of the *generic written tradition.*

However, once we move away from the generic perspective, things become much less easy. If we look at basic numeracy from Uruk IV until Ur III, it *is* possible to point to continuity and thus to a "tradition", and also if we look at place-value practical computation from Ur III onward – but already the relation of the latter tradition to type of writing after the Old Babylonian period is not well elucidated by the sources.

Much worse, however, is the situation if we consider the sophisticated mathematics created during the Old Babylonian period. Its connection to the school institution and the new literate style of the period is indubitable; but we find no continuation similar to that descending from Old Babylonian beginnings in fields like medicine and extispicy. Still worse, if we look closer at the Old Babylonian material, we seem to be confronted with a small swarm of attempts to *create* traditions, but all rather short-lived. The few mathematical texts from the Late Babylonian (including the Seleucid) period also seem to illustrate attempts to *establish* norms rather than to be witnesses of a survival lasting sufficiently long to allow us to speak of "traditions".

Contents

PETER DAMEROW
in memoriam
1939–2011

On ignorance and limited knowledge

In Neugebauer's *Vorgriechische Mathematik* from [1934: 204] we find this warning:

> Unser Textmaterial der babylonischen Mathematik ist im ganzen noch viel zu lückenhaft. Es ist gewiß methodisch nicht richtig, die Texte, die wir besitzen, kurzerhand als etwas Einheitliches zu betrachten. Jeder Text (oder jede Textgruppe) hat seine bestimmte Absicht. Wenn der eine sich mit gewissen g e o m e t r i s c h e n Dingen beschäftigt, so darf man daraus nicht unmittelbar auf die allgemeine Methode schließen, die für gewisse n u m e r i s c h e Fragen, etwa Wurzelapproximationen, angewandt worden ist. So kann also die Voraussetzung, die gewissen Textgruppen zugrunde liegt, ganz anders sein als die von anderen Typen.
>
> Man darf bei allen diesen Fragen nicht vergessen, daß wir über die ganze Stellung der babylonischen Mathematik im Rahmen der Gesamtkultur praktisch noch gar nichts wissen. ⟦[1]⟧

When this was written, practically nothing was known about anything but the mathematics of the Old Babylonian and the Seleucid periods. Since then we have learned much about the mathematics of the late fourth and the third millennium BCE, and further something about that of pre-Seleucid Late Babylonian times. We have also come to know a number of geographically localized text groups from the Old Babylonian period, and are now able to distinguish text groups from this time in a way which Neugebauer could only adumbrate.[2]

Sadly (for a discussion about *traditions*), this has only provided us with a larger number of islands in a vast ocean. At times they seem to form a chain, and as in the case

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[1] ⟦In general, our text material for Babylonian mathematics is still all too incomplete. It is certainly not methodically justified to consider the texts we possess right away as homogeneous. Each text (or each text group) has is own specific purpose. Even if one deals with specific geometric matters, one should not directly derive from it a general method that was applied to certain numerical questions, for example root approximations. In the same vein, presuppositions underlying particular text groups may also be wholly different from those other text types are based on.

Concerning all these questions we should not forget that so far we know practically nothing about how Babylonian mathematics was situated within the framework of general culture.⟧

[2] What Neugebauer did in [1932: 6*f*] was to propose a division of the Old Babylonian material known by then into two groups, represented respectively by the Strasbourg texts and the CT IX-texts (Louvre). He further suggested the former to be slightly older and the latter slightly younger, and even that the Strasbourg texts are from Uruk, and that AO 8862, though not properly a member of the Strasbourg group, is still likely to be related to it. Everything agrees with the best knowledge of today!

of the Aleutian Islands we may assume that they are connected by a submersed mountain ridge; but others stand out in isolation, and even when connections can be suspected, their precise nature (oral/written/…) and geographic location (transmission within Mesopotamia or through peripheral areas) remains hypothetical.

Unless we accept indirect evidence, Neugebauer's second paragraph remains almost as true today as when it was written. I shall therefore *not* restrict myself to written traditions, since we often do not known whether a particular document class is really an expression of a generally written practice or only an accidentally written reflection of a non-literate though certainly numerate culture – and in the former case, whether this practice belonged to an environment of scholar-scribes or less educated people. Of course, I shall try when possible to decide in each case what is the situation – but hopefully not be illuded and go much beyond that.

Elementary numeracy and literacy

During Uruk IV, around 3200 BCE[3], writing was created as a means for accounting, and for no other purpose. Accounting needs also gave rise to the development of metrologies with fixed numerical proportions between units and – in the case of length and area metrologies – geared to each other ⟦see article II.6⟧. From the beginning, basic mathematics – numeration, metrologies, and fundamental calculation – was thus not only part but an essential constituent of the Mesopotamian written tradition.[4]

Part of this tradition did not survive the proto-literate period – or at least did not make it into Early Dynastic III (2600–2350 BCE), the next period from which we possess numerate documents. The bisexagesimal system disappeared – it had served for counting bread or grain rations, perhaps also for portions of dairy products, so changes in bureaucratic procedures is a likely explanation; the "grain system" was reshaped, different city states having different factor sequences; and with some exceptions, the markings that indicate the kind of good being measured (barley, malted barley, etc.) vanished. Other systems survived – the area system and its underlying length metrology, and an administrative calendar where each month is counted as 30 days and each 12 months as a year, serving in the distribution of fodder (it was to be used again in Ur III, now also for distribution of rations and calculation of labour obligations, see [Englund 1988]); most important of all, the absolute-value sexagesimal counting system persisted – gradually, the curviform shapes were replaced by cuneiform versions, but for long the two were used side by side, and there is no doubt about its continuous existence. Hypothetically, even the proto-literate notion of fractions can be supposed to have been transformed, not

3 Here and in the following I use the "middle chronology".

4 A convenient summary can be found in [Nissen, Damerow & Englund 1993], which also deals with important aspects of the development until Ur III.

replaced – the phrase used from Early Dynastic III onward when fractional notations turn up again, i g i *n* g̃ á l, might mean something like "*n* (dots) placed in eye (i.e., circle)", which would be a description of the proto-literate notation.[5] All in all, basic mathematics survived as part of the same tradition as the lexical lists, with a similar amount of transformation in continuity.

Where Sumerian was not or no longer the administrative language – for instance, in Old Babylonian and later Babylonia and Assyria – we still find the system, but now coupled with Akkadian number words for one hundred (*mē*) and one thousand (*līmum*). The mathematical tradition – as could be expected – could not be totally stable when the habits of the environment where it served were different or changing.[6]

The place-value system and complex

During Ur III, probably in the wake of Shulgi's administrative reform (2075 BCE),[7] the place-value notation for intermediate calculation was introduced together with the whole spectrum of tools without which it would be useless: tables for metrological conversion (and the "metrological lists", didactical preliminaries to the metrological tables); tables of technical constants; tables of reciprocals; and multiplication tables. And, not least, a training system which was also a *sine qua non* for the functioning of the techniques.[8]

Our evidence that the whole complex goes back to Ur III is indirect but compelling: as Eleanor Robson [1999: 182] has shown, some of the technical constants taught in the Old Babylonian school had gone out of use after Ur III. But from Old Babylonian Nippur

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5 This connection – the only plausible one ever advanced – was first proposed as a possibility by Jöran Friberg [1978: 45], though with an erroneous (and less adequate) interpretation of g̃ á l as "to open".

The occasional Old Babylonian interpretation (i g i ∼ *pani,* "in front of", namely in the table of reciprocals), found for instance in Haddad 104 [al-Rawi & Roaf 1984: 22] (also proposed by E. M. Bruins [1971: 240]) is certainly a mistaken folk etymology. The phrase was used in Lagash around 2400 BCE [Bauer 1967: 508–511; Lambert 1953: 60, 105*f*, 108, 110; Allotte de la Fuÿe 1915: 132], preceding the creation of tables of reciprocals by more than 300 years.

6 The numerate culture of Assyria being already in the Old Assyrian period on the whole rather different from what we know from central and southern Mesopotamia, I shall only refer in the following to Assyrian material on a single occasion.

7 Since the system was used for intermediate calculation, never surviving, and since texts containing only numbers are difficult to date palaeographically, the Ur III date was only indirectly attested until recently; some years ago, however, Eleanor Robson [personal communication] discovered tables of reciprocals found in dated contexts, which definitively settles the matter.

Long before that, small stylistic differences allowed to distinguish older (presumably Ur III) from normal Old Babylonian specimens; see [Oelsner 2001] and [Steinkeller 1979].

8 We are thus confronted with a whole *technical system,* like those created in recent centuries [Mayntz & Hughes 1988] but rarely before, social as well as technological.

we have direct indications of how the complex was taught as a coherent curriculum [Robson 2002a; Proust 2008].

We also have evidence – though not in detail – that the complex spread (at least in part) as a constituent of the scribal curriculum to regions that had only been submitted for a shorter period (Eshnunna) or not at all (Mari) to Ur III,[9]. Even after the Old Babylonian period, we find traces outside the Babylonian area. Of particular interest is Assurbanipal's assertion [Ungnad 1917: 41*f*, revised interpretation] that he is able to "find reciprocals and make difficult multiplications", which shows that the scholar-scribes of his times (these, indeed, must be the ones who had inspired his literate pretensions) kept the tradition alive – whether in genuine continuity or as part of the same antiquarian interest which sometimes made them emulate the script of the mid-third millennium, which the king claims to understand in the same text [Fincke 2003: 111].

Late Babylonian (fifth-century as well as Seleucid) mathematical texts produced within the environment of scholar-scribes, though insufficient in number to let us know much about traditions at a higher mathematical level (see below), also show that the place-value system and the use of reciprocals were still alive there (as does mathematical astronomy).

What Marvin Powell [1990: 458] calls the "standard (scientific) system" of metrology was largely present already in Shuruppak, to some extent already in proto-literate Uruk; during the Sargonic epoch it underwent some regularization, to be ultimately stabilized by becoming part of the place-value complex during Ur III. The very purpose of that complex was indeed to harmonize the metrological system with the principle of sexagesimal place-value computation; integration of a change in the factor structure of a metrology would only be possible if new metrological lists and tables were created. During the Kassite period new measures arose, but these were never integrated systematically – Jöran Friberg's survey [1993] of known texts from the later period somehow related to metrological tables show this. Some of them refer to the traditional system, which was thus still, to an extent that cannot be precisely determined, part of the tradition carrying sexagesimal computation; others include some of the new sequences, in a format which reflects the idea of a metrological table but is hardly thought of as an aid to intermediate

9 Eshnunna broke loose in 2025; interestingly, texts from Eshnunna (to be dated c. 1775 BCE) often use deviant ("unorthographic", that is, phonetic) spellings of í b . s i $_8$. They also often use b a . s i referring to a square; this usage (though written orthographically, as b a . s i $_8$) is also found in texts from 19th-century Ur, while other Old Babylonian texts only use it about a cube – see [Høyrup 2002a: 253].

Mari had never been directly subjugated, though certainly for a while under Ur III influence; this, however, is a type of political bond which would not automatically entail adoption of administrative or scribal techniques; that tables of reciprocals belonging to the earlier decades of the 18th century are none the less found in the palace archives from Mari is thus evidence of a deliberate adoption of the system – parallel to, but not necessarily concomitant with Samsî-Addu's adoption of Eshnunna orthography and syntax in the Kingdon of Upper Mesopotamia.

calculation (at least not place-value computation) – one [Friberg 1993: 391], probably of Late Babylonian date, for instance, expresses "the successive units of length [...] as multiples of one or two of the nearest smaller unit". As Marvin Powell [1990: 469] argues from scattered occurrences in non-mathematical contexts, the new units were probably "more widely used that our sparse evidence suggests".[10] The Late Babylonian scholar-scribes, when taking up interest in basic (and sometimes less basic) mathematics, probably combined whatever was still handed down from the "scientific" system with what was actually used in the world around them, producing something which was neither really the tradition nor a faithful representation of what was done by those who measured and counted professionally.

Since later periods did not resuscitate the centralized meticulous Ur III accounting structure, there was of course no motive for refashioning a system that had been created as a tool for this structure.

Area computation

As mentioned, the proto-literate area metrology was geared to the length system, and rectangular areas were determined correspondingly, as product of length and width (where we have no indication of the conceptualization of "multiplication" as an arithmetical operation before Ur III). One model document (that is, a teaching text shaped as a real administrative document) shows that approximately rectangular areas were determined by the "surveyors' formula", as average length times average width [Damerow & Englund 1987: 155 n. 73].

This latter tablet must have served teaching, and we can thus safely presume that the very restricted circle of manager-priest were caring for such matters, which in consequence were part of the incipient written tradition (whether there was any specialization we cannot know); already in Shuruppak, however, surveying and scribal management were no longer fully coincident, and specialization within the scribal profession appears to have taken place – one contract about the sale of a house [Visicato & Westenholz 2002: 2], for instance, refers to the u m . m i . a l ú . é . e š . g a r, "the schoolmaster who measured the house" ⟦distinct thus from the scribe who stipulated the contract⟧; this professional figure is also present in many other contracts, as is the d u b . s a r . a š a $_5$, "surveyor-scribe" [Visicato 2000: 22–25 and *passim*].

At least in Old Babylonian times, surveying appears to have been to some extent incumbent on a "lay", that is, non-scribal profession. However, surveying also remained part of the scribal curriculum for long. Firstly, the Sargonic school texts that have been

[10] Some fifth-century BCE "sophisticated" texts to which we shall return, which combine "scientific" length metrology with seed measure also show that no genuine integration has been achieved – instead of using the methods connected to the place-value complex they translate by means of a technical constant [Friberg 1997: 260].

identified all deal with (mostly rectangular) areas and their sides [Foster & Robson 2004]; like a grain distribution problem from Shuruppak,[11] their question is invariably marked by the possessive suffix . b i , "its [area, etc.]". Secondly, the same mark (completed however now with the pseudo-Sumerogram e n. n a m) is sometimes found in Old Babylonian school tablets from Nippur in which square areas are determined – examples in [Proust 2008]: 181, 183].[12] It seems reasonable to assume continuity within the scribal educational tradition.

But the school tradition cannot have been the only carrier of agrimensorial calculation between the Sargonic and the Old Babylonian period. This follows *inter alia* from other aspects of the way to ask or answer the question. In many of the Sargonic texts, results are either *seen* or *to be seen* (using p à d or the unorthographic p a d) [Foster & Robson 2004: 6]. The same term is used in many of the texts from 19th-century Ur [Friberg 2000], cf. below, but never afterwards in any Old Babylonian text we know about. Instead, the texts from early 18th-century Eshnunna (and later texts from the periphery, not least Sippar and Susa) use Akkadian *tammar,* "you see", when announcing results. A few Old Babylonian texts from the periphery use a new (and not very adequate) Sumerographic writing;[13] texts from the southern former Sumerian core avoid the expression consistently, but a slip in the text YBC 4608, probably from Uruk, shows it to have been known.[14] This suggests (and other evidence corroborates the suspicion) that a lay environment of Akkadian-speaking surveyors was also engaged in area computation (conceivably only in the Akkadian part of "Sumer and Akkad"); that it used the idiom of "seeing" results; and that this idiom was adopted by the school tradition in the periphery while being known but mostly avoided in the south. Since the texts from 19th-century Ur never use . b i to indicate questions we may presume that its use of p à d was also no intra-school heritage from the Sargonic period but a translation from Akkadian (after all, it is the regular Sumerian translation, better indeed than i g i . d u $_8$ – not to speak of i g i . d ù).

As already hinted at in the discussion of metrologies, the end of the Old Babylonian period probably deepened the split between the scholar-scribes taught in scribal families and those who "measured and counted professionally"; those of the latter who measured land were probably responsible for the area metrologies created in Neobabylonian times

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11 A granary of 40·60 g u r . m a ḫ, each of 8·60 s i l a , of which "each man" receives 7 s i l a. The question is formulated "Its men". See [Høyrup 1982].

12 On p. 194, the same phrase is used in a problem about the weight of a brick.

13 IM 55357, the earliest text from Eshnunna, uses i g i. d ù, an unorthographic writing of i g i . d u $_8$. The latter spelling is used in the probably late Old Babylonian "series texts" YBC 4669 and YBC 4673.

14 It asks what to do *aš-šu* X *a-ma-ri-i-ka,* "in order to see [i.e., find] *X*".

The preceding discussion draws on [Høyrup 2002a: 319–361, *passim*].

[Powell 1990: 482–483]: the “reed measure” based on “broad lines” and thus allowing the measurement of areas in length units[15], and the two slightly different “seed measures”, measuring land in terms of the amount of seed needed to plant it and to feed the plough oxen; the modes of thought inherent in broad lines as well as seed measures are those of people engaged in real surveying and agricultural management, not of scholars producing the counterpart of the “rational mechanics” of more recent times.[16]

The sophisticated level: “Babylonian mathematics”

What is spoken of in general histories of mathematics as “Babylonian mathematics”, and what together with the arithmetical tables belonging with the place value system occupies almost all space in the famous source editions on which general histories are ultimately based – MKT, TMB, MCT, TMS – is the sophisticated mathematics of the Old Babylonian period, together with a few texts of a similar kind from the Seleucid era.

In the general histories, all of this is treated as one homogeneous body; the text editions, on their part, seem to suggest that at least the Old Babylonian material is homogeneous (apart from Evert Bruins’ unfounded claim that the Susa texts distinguish between the Susian and the Akkadian methods – see [Høyrup 2002a: 98 n. 128]).

Actually, MKT and MCT are more perceptive. As pointed out in note 2, Neugebauer had already suggested a separation of the material into two groups in 1932 (corresponding to my preceding distinction between texts from the periphery and from the core), and this was carried over to MKT. MCT contains a whole chapter written by Albrecht Goetze [1945], in which he divides the Old Babylonian corpus as known by then into six groups, purportedly on the basis of Akkadian orthography but in fact also from considerations of vocabulary.

More systematic investigation of the terminology and phraseology has confirmed Goetze’s classification, moving only a few dubious texts from one group to another one (and dividing a group which even Goetze had difficulty in seeing as being really a group). Beyond that, several groups of texts found *in situ* (though sometimes badly excavated) and not on the antiquity or black market have been added. The situation as it looks now is described in [Høyrup 2002a: 319–361], on which I shall draw heavily in the following.

In an introduction to a discussion of the shaping of extispicy as a literary form Seth Richardson [2010: 225] writes that

15 On the notion of “broad lines” and its widespread occurrence in pre-modern practical metrologies, see [Høyrup 1995] ⟦translated as article I.7⟧.

16 The “scientific system” measures volumes in terms of “thick surfaces” provided with a default height of 1 cubit, but “broad lines” are only visible in certain substructures of Old Babylonian mathematics – in particular the use of *našûm,* “to raise”, for the multiplication involved in area calculation, suggesting an operation of proportionality.

> The Old Babylonian period [...] was a time in which many third millennium cultural forms were being transformed by programmatic revision and political appropriation in the contest to restore geopolitical equilibrium,

This appears to be also relevant for mathematics. The small lot of mathematical texts from (probably) 19th-century Ur mentioned above looks as evidence for the beginnings of the process. Most of the texts are elementary number exercises – four of them, as Friberg [2000: 147*f*] observes, seemingly coming from a small private school teaching only part of the classical curriculum. But there are a few genuine problems. None of them correspond to the favourite types from the mature Old Babylonian period,[17] but they are interesting because they are in a rudimentary problem *format,* which appears to have been absent from the mathematical curriculum of the Ur III period.[18] The question may be made explicit (depending on grammatical case by the regular Sumerian a . n a . à m or by the pseudo-Sumerogram e n . n a m); a few times results are "seen" (p à d or p a d).[19]

In a general sense, these texts seem to inaugurate a "tradition" of mathematical problems. However, everything specific is so different from what turns up elsewhere in the Old Babylonian record that it is preferable to see them as an early expression of a "mood" or "culture" characterizing Old Babylonian school mathematics; it appears that the 19th-century Ur expression of this mood left no traces in the later record, and thus did not give rise to (or participate in) a genuine tradition. Its interest lays in its way to show how the general mood could express itself in a reshaping of Ur III mathematics.

Another lot of mathematical texts, published by Denis Soubeyran [1984], is from the palace archive of early 18th-century Mari. It mainly consists of arithmetical tables,[20]

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17 Particularly striking is a problem about the bisection of a trapezium by a parallel transversal (UET V, 858, [Friberg 2000: 142]), a problem whose correct solution goes back to Sargonic times [Friberg 1990: 541], and which has a certain family connection with the "algebra" of the following centuries. In the present case, the ratio in which the sides has to be divided is taken to be given, for which reason the solution becomes trivial.

This is not the place to take up the discussion whether Old Babylonian "algebra" was "an algebra" or not, the answer to which will anyhow depend on definitions; see, for instance, [Høyrup 2002a: 278–282] or [Høyrup 2010: 103–106]. For the sake of simplicity, I shall refer in the remainder of the present article to the technique dealing with square and rectangular areas and their sides (as well as its extensions) as "algebra", retaining the quotes.

18 The evidence for this is complex, coming mostly from the presence/absence of Sumerographic writings for terms for operations and terms structuring the format in Old Babylonian mathematical texts. See [Høyrup 2002c] ⟦= article II.7⟧.

19 Except for the appearance of a few Akkadian loan words, the texts are written in grammatical Sumerian – but so grammatical that they seem to be written "grammar book in hand": grammatical elements are not always contracted as they would be in regular Sumerian writing (thus ù . u b . instead of u b . , cf. [Thomsen 1984: 208]).

20 Some of Soubeyran's texts seem not to be mathematical at all. One, for instance (pp. 41–45),

but one text (pp. 30–35; seen as a an exponential table by Soubeyran) is of a different kind: an early version of the "chess-board problem" about continued doublings of a grain of barley. There is no hint of a problem format, only the mere calculation; but there is no doubt that the text deals with the well-known and widely circulating problem; it has 30 steps, as was the standard until the spread of familiarity with the chess-board game (after which 30 and 64 were competing).[21] The appearance of this problem is thus another expression of a new mood in the school, and an example of how this mood led to the adoption of circulating mathematical riddles and "recreational" problems.

The mathematical texts from Eshnunna (Tell Harmal, Tell Dhiba'i, Tell Haddad) are much more informative. With the possible but unlikely exception of the undated "Tell Harmal compendium" [Goetze 1951], the earliest mathematical text from the region is IM 55357 [Baqir 1950a] from c. 1790 BCE. It deals with the subdivision of a triangle with sides 45′, 1 and 1°15′ into triangles that are similar to it. The choice of parameters is strong evidence that the author was aware of the "Pythagorean rule", at least for these proportions, but the rule is not made use of in the solution. For our purpose, it is perhaps more interesting that we have a rudimentary indication of format: after the presentation of the data follows an explicit question, and the prescription is introduced by the phrase z a . e a k . t a . z u . u n . d è , "You, to know the proceeding". The writing makes heavy use of logograms, and shares one peculiarity with one of the texts from Ur – namely the use of a. n a. à m for the accusative of the question "what" (the nominative is a syllabic *minûm,* whereas the texts from Ur have e n . n a m). The outcome of calculations are "seen", but the term employed is i g i. d ù, not p à d. The use of a . n a . à m, though not present in other mathematical texts I remember, is therefore not necessarily evidence of any specific link to the Ur group. Because of the predominance of logograms, we cannot ascertain to which extent the later change of grammatical person[22] was intended.

The remaining Eshnunna texts [Baqir 1950b; Baqir 1951; Baqir 1962; al-Rawi & Roaf 1984] date from c. 1775 BCE. They span much of the thematic spectrum known from later

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deals with the loss of weight of various amounts of precious metal during refinement, not according to expectation but apparently in material processes – the amounts do not form an ordered list, and the relative loss changes from case to case.

[21] A papyrus from Roman Egypt [ed. Boyaval 1971] thus has 30 steps; the *Propositiones ad acuendos iuvenes,* a Carolingian problem collection, also has 30 [ed. Folkerts 1978: 51*f*]; al-Uqlīdisī, Damascus, CE 952/53 [ed. Saidan 1978: 337], states that "many people ask […] about doubling one 30 times, and others ask about doubling it 64 times".

[22] That is: Statement in the first person singular, past tense; prescription in the imperative or the second person singular, present tense, occasionally with references to the statement as what "he" has said. The implied voices are thus those of the teacher and the instructor – the š e š . g a l , "big brother", of edubba texts [Kramer 1949: 209 n.187 and *passim*].

Old Babylonian mathematics, and are characterized by more elaborate problem formats that the text just discussed.

With minor variations depending on exact context, the majority (the ten texts published in [Baqir 1951] and the one in [Baqir 1962]) start "If somebody asks you thus", after which follows the statement in the first person singular, "I have done so and so". This is not the format known from later texts (see note 22) but that of a riddle. This connection to non-school riddle traditions is confirmed by one of the problems, namely IM 53957 ([Baqir 1951: 37], corrections and interpretation [von Soden 1952: 52]):

> If [somebody] asks (you) thus: To $^2/_3$ of my $^2/_3$ I have appended 100 s i l à and my $^2/_3$, 1 g u r was completed. The *tallum*-vessel of my grain corresponding to what?

Problem 37 of the Rhind Mathematical Papyrus [trans. Chace et al. 1929: Plate 59] instead runs as follows:

> Go down I [a jug of unknown capacity – JH] times 3 into the *ḥeḳat*-measure, $^1/_3$ of me is added to me, $^1/_3$ of $^1/_3$ of me is added to me, $^1/_9$ of me is added to me; return I, filled am I [actually the *ḥeḳat*-measure, not the jug – JH]. Then what says it?

The affinities are too numerous to be accidental. Firstly, we notice the shared use of an ascending continued fraction; in the rich Egyptian record of texts using fractions, RMP #37 appears to contain the only ascending continued fraction ($^1/_3$ and $^1/_3$ of $^1/_3$) occurring at all.[23] Secondly, there are the details of the topic: an unknown measure which is to be found from the process, the reference to a standard unit of capacity, and the notion of filling.

The Rhind papyrus *solution* proceeds in agreement with the normal ways of Egyptian arithmetic, making elaborate use of the system of aliquot parts and the appurtenant "red auxiliary numbers". The Eshnunna solution, on the contrary, is a mock solution, a sequence of operations which only yield the correct result because the solution has been presupposed. It is nothing but a challenge meant to impress and make fools of the non-initiate and teaches no useful mathematical procedure. In other words, it is a genuine riddle posing as a mathematical riddle – a type which also turns up in other sources drawing on oral or semi-oral practitioners' traditions.[24]

We thus have good evidence that the creators of the Old Babylonian school tradition did in mathematics as the diviners had done in their field (according to Seth Richardson): borrowing from oral practices, and putting into order. The similarity with divination is

[23] In Semitic languages, Akkadian as well as Arabic, it is instead a standard way to express difficult fractions, see [Høyrup 1990] ⟦= article I.1⟧.

[24] One example, contained in the Carolingian *Propositiones ad acuendos iuvenes* [ed. Folkerts 1978: 47*f*], explains how two merchants selling swines at the same price as they bought them for make a profit all the same.

also (though only superficially) reflected in the language. Taha Baqir explains [1951: 29] that

> In a preliminary classification, these tablets and some others which will be dealt with in coming issues of "Sumer", were wrongly labelled as, "probably religious or omen texts", probably because they start with the phrase,"shumma ishalka" etc.[25]

The filling problem and the continued doublings from Mari may have been adopted from a merchants' environment – the presence of the same problem structure in Eshnunna and Pharaonic Egypt suggest travelling merchants. Much more important than these, however, are problems that refer to surveyors' practice: problems dealing with rectangles, trapezia, and measuring reeds that break.

Not all the Eshnunna texts are derived from riddles or formulated as riddles (not the same thing, formats may be borrowed). The long text Haddad 104 published in [al-Rawi & Roaf 1984] mostly contains rules and problems falling within the range of Ur III scribal calculation (capacity of containers, quantity of labour needed for a specified piece of work, etc.). The format here is similar to that of the early triangle division IM 55357, but in syllabic Akkadian and more elaborate: grammatically neutral explanation of the situation (though at times preceded by *nēpeš,* "procedure of", or, if a variant is concerned, by *šumma,* meaning "if [instead]"); and prescription preceded by *atta ina epēšika,* "you, by your making". Mostly, the prescription closes by *kīam nēpešum,* "thus the procedure".

The effort to develop the problem format can also be seen in the texts published in [Baqir 1951]. Nine of these ten texts were found in the same room in a private house,[26] and the tenth in the immediate vicinity; one of them is the mock filling calculation mentioned above, and all are in riddle format "if somebody …". Prescriptions open with the phrase *atta ina epēšika;* closing phrases are absent.

The ten texts have other characteristics in common, several of which are not even shared with other texts from Eshnunna. Results may either be "seen" or "come up" (*elûm*); in the latter case, they are invariably asked for by the word *minûm,* in the former always with the phrase *kī masi,* "corresponding to what"; only the tablet not found in the same room as the other nine uses both. Length and width of rectangles occurring in "algebraic" problems are invariably written with the logograms u š and s a g̃ , never with grammatical

[25] It should be observed, however, that the opening *šumma*, standard in legal, divinatory and medical texts, only characterizes a subset of the mathematical problems. The similarity concerns a style which in mathematics was neither compulsory nor connected by necessity to substance; it is not evidence of properly parallel intellectual endeavours.

[26] A complete list of the texts found in this room (and of whatever else may have been found) could be our first hint of the social setting of Old Babylonian sophisticated mathematics – which, though written in "school format", appears not to have been part of the normal scribal curriculum as we know it from Nippur and as analyzed by Christine Proust [2008].

or phonetic complements;[27] if real distances are meant (including the dimensions of a field measured by a reed which breaks), the writing is phonetic, as *šiddum* and *pūtum.* The "logical particles" *aššum* ("since"), *inūma* ("as") and *šumma* ("if') are absent (except for the appearance of the latter word in "if somebody"). The plane "equalside" (the square parametrized by its side) is always treated as a verb ("what is equal"), and always appears in unorthographic (or rather, phonetic) writing as i b . s i or i b . s e . e . The cubic equalside, on the other hand, is i b . s i $_8$ (still a verb) the only time it appears. Subtraction by removal is usually *ḫarāṣum,* "to cut off', a term apparently without Sumerographic equivalent in the mathematical texts.[28]

The text Db$_2$-146 [Baqir 1962] has much in common with these ten texts, not least the riddle introduction.

Looking at the whole Eshnunna corpus we find, firstly, outspoken efforts to create terminological and structural uniformity; secondly, that authors even a few kilometres and at most a decade apart did not agree on *how* this uniformity should look.

Eshnunna was conquered by Hammurapi in 1761 BCE, after which we know about no more mathematical texts from the area. The beginning of sophisticated mathematics in the south may perhaps be dated shortly *after* this event. In any case, the prism AO 8862, according to internal criteria probably an early exponent of this development[29] is almost certainly from the same place and approximately the same time as a prism carrying tables of squares, inverse squares and inverse cubes which was written in Larsa in 1749 BCE [Robson 2002b].[30]

If this dating (of the prism, and of the beginning of sophisticated mathematics in the south) is correct, it leaves a short time span only for its development. From around 1720

[27] This was to be the general norm. However, precisely in the early Eshnunna texts we see that it was a *choice*. The "Tell Harmal compendium" ([Goetze 1951] – a catalogue of problem types, undated because it was found on the ground (left behind after an illegal digging) but probably contemporary with the other texts – sometimes writes u š with a phonetic complement (the possessive suffix *-ia*), and sometimes uses Akkadian phonetic writing (*ši-di-i*). This alternation corresponds to the pattern we find with terms where no strict technicalization is attempted; its virtual absence from the "algebraic" texts is thus evidence of precise awareness of the particular technical role of u š and s a g̃ as "algebraic variables".

[28] Lexical lists give k u d , which to my knowledge appears only with this possible meaning in the atypical mathematical Susa text TMS XXVI [TMS, 124*f*] – but the intention there might just as well be *nasāḫum,* as supposed by Bruins in his transcription and commentary, even though this would also by singular. Normally, k u d when used in Old Babylonian mathematical texts stands for *nakāsum* or *ḫasābum.*

[29] See the analysis in [Høyrup 2002a: 162–174].

[30] As Robson points out, we have a mathematical text from Larsa from the late 19th century BCE – but a multiplication table (YBC 11924, [MCT, 23]). Edubba texts on similar prisms from Larsa reflecting the ideology of the school are dated 1739 BCE.

BCE, that is, from the successful secession of the Sealand, we have very few dated documents, and the main cities appear to have been depopulated; there is no reason to assume that this is not evidence of a general decline of high literate culture in the area. Already twenty years before that, after an earlier rebellion, the emigration of scholar-priests toward the north seems to have begun.

The sophisticated mathematical texts produced in the south thus represent something like snapshots of local "styles" or "schools".[31] Most of them belong to four more or less well-defined text groups, two of which are likely to come from Uruk and one from Larsa. They are described in [Høyrup 2002a: 333–349]. Beyond certain orthographic characteristics, they all have in common the avoidance of the idea of "seeing" the outcome of calculations.[32] Other conspicuous features, however, allow us to differentiate.

We may look first at the two Uruk groups – labelled "group 3" and "group 4" by Goetze [1945]. The mathematical language is characterized by multiple possibilities to express the same operation or process – we have already encountered some of them. The "equalside" (the side of a square area or cubic volume) may be treated as a verb or as a noun; in group 3, it is consistently a verb, in group 4 a noun. Bisection (*ḫēpum/*g a z , literally "breaking") may be explained to be "into two"; so it is consistently in group 4, but never in group 3. The prescription may open with an elaborate formula "you, by your making", and always does so in group 3; or this may be reduced to a mere "you" or be totally absent, which are the two possibilities used in group 4. Similarly concerning a number of other features of the terminology as well as of the way to structure problems by means of logical operators.

31 This discussion concerns *formats*, which best characterize particular written traditions. Similar *problems, problem types,* and *methods,* on the other hand, are found in all groups, in the south as well as the northern periphery; they can thus be seen to have travelled, and to have provided that shared cultural framework of which we speak as "Old Babylonian mathematics".

32 This apparently highly deliberate avoidance of what seems to be a characteristic of an Akkadian tradition allows us to formulate a working hypothesis concerning the relation between the Eshnunna and the Larsa-Uruk texts. Eshnunna was, and was apparently recognized as, the cultural centre of "Akkad" (consisting in the early 18th century of Eshnunna, Babylonia and Sippar: Eshnunna's scribal culture was emulated by Samsî-Addu, Eshnunna produced the first Akkadian law code we know about (apparently at the same time as the mathematical texts we have just discussed). If Hammurapi, recognizing this, carried Eshnunna scholars and scholarship with him, he will rather have brought them to Babylon than to the former Sumerian and newly conquered south. What we find in Larsa and Uruk may thus have been inspired by new activities known from the Babylonian schools, but it will have differentiated itself from that part of the vocabulary which had obvious political connotations, that is, avoiding *tammar* and *ḫarāṣum* as well as the riddle introduction, "If somebody …". Mesopotamian rulers, as we know, were no less confident in the power of words than those of the last century – and the technicians of such words, the scholar-scribes, probably no less. Tanret [2010: 247] points to a similar symbolic act of resistance against Hammurabi on the part of the head administrator of the temple of the Sun god Šamaš in Sippar.

Each group is so internally consistent that its texts are likely to come from the same school (and perhaps school room), and thus also to have been produced within a rather short time span. On the other hand, the formats of the two groups differ so clearly from each other that none of them can have descended from the other, neither by reduction nor by elaboration. They represent two different ideas of how a mathematical problem should look, and two different attempts at norm-setting. Maybe they even express mutual deliberate rejection (Uruk being a large city this is not certain).

Group 1 is probably from Larsa. AO 8862, just discussed, belongs to this group. The group is less uniform that groups 3 and 4, and even on the same tablet different problems may use different formats – see [Høyrup 2002a: 337–345]. Sometimes these differences may point to differences in inspiration – in AO 8862, "algebraic" problems about rectangles and their sides differ from those that deal with bricks, a traditional scribal concern; but even the "algebraic" problems do not fully agree on the choice of terminology. Similarly, four "algebraic" problems about the same geometric configuration in YBC 6504 do not agree with each other in this respect. The texts from the group are likely to have been produced within a single environment, but perhaps over some time; in general the group appears to offer evidence of experimentation rather than codification.

Group 2 [Høyrup 2002a: 345–349], not even hypothetically located better than "in the south", presents us with a new phenomenon – extensive "theme texts". Since it is a theme text (containing 24 "algebraic" problems about one or more squares), Goetze also included the text BM 13901 in the group with some doubts, but the inclusion can now be seen to be ill-founded. The theme texts that remain, and which are almost certainly made within the same environment though not by the same author, deal with excavations (k i . l á) and small canals (p a $_5$. s i g). They are characterized by combining geometric or "algebraic" calculation with determination of the labour costs of producing the objects. Beyond the theme texts, the group encompasses a number of statement catalogues, in part corresponding to known theme texts – an extraordinary luck, and a strong indication that the texts really come from the same find spot.[33] Some slips indicate that the texts were

33 The catalogue YBC 4612 [MCT, 103*f*], dealing with simple rectangle problems, is written in a courser ductus than the catalogues that with certainty belong to the group; otherwise it is similar to them, but not sufficiently similar to eliminate all doubts concerning its appurtenance.

In any case, this simple text may be of singular interest, as it seems to provide the missing link between the area computations that represent the culmination of the normal mathematical syllabus and the sophisticated "algebra" problems. It contains 15 problem statements about rectangles, with answers. As in the catalogues certainly belonging to group 2, the format is rudimentary: a grammatically neutral and almost purely logographic presentation of the situation, the question marked by . b i e n . n a m, and answer – precisely as in the Nippur area problems solved by students that were discussed above, before note 12. Everything is stated in specified length and area metrology, whereas the "algebra" texts usually leave units implicit (or, said in another way, remain within the domain of place value calculation, where all units have been transformed into tacitly

inspired by material of northern origin, and that the authors attempted to reformulate this material in a way prescribed by their own norm.

The creation of theme texts and of corresponding catalogues is evidently a parallel to what happened in other domains of Old Babylonian scholarship like extispicy, astrological divination and medicine [Maul 2005: 71; Rochberg 2004: 63; Rochberg 2006: 347; Glassner 2009: 3; Geller 2010: 42]. The affinity is enhanced by the fact that the catalogue texts indicate the number of sections they contain (normally on the edge, which would allow this number to be read when the tablet was on the shelf).

Among the northern texts, Goetze's group 5 [Høyrup 2002a: 332] is too small to say much – it consists of one complete and fairly well-preserved text, a fragment and a heavily damaged text. It exhibits some similarities to Haddad 104 [al-Rawi & Roaf 1984], referred to above as containing "rules and problems falling within the range of Ur III scribal calculation", that is, of the Ur III tradition as digested in Eshnunna. It cannot be decided whether the similarities between the three texts that constitute the group reflect a deliberate attempt to adjust to or develop a norm or merely reflect loose local habits.

Goetze's group 6 (as augmented with texts belonging to the same family and published or identified as such in the meantime) is much more extensive [Høyrup 2002a: 329–332]. One of its members mentions a name in the colophon which is likely to be from Sippar [Robson 1999: 240 n. 26], which agrees well with the shared orthographic habits of the group.

A subgroup (labelled 6A in [Høyrup 2002a]) is so uniform that it certainly comes from a single school with a particular norm. A few more texts differ from this subgroup on several accounts but are still sufficiently close to allow us to distinguish a local style.

6A encompasses both theme texts (including BM 85200 + VAT 6599, famous for treating irreducible cubic problems about "excavations" but indeed also problems of the first and second degree about this configuration) and a catalogue (BM 80209, see [Friberg 1981]); the theme texts indicate the number of sections, as did the catalogues from group 2, but they are much less orderly than these (bordering upon the class of "anthology texts") and in so far less related to the omen and medical series emerging at the time.

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assumed basic units). The problems fall into three groups, the last of which varies the two sides and ask for the area; they correspond precisely to the student exercises. The former two groups both: (1) start by stating the sides, asking for the area, and then go on with four problems where the area is given together with (2) the length, (3) the width, (4) the sum of length and width, or (5) the difference between them. (1), (2) and (3) are already present in the Sargonic school texts. (4) and (5) are not, but they are the basic "algebra" problems.

In other Old Babylonian text – e.g., YBC 6504 – we see that the types (2)–(5) were regarded as a closed group, but too elementary to be presented directly; therefore they had to be embedded in more complicated situations, or submitted to variation.

Certain features of the texts show a still living contact with the lay surveyors' environment. Some of these features (and a number of others) also indicate affinity with the texts from Eshnunna – not least the use of *tammar*, "you see", for the results of calculations. Goetze's claim [MCT, 151], advanced before the Eshnunna texts were known, that the "6th group comprises northern modernizations of southern (Larsa) originals" can be put safely to rest.

The mathematical texts from Susa,[34] presumably from the outgoing Old Babylonian period, are also in "northern" style and with a single exception coherent enough to be regarded as expressions of a particular normative ideal. Their being found together already shows them to have belonged to the same archive; the explicitly didactic character of several of the texts (explaining concepts, not solving problems, see [Høyrup 2002a: 85–95]) confirms that this must have been some kind of school archive. It contains some of the most intricate problems ever dealt with in Old Babylonian mathematics – not least TMS XIX, which solves a bi-biquadratic problem. It also confronts us with the first known experiments with intermediate zeroes (in text XII).[35] The sign is sufficiently close to what is used in Seleucid texts to make us suspect a link; but since it is nothing but the separation sign, reinvention is not to be excluded.

We shall close the discussion of Old Babylonian mathematical text groups by the "series texts", which certainly constitute the closest parallel to the scholarly series produced in domains like divination and medicine. The texts were given the name by Neugebauer [MKT I, 383*f*] because the tablets are indeed numbered as members of series.[36]

The texts are written in an utterly compact logographic style; often the single statement can only be understood in the context of those that precede it, as it just indicates the variation with respect to what comes before and not the complete set of data. The variation

[34] Published and (often badly) translated and commented upon in TMS – and also badly excavated by an expedition that was not interested in mud-brick structures or the provenience of tablets, see [Robson 1999: 19] and [MCT, 6 n. 28].

[35] In order to understand that these are intermediate zeroes one should realize that the place value system was not really sexagesimal but seximal-decimal, as the Roman number system is dual-quintal. So, it stands (three times) where a 1-place is empty between two 10-places: 1.30▲16.40, 5.7.30▲41.40, 1.30▲16.40. The "zeroes" are there not in order to eliminate (non-existent) ambiguity but as a matter of principle.

[36] Since such serialization was a widespread phenomenon in late Old Babylonian scribal culture, it is not to be excluded that serialization of mathematical texts was initiated in several places. Friberg [2000: 164] suggests to move VAT 7528, YBC 4669, YBC 4698 and YBC 4673 (all classified as series texts in MKT) to a "group 2b", related to the expurgated group 2 (which he calls "2a", following [Høyrup 2000], and which already Neugebauer [MKT I, 506] has regarded as a separate "Gruppe C"). He could be right – apart from the absence of serial numbering from the group-2a catalogues there are outspoken similarities.

is highly systematic, organizing the variation of up to four parameters in Cartesian product.[37] Similar aims can be found in other fields where series were produced – but their subject-matter did not permit a similar unfolding of the principle, as illustrated by this excerpt from the "Diagnostic Handbook" going back to c. 1700 BCE [Geller 2010: 90, cf. p. 42]:

> [If] his urine is like ass urine, that man suffers from "discharge".
> [If] his urine is like beer dregs, that man […]
> [If] his urine is like wine dregs, […]
> [If] his urine is like clear paint, […]
> If his urine is like *kašu*-juice, […]
> If his urine is yellow-green, […]
> If his urine is white and thick, […]
> If his urine is like *dušû*-stone, […]
> If his urine is as normal, but his groin and epigastrium cause [him] pain, […].

It is difficult to determine with precision the geographical origin of the series texts. Neugebauer in MKT suggested Kish, with arguments that he himself and Sachs eliminated in [MCT, 95], together with the whole category [MCT, 37].[38] In [Høyrup 2002a: 351*f*] I conclude from a sequence of arguments of which none are fully coercive when taken in isolation

> that the series texts are less closely related to group 6A than believed by Neugebauer; that they will have been produced somewhere in the peripheral orbit – that is, outside the ancient Ur III core area. If we look at the problem types where n u . z u and a . n a u š u g u s a g̃ d i r i g and their syllabic equivalents turn up in groups 1 and 3 (broken-reed and stone riddles, etc.) we may also infer that the series texts, in spite of their sophistication and highly technical language, were produced in a place where the riddle tradition was closer to the surface than in the school where (e.g.) group 6A was produced and used.

Friberg [2000: 172] concludes from analysis of the use of Sumerograms that

[37] Christine Proust, who is undertaking a new profound study of the text group, speaks of "tree-structured lists" [2010] or "schéma arborescent à 4 niveaux" [Proust 2009], which is adequate if (and only if) we think of all branches at the same level splitting up in the same way.

[38] Neugebauer and Sachs argue that the same number might be given to different texts (which however only shows that no single canonical series similar to *Enūma Anu Enlil* existed in mathematics), and that therefore

> the numbering of these texts implies nothing more than an arrangement of tablets of various groups by a scribe to keep them in order.

However, as it has turned out, even the mature *Enūma Anu Enlil* exists in several variants, and in general the attempt to create standardized ("canonical") series seems to belong to the Kassite period [Rochberg-Halton 1984: 127*f*]. The extispicy texts k i + *n* [Glassner 2009: 24–29] would fit Neugebauer's and Sachs's discussion no less well than the mathematical texts.

> the general impression one gets is that the Sumerian terminology of the mentioned [main] group of series texts, Group Sa, is closest to that of Group 3, the one assumed to be from Uruk (in spite of what Hoyrup claims, op. cit.)

while Proust [2010: 3] suggests

> that the structure of the colophons might speak in favor of a connection between the mathematical series texts and a tradition which developed in Sippar at the end of the dynasty of Hammurabi

more precisely, during Ammisaduqa's reign – cf. also [Proust 2009: 195].

As to the time when the mathematical series texts were produced, we also have to rely on indirect arguments. Proust's observation of the similarity with dated colophons from the late 17th century is supported by the observation that the utterly intricate elaboration of the texts show them to be the end product of a long development. This, on the other hand, can be combined with our general knowledge of history: mathematical texts written at that moment can hardly have been made in the Sealand, and thus not in the former Sumerian core (Ur, Larsa, Uruk); they may, on the other hand, have been produced by scholarly emigrants from the south or their professional descendants, which would explain the features shared with texts from groups 1 and 3.

In the end, there turned out to be a fundamental difference between the genre of mathematical series texts and other incipient serializations like k i + *n*. The latter were adopted by the scholar-scribes of the Kassite and later times, giving rise to the large series we know from the Assyrian libraries. The former, like the whole fabulous enterprise of Old Babylonian sophisticated mathematics, did not survive the breakdown of the Old Babylonian cultural complex. Mathematics may serve for warfare and already did so in the Bronze Age,[39] but it appears to be better served itself by peace. At the conquest of Eshnunnna, it could follow the victors to the south and flourish in the *pax babyloniaca* (relative as it was), even though the choices of format indicate that it was a general idea and not a precise written tradition nor a well-defined professional carrier group that made the transfer to Larsa and Uruk. When the southern cities fell to the Sealand, some carriers of the tradition might still go north – but at the Kassite take-over, there was nowhere left to go. Divination and magic could survive in "inner emigration" within the scribal families and eventually re-emerge; mathematics, if admitted, withered away.

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[39] The "siege calculations" of mathematical texts are certainly artificial, but they are none the less witnesses of a military practice where volume calculation (etc.) served.

Late Babylonian sophistication

One seemingly sophisticated – but actually pseudo-sophisticated – text does seem to come from the Kassite period: AO 17264. It deals with a topic dear to Old Babylonian calculators: a trapezoidal field divided by parallel transversals into strips – here six strips that are pairwise equal in area. As Lis Brack-Bernsen and Olaf Schmidt conclude [1990: 38] after analyzing the text and the mathematics of the problem, it

> is beyond the capability of Babylonian mathematicians, and it looks as if they have given up in despair in their attempt at solving this problem and just given some meaningless computations that lead to a correct result.

The solution is indeed another mock solution, not mathematics but just mystifying calculations. The Kassite date, originally suggested by Thureau-Dangin [1934: 61] for palaeographic reasons, is supported by the terminology and format [Høyrup 2002a: 387*f*]. It is of vaguely northern type, but not similar in details to anything known to be Old Babylonian. It suggests (nothing more!) conservation within a scribal family of some memory of the high level of Old Babylonian mathematics and a rather vain ambition to show that the author was still at that level. In any case, we have to wait until the fifth century BCE before we find a few texts which are somehow akin to Old Babylonian "algebra".

The texts in question have been published in [Friberg, Hunger & al-Rawi 1990] and [Friberg 1997]. According to [Friberg 2000: 175*f*]

> these texts contain what must be Late Babylonian reformulations of Old Babylonian mathematical problems, with the ninda as the basic unit of length and the square ninda as the basic unit of area, as well as obviously Late Babylonian mathematical problems, with the cubit as the basic unit of length and surface extent measured in terms of either seed measure or reed measure.

However, the texts that combine the "standard" length metrology (still present in lists, we remember) with the new area metrology show in other respects *not* to be mere reformulations of Old Babylonian texts except in a very vague sense. They deal with rectangles for which the area is known together with one of the sides; the sum of the sides; or the difference between them. As we remember from note 33, these are the simple problems that so to speak hide below Old Babylonian algebra but were too simple to be presented directly; half of them, we also remember, were already taught in the Sargonic school. More decisively, they belong to that small set of surveyors' riddles that was borrowed by the Old Babylonian mathematics teachers and developed by them into the "algebraic" discipline[40]

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[40] This set of riddles, together with its widespread influence and duration until the Sanskrit, Islamic and even Latin/Italian Middle Ages, is discussed in [Høyrup 2001] ⟦= article I.3⟧.

As can be read in a colophon, the texts in question belonged to a scholar-scribe from the fifth century BCE.[41] It is therefore informative that one of the Sumerian terms occurring in the texts (n i m, "lift up") is used differently than in the Old Babylonian period. In Old Babylonian mathematics it had been one of the logograms that could designate the "multiplication by proportionality" (Akkadian *našûm*), by now it meant "subtract" (namely, by lifting up from the counting board[42]). In corroboration of what was said above, it appears that the Late Babylonian scholar-scribes, when taking up interest in mathematics, probably combined whatever was still handed down from the "scientific" system with what was actually used by "those who measured and counted professionally" and with substance borrowed from these. What these people did was probably already carried out in Aramaic, and written not on clay but on wax tablets or on papyrus; the colophon just mentioned states indeed that the text is copied from a wax tablet [Friberg, Hunger & al-Rawi 1990: 545]. It is therefore not possible to claim that these texts are really part of a written tradition belonging to the scholar-scribes, they may as well represent an attempt to *re-establish* a tradition which was known to have been lost – in the way 12th-century (CE) Latin scholars struggled to reconquer a Greek scientific and philosophical heritage whose existence they only knew about from late ancient Latin encyclopediae. Perhaps they represent a temporarily successful attempt, whose continuation we only have not been fortunate enough to find, perhaps they are nothing but the remains left over after a failure.

In any case, the next small group of sophisticated texts we know about, written some 200 years later, is again quite different in character. Apart from a particular kind of second-degree "algebra" asking for the value of a pair of reciprocal numbers (*igûm* and *igibûm*) whose sum or difference is given (an application of the simple rectangle problem structure), already popular in the Old Babylonian period and probably handed down together with the place value system, what we find in the Seleucid texts are again geometrical riddles – now in pure numbers, as in the Old Babylonian period, but involving for instance the sum of the sides and the diagonal of a rectangle and using new (but still geometric cut-and-paste) techniques. Since the same problems turn up at approximately the same time in sources from Demotic Egypt [Høyrup 2002b], they cannot have been developed and kept within a closed environment of scholar-scribes, as supposed by Eleanor Robson [2008: 261*f*]. One problem in a text which otherwise contains "algebraic" rectangle diagonal problems (BM 34568 #16, in [MKT III, 16]), moreover, deals with a cup consisting of

[41] See ([Friberg, Hunger & al-Rawi 1990: 545]; dating from [Robson 2008: 227–237]).

[42] This was also the original (Ur III) sense of z i , in Old Babylonian times used as a logogram for *nasāḫum,* "to tear out", the concrete, "identity-conserving" subtraction. Irrespective of language change and interruption of textual traditions, material calculational practice had remained the same. Not all traditions in Mesopotamian mathematics were written.

an alloy of gold and copper – a type which was to become very popular in medieval merchants' arithmetic. Once again, what we see is a reflection of the impact of external traditions (literate, semi-literate or oral, we do not know) on the cuneiform-scholarly environment, in an interesting replay of the influence of similar traditions on Old Babylonian (and, to a much more restricted extent, Pharaonic) mathematics – but with the difference that this time no lasting tradition or mathematical culture resulted within the cuneiform literate world (which by then was reduced to a tiny though stubborn *arrière-garde*); Marx's adage about history being played twice, first as tragedy and then as farce, comes to mind.

Summing up

As we have seen, basic mathematical techniques were handed down within the cuneiform literate tradition over very long periods, some of them (part of the metrology) over a time span longer than the one which separates us from Homer. We may suspect but often cannot specify interactions between this literate and other less literate ("lay") traditions.

When looking instead at what is mostly thought of as "Babylonian mathematics", namely the sophisticated level, it is much more difficult to distinguish true traditions. The Old Babylonian period presents us with a mathematical culture of high level, and when we look at details we find attempts to *establish* standards and traditions – but all of them apparently short-lived, for internal or external reasons. Parallels to the omen or grammar traditions beginning in Old Babylonian times and still alive in the first millennium cannot be found.

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Chapter 27 (Article II.10) Mesopotamian Mathematics, Seen "from the Inside" (by Assyriologists) and "from the Outside" (by Historians of Mathematics)

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Original wording of translated quotations and a few
additions touching the substance in ⟦…⟧

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Abstract

Since the 1950s, “Babylonian mathematics” has often served to open expositions of the general history of mathematics. Since it is written in a language and a script which only specialists understand, it has always been dealt with differently by the “insiders”, the Assyriologists who approached the texts where this mathematics manifests itself as philologists and historians of Mesopotamian culture – and by “outsiders”, historians of mathematics who had to rely on second-hand understanding of the material (actually, of as much of this material as they wanted to take into account), but who saw it as a constituent of the history of mathematics. The article deals with how these different approaches have looked in various periods: pre-decipherment speculations; the early period of deciphering, 1847–1929; the “golden decade”, 1929–1938, where workers with double competence (primarily Neugebauer and Thureau-Dangin) attacked the corpus and demonstrated the Babylonians to have possessed unexpectedly sophisticated mathematical knowledge; and the ensuing four decades, where some mopping-up without change of perspective was all that was done by a handful of Assyriologists and Assyriologically competent historians of mathematics, while most Assyriologists lost interest completely, and historians of mathematics believed to possess the definitive truth about the topic in Neugebauer’s popularizations.

Contents

"Through a glass, darkly" – historians of mathematics before Assyriology

Until 1850, historians of mathematics had no other way to know about pre-classical Near Eastern mathematics[1] than using the information they could draw from classical authors, at best submitted to historical and epistemological common sense – whence the quotations from 1 Corinthians 13:12 in the above headline (which entails no promise like that of St. Paul that in the end we shall see "face to face"). This, for instance, is Jean–Étienne Montucla's (1725–1799) account, in Enlightenment spirit, about "what is told" about the birth of arithmetic [Montucla 1758: I, 46*f*]:[2]

> The Phoenicians, some say, were the first and the most able merchants of the world; but Arithmetic is nowhere more useful and more necessary than in trade: these people must therefore also have been the first Arithmeticians. Strabon[3] relates this as the accepted opinion of his age; and even if we should believe a historian,[4] Phoenix son of Agenor wrote as first an arithmetic in Phoenician language. On the other hand Egypt boasts of having been the cradle of this art;[5] and since a human intelligence hardly seemed to suffice for so useful an invention, one devised the pious fable that a god was its author, and had communicated it to mankind.[6] At least it was the general opinion, according to Socrates or Plato[7] that *Theut* was the inventor of numbers, calculation and geometry; and it is quite likely that the Greeks took from here the idea to attribute to their Mercury, with whom *Theut* or the Egyptian *Hermes* has a conspicuous connection, the jurisdiction of trade and arithmetic.[8]

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1 A conceptual clarification: The "Near East" encompasses Egypt, the Palestino-Syrian area, Arabia and Mesopotamia – sometimes other neighbouring areas are included as well. Mesopotamia largely coincides with present-day Iraq. Its northern third is Assyria, and the remainder is Babylonia. Chaldea strictly speaking is the southern third (in the third millennium BCE Sumer), but often in the quotations that follow it stands for the whole of Babylonia.

2 My translation, as everywhere below where nothing else is stated. In cases where the titles of publications that are cited or quoted have been translated, the genuine titles can be found in the bibliography.

3 *Geograph*.lib.XVII.

4 Cedrenus [an 11th-century Byzantine historian / JH].

5 Diog. Laer. *in proemio.* [ed. Hicks 1925: I, 12 / JH].

6 [Plato,] *In Phædro.* p. 1240 ed. 1602. [274c /JH].

7 *Ibid*.

8 [At this point, the astronomer Joseph-Jérôme Lalande (1732–1807) adds the following in the second edition – the earliest reasoned reference to *Babylonian* mathematics [Montucla 1799: I, 43*f*]:

> It is even quite difficult not to affiliate them with the Chaldeans. Indeed, since they present us with the first traces of astronomical knowledge, they must necessarily have possessed an arithmetic, a very advanced one at that. How would they, without that tool, have been able to discover several astronomical periods, knowledge of which has come down to us!
> ⟦Il est même bien difficile de ne pas leur associer les Chaldéens. Car puisque ces peuples

But I shall insist no further on these fabulous or risky lines; who wants to discuss the origin of our knowledge somewhat philosophically will see that Arithmetic must have preceded everything else. The first civilized societies could not do without it; it suffices to possess something for being forced to use numbers, and even the first men, if only they had to count days, years, their age, that is enough for saying that they knew Arithmetic. Admittedly, richer or more trading societies may have expanded the limits of this natural Arithmetic by inventing perhaps shortened ways or procedures; and in this sense Strabon has said nothing contrary to reason. As regards *Josephus*' account[9] indicating Abraham as the first of Arithmeticians and making him teach the Egyptians the first elements of Arithmetic, it is easy to see that this historian wanted to adorn the first father of his nation with part of that knowledge which he saw honoured among foreigners. This is one of those pictures that will be favourably received only by some compiler deprived of critical sense and reasoning. ⟦[10]⟧

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nous présentent les premières traces des connoissances astronomiques, il falloit bien qu'ils eussent une arithmétique, et même fort avancée. Comment, sans ce secours auroient-ils pu parvenir à la découverte de plusieurs periodes astronomiques, dont la connoissance est venue jusqu'à nous!⟧

Apart from that, Montucla's passage is unchanged. / JH]

9 *Ant. Jud.* liv. I c. 9. [Actually chapter 8 / JH].

10 ⟦Les Phéniciens, ont dit quelques-uns, furent les premiers & les plus habiles commerçans de l'univers; mais l'Arithmétique n'est nulle part plus utile & plus néccessaire que dans le commerce: ainsi ces peuples ont dû tre aussi les premiers Arithméticiens. Strabon nous donne cette opinion comme accréditée de son tems; & même si nous en croyons un historien, *Phœnix* fils d'*Agenor* écrivit le premier une arithmétique en langue Phénicienne. D'un autre côté l'Égypte se faisoit gloire d'avoir été le berceau de cet art; & comme une intelligence humaine parut à peine suffire pour une invention si utile, on imagina cette pieuse fable qu'une Divinité en étoit l'auteur, & qu'elle en avoit fait part aux hommes. C'étoit du moins l'opinion générale, suivant Socrate ou Platon que *Theut* étoit l'inventeur des nombres, du calcul et de la Géométrie; & il est fort probable que c'est de là que les Grecs ont pris l'idée, de donner à leur *Mercure,* avec qui le *Theut,* ou l'*Hermes* Egyptien a un rapport marqué, l'intendance du commerce & de l'Arithmétique.

Mais je n'insisterai pas davantage sur ces traits fabuleux ou hazardés; quand on voudra discuter un peu philosophiquement l'origine de nos connoissances, on verra que l'Arithmétique a dû précéder toutes les autres. Les premières sociétés policées ne purent s'en passer; car il suffit de posséder quelque chose pour être obligé de faire usage des nombres, & même les premiers hommes n'eussent-ils que compté les jours, les années, leur âge, leurs troupeaux, en voilà allez pour dire qu'ils étoient en possession de l'Arithmétique. Il est vrai que les sociétés les plus riches ou les plus commerçantes ont pû étendre les limites de cette Arithmétique naturelle, en inventant peut-être des lignes ou des precédés abrégés pour soulager l'esprit dans les supputations un peu compliquées: & en ce sens Strabon n'a rien dit que de conforme à la raison. Quant au récit de *Josephe* qui nous donne *Abraham* comme le plus ancien Arithméticien, & qui lui fait enseigner aux Egyptiens les premiers élémens de l'Arithmétique, il est aisé de voir que cet historien a voulu parer le premier pere de sa nation de quelques-unes des connoissances qu'il voyoit en estime chez les étrangers. C'est un de ces traits qui ne peuvent trouver de l'accueil qu'auprés de quelque compilateur dénué de critique & de raisonnement.⟧

The last line could be directed at Petrus Ramus (1515–1572), in whose *Scholae mathematicae* [1569: 2] this story is taken for a fact (yet with a correct reference to chapter 8).[11]

Abraham Gotthelf Kästner (1719–1800) has no more sources than Montucla and is even more cautious in his *Geschichte der Mathematik* [1796: I, 2]:

> To us, the oldest teachers of mathematics were the Greeks. What *they* may have learned from the Orient we only know from their own confessions, and how far their teachers have continued on their own, that they did not consider it necessary to write down. ⟦[12]⟧

These two quotations, with the addition quoted in note 8, illustrates how much could be known about the mathematics of Mesopotamia and neighbouring areas until the birth of Assyriology.

The beginnings of Assyriology

The earliest dead languages and writing systems to be deciphered were Aramaic dialects – first Palmyrene in 1754, then in 1764 and 1768 Phoenician and Egyptian Aramaic [Daniels 1988: 431]; all three scripts were alphabetic, and the basis was provided by bilingual texts containing proper names, which were skilfully exploited by Jean-Jacques Barthélemy (1716–1795).

Much more famous is Jean-François Champollion's (1790–1832) use of the Rosetta Stone in the decipherment of the hieroglyphs and the Demotic script [1824], proving the mixed alphabetic-ideographic character of the former as well as the existence of homophones in the alphabet.

The decipherment of the cuneiform scriptures was a more involved affair – a short description will illustrate how much more involved. It will make it clear why even understanding of cuneiform *mathematics* had to be made in very small and slow steps.

Initially, everything was based on the trilingual inscriptions from Persepolis, which Pietro della Valle (1586–1652) had seen in 1621 to be written from left to right.[13] The

[11] Abraham is at least absent from Giuseppe Biancani's (1566–1624) *Clarorum mathematicorum chronologia* [1615: 39], and also from Gerardus Vossius's (1577–1649) *De universae mathesios natura et constitutione liber* and *Chronologia mathematicorum* [1650], while Polydorus Vergilius (c. 1470–1555) [1546: 59*f*] has no chapter reference. Since Montucla does not abstain from identifying Ramus by name when chiding him for following "the inclination of the mob toward everything that seems marvellous" (p. 450), the present reference is most likely at least not to be to Ramus alone.

[12] ⟦Für uns sind die ältesten Lehrer der Mathematik, die Griechen: Was sie selbst von den Morgenländern gelernt haben, wissen wir nur aus ihren eignen Geständnissen, und wir weit ihre Lehrer für sich fortgegangen sind, das aufzuzeichnen, war ihnen nicht nöthig.⟧

[13] What follows about work done before 1860 is drawn, when no original sources are referred to, from Charles Fossey's (1869–1946) very detailed exposition of (good and bad) arguments and results [Fossey 1904: 85–220].

development until around 1800 is described by Fossey as on the whole a "period of groping and of hazardous and contradictory hypotheses" (p. 90). Noteworthy positive contributions were, firstly, Carsten Niebuhr's (1733–1813) new and more precise copies of the Persepolis inscriptions – his discovery that three different scripts are involved – and his confirmation of the writing direction [1774: II, 138*f*, pl. XXIII, XXIV, XXXI]; and secondly, at the very close of the period, Friederich Münter's (1761–1830) dating of the inscriptions to the Achaemenid era (1798, published in Danish in 1800) – his confirmation that three scripts are used – and his arguments that the first of these is alphabetic, the second apparently mixed alphabetic-syllabic and the third perhaps mixed alphabetic-logographic [Münter 1802: 83–86] – his identification of a few signs from the alphabetic script as vowels (*ibid.,* pp. 104–109) – and his identification of its language as Old Iranian (more precisely he suggests Zend). Also of importance was Münter's verification that the Persepolis writing type had also been used in Babylon, and that it had probably originated in Mesopotamia (*ibid*, pp. 129–144).

In [1802], Georg Friedrich Grotefend (1775–1853) presented a memoir to the Göttingen Academy[14] which is habitually taken as the stumbling beginning of decipherment proper. He came to the same conclusions as Münter (whose work only appeared in German during the same year, and which Grotefend may not have known). He went further on three decisive points: showing that all inscriptions were linked to Darius and Xerxes; finding the royal names mentioned in the inscriptions as well as the word for king; and using this to identify a number of letters (he claimed identification of 29 letters of the alphabetic script, 12 of which were later confirmed).

Over the next four decades or so, a number of scholars extended and corrected Grotefend's work, removing false values and adding new ones (not always correctly at first), and identifying the language as an Old Persian dialect distinct from Zend (adding also new inscriptions to the corpus) [Fossey 1904: 112–146]. However, all of this concerned the alphabetic script, which was certainly derived from the cuneiform script of Mesopotamia but had a totally different character (and moreover concerned matters without the slightest relation to mathematics).

Decipherment of the second script (Elamite), using about one hundred signs and being in a language with no known kin, made some but little progress during the same period, and is anyhow irrelevant for the present purpose. Grotefend made some attempts at the third script, which is Akkadian (the language of which Babylonian and Assyrian are dialects). His firm belief that the language had to be an Iranian dialect was one of the reasons he had no success – but until the second half of the 1840s nobody else did much better. In the meantime, excavations had begun, and a much larger, geographically wider and chronologically deeper text corpus was now available.

[14] Published only in full in [Meyer 1893], for which reason I build on Fossey's account [1904: 102–111] of the arguments that circulated.

From 1845 onward, a large number of workers took up discussion and competition about the third script, from which some 300 signs were known: Isidore Löwenstern (1810–1858/59; pertinent publications 1845 and onward); Henry Rawlinson (1810–1895; 1846 and onward); Paul-Émile Botta (1802–1870; 1847 and onward); Edward Hincks (1792–1866; 1846 and onward); Félicien de Saulcy (1807–1880; 1847 and onward); Henry de Longpérier (1816–1882; 1847); Charles William Wall (1780–1862; 1848); and Moriz Abraham Stern (1807–1884; 1850) – of whom Rawlinson, Botta and Hincks were by far the most important. Before 1855 it was known that the language of the third script was that of Babylonia and Assyria; that this language (Akkadian) was a Semitic language, and thus a cognate of Arabic and Hebrew; that the same sign might have (mostly several) phonetic and (often several) logographic values, and even function as a semantic determinative (an unexpected function which Champollion had discovered in Hieroglyphics); and that the original shape of the signs had been pictographic. Moreover, Hincks had shown early on that the inventors of the script must have spoken a non-Semitic language. This is all summarized in a letter written by the young Jules Oppert (1825–1905) in 1855 (published as [Oppert 1856]), together with observations and hypotheses of his own. So, from then on large-scale reading of documents could begin – and we may speak of the birth of Assyriology. In [1859], Oppert himself was to stabilize the field – in his obituary of Oppert, Léon Heuzey (1831–1922) was eventually to write as follows [Heuzey 1906: 7]:

> After some works on ancient Persian, Oppert concentrated his principal effort on the Assyrian inscriptions. Having been charged by Fresnel with a mission into Babylonian territory, he published at his return, in 1859, a volume, the second tome (in date actually the first) of his *Expédition en Méslopotamie* [sic] in which, using recently discovered sign lists or syllabaries, he established the central rules of decipherment. This volume, Oppert's *chef d'œuvre*, indicates a turning-point; it put an end to gropings and instituted Assyriology definitively. ⟦[15]⟧

Assyriologists' history of mathematics, 1847–1930

On one account Oppert says nothing in his letter from 1855, even though this was to be one of the things that occupied him during his later brilliant career: mathematics.

However, already in a paper read in 1847 (published as [Hincks 1848]), Hincks had described the "non-scholarly" number system correctly.[16] In comparison, of the 76

[15] ⟦Après quelques travaux sur l'ancien perse, Oppert porta son principal effort sur les inscriptions assyriennes. Chargé avec Fresnel d'une mission en pays babylonien, il publia à son retour, en 1859, un volume, le tome second (en réalité premier en date) de son *Expédition en Méslopotamie* [sic], où, s'aidant des recueils de signes ou syllabaires récemment découverts, il fixa les principales règles du déchiffrement. Ce volume, le chef-d'oeuvre d'Oppert, marque une date ; il mit fin aux tâtonnements et fonda définitivement l'Assyriologie.⟧

[16] This system is sexagesimal but not positional until 100, after which it is combined with word-signs

syllabic values identified in this early paper only 18 turned out eventually to be correct or almost correct, while 46 had the right consonant but erred in the vowel, and 12 were wholly wrong [Fossey 1904: 185] – which however was already a significant step forward. The discovery of the place-value system followed soon. It was also due to Hincks [1854a: 232], who detected it in a tabulated "estimate of the magnitude of the illuminated portion of the lunar disk on each of the thirty days of the month".[17] A slightly later publication dealing with the numbers associated with the gods [Hincks 1854b: 406*f*] refers to the "use of the different numbers to express sixty times what they would most naturally do" on the tablet just mentioned; there, 240 is indeed written as iv (Hincks uses Roman numerals for the cuneiform numbers), while "iii.xxviii, iii.xii, ii.lvi, ii.xl, etc." stand for "208, 192, 176, 160, etc.".

Rawlinson also contributed to the topic in [1855] (already communicated to Hincks when the second paper of the latter was in print, in December 1854). A five-page footnote (pp. 217–221) within an article on "The Early History of Babylonia" points out that the values ascribed by Berossos [ed. Cory 1832: 32] to σάρος (*šār*), νῆρος (*nēru*) and σώσσος (*šūši*), respectively 3600, 600 and 60 years, are "abundantly proved by the monuments" (p. 217). As further confirmation Rawlinson presents an extract of "a table of squares, which extends in due order from 1 to 60" (pp. 218–219), in which the place-value character of the notation is obvious but only claimed indirectly by Rawlinson. The note goes on as follows:

> while I am now discussing the notation of the Babylonians, I may as well give the phonetic reading of the numbers, as they are found in the Assyrian vocabularies.

All three of "cuneiform's 'holy' triad", as Rawlinson, Hincks and Oppert were called by Samuel Noah Kramer (1897–1990) [1963: 15], were indeed quite aware that numbers and what had to do with them was important for understanding Mesopotamian history and culture.[18]

The reason that this was so is reflected further on in Heuzey's obituary:

> Oppert's scientific activity followed many directions: historical texts and religious texts, bilingual (Sumero-Assyrian) texts and purely Sumerian texts, juridical texts and divination texts, Persian texts and neo-Susian texts, there is almost no branch of the vast literature

for 100 and 1000.

[17] Archibald Henry Sayce (1845–1933), when returning to the text in [1875: 490; cf. Sayce 1887: 337–340], reinterpreted the topic as a table of lunar longitudes. Geometrically, the two interpretations are equivalent, but the final verb of the lines (DU, "to go") suggested this new understanding.

[18] In contrast, the just published Blackwell *Encyclopedia of Ancient History* planned the same number of pages for Mesopotamian mathematics and Mesopotamian hairstyles. It should be added that those who planned the volume had little idea about Mesopotamia (nor were they very interested in receiving advice, however).

> of the cuneiform inscriptions he has not explored. The most special questions, juridical, metrological, chronological, attracted his curiosity […]. ⟦[19]⟧

Evidently, administrative, economical and historiographic documents could – and can – only be understood if numeration and metrology were/are understood. Reversely, such documents, in particular administrative and economical records, are and were important sources for understanding numeration and metrology. Oppert’s observations on “the notation for capacity measures in cuneiform juridical documents” from [1886] offer an example.

For a long time, however, they were far from being the only sources for knowledge and assumptions. Already the decipherment of the scripts had drawn much on sources from classical Antiquity (how else could the names of the Achaemenid kings have been known?) and on comparison with Zend, Hebrew and Arabic. Similarly, known or supposedly known metrologies and numerical writings from classical Antiquity were drawn upon – sometimes with success (Rawlinson’s use of Berossos is an example), sometimes with exaggerated confidence in the stability and uniformity of metrologies. Didactical materials such as bilingual lexical lists and tables also played their role (as they had done in the decipherment); so did astronomical texts (as already for Hincks and Rawlinson in 1854–55).

Three illustrative examples are [Norris 1856], [Smith 1872] and [Oppert 1872]. Edwin Norris (1795–1872) not only draws much on Biblical material in his dubious article but also reads the cuneiform signs on Mesopotamian weight standards as Hebrew characters (“I thought the first word looked like מ נ ה” – p. 215). George Smith (1840–1876) makes use of metrological lists in order to establish the sequence of length units and their mutual relations, and of “lion” and “duck weights” (that is, stone sculptures of these animals on which their weight is inscribed) and of written documents in order to reach a similar understanding of the weight system (which turns out to be contradictory).

Oppert makes use of similar material. But he also believes in a shared stable “ancient” metrology[20] and draws in particular on Hebrew parallels (and on Hebrew measures which he assumes *must* have had a parallel[21]). Jöran Friberg [1982: 2] justly character-

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19 ⟦L’activité scientifique d’Oppert se porta dans des directions très diverses : textes historiques et textes religieux, textes bilingues (suméro-assyriens) et textes purement sumériens, textes juridiques et textes divinatoires, textes perses et textes néo-susiens, il n’est presque pas une branche de la vaste littérature des inscriptions cunéiformes qu’il n’ait explorée. Les questions les plus spéciales, juridiques, métrologiques, chronologiques, attiraient sa curiosité […].⟧

20 We may see this belief as the last scholarly and pseudo-scholarly survivor of the Renaissance faith in ancient *prisca sapientia.*

Paradoxically, Oppert had pointed out already in [1886: 90] that there were “in Assyria and Chaldea, as everywhere else, ceaseless variations in the measures”, which should have warned him against the dangers inherent in the comparative method.

21 See for example p. 427*f* on the postulated unit “hair”, which leads him to rather far-fetched hypotheses (presented “with all reserve”, it is true).

izes the outcome as "somewhat premature", even in comparison with other publications from the period.

The use of the place-value principle not only for integers but also for fractions was established in Johann Strassmaier's (1846–1920), Josef Epping's (1835–1894) and Franz Xaver Kugler's (1862–1929) analysis of the Late Babylonian astronomical texts, beginning with [Strassmaier and Epping 1881]; so far, however, it was understood only in analogy with the use of sexagesimal fractions in Ptolemaic and modern astronomy.[22]

In a way, Hermann Hilprecht's (1859–1925) *Mathematical, Metrological and Chronological Tablets from the Temple Library of Nippur* [Hilprecht 1906] constitutes a decisive step. As we have seen, tables of squares and metrological lists had already been used in the early period by Rawlinson [1855] and Smith [1872]. Hilprecht, however, put at the disposal of Assyriologists a large number of arithmetical and metrological tables. Unfortunately, his failing understanding of the floating-point character of the place-value system; the still strong conviction that the classical authors could provide interpretations of Mesopotamian texts; and a belief that everything Babylonian had to be read in a mystico-religious key[23] caused him not only to read very large numbers into the texts but also to understand a division of 1;10 (or 70) by 1 as 195,955,200,000,000÷216,000 (p. 27), where the denominator was then explained from a (dubious) interpretation of the passage about the "nuptial number" in Plato's *Republic* VIII, 546B–D (pp. 29–34) and coupled to postulated cosmological speculations:

> How can this number influence or determine the birth and future of a child? The correct solution of the problem is closely connected with the Babylonian conception of the world, which stands in the centre of the Babylonian religion. The Universe and everything within, whether great or small, are created and sustained by the same fundamental laws. The same

[22] Basing himself on indirect evidence and on Greek writings, Johannes Brandis (1830–1873) had already claimed that the unending sexagesimal fraction system of the Greek astronomers had to be of Chaldean origin "even if we never find direct or indirect testimony ascribing it to them" [Brandis 1866: 18].

[23] Hilprecht quotes this passage from Carl Bezold's (1859–1922) "concise survey of the Babylonian - Assyrian literature" [Bezold 1886: 225]:

> As far as we know by now, Babylonian-Assyrian mathematics primarily served astronomy, and this on its part a pseudo-science, astrology, which probably arose in Mesopotamia, propagated from there into the Gnostic writings, and was inherited by the Middle Ages, although we are not yet able to reconstruct this whole chain, the links of which are often broken from each other.
>
> ⟦Die Mathematik stand bei den Babyloniern-Assyrern, so viel wir bis jetzt wissen, vornehmlich im Dienste der Astronomie und letztere widerum in dem einer Pseudowissenschaft, der Astrologie, die wahrscheinlich in Mesopotamien entstand, sich von dort aus verbreitete und bis hinein in die gnostischen Schriften und aufs Mittelalter vererbte, ohne dass wir aber bis jetzt im Stande sind, die Kette dieser ganzen Ueberlieferung, deren Glieder vielfach zerstückt sind, widerherzustellen.⟧

> powers and principles, therefore, which rule in the world at large, the macrocosm, are valid in the life of man, the microcosm.

So, while Hilprecht's publication represented a material step forward, his approach remained that of the 19th century.

Franz Heinrich Weißbach's (1865–1944) article "about the Babylonian, Assyrian and Old Persian weights" [Weißbach 1907], on the other hand, inaugurated a new trend. As formulated by Marvin Powell [1971: 188], "the study of Mesopotamian weight norms can be divided into two eras: the pre-Weissbach and the post-Weissbach eras". Weißbach discarded the comparative method, concentrating (like George Smith) on what could be derived from Mesopotamian sources and artefacts. He did not convince those who were committed to the "comparativist paradigm"; instead, the process confirms the observation made by Max Planck [1950: 33] (concerning Ludwig Boltzmann) and famously quoted by Thomas Kuhn [1970: 151], namely that

> a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.7

The following generations of Assyriologists, indeed, less trained in Hebrew and classical scholarship but familiar with the results of a mature discipline, followed the model set by Weißbach and by the immensely influential François Thureau-Dangin (1872–1944).[24] The earliest work on metrology and mathematical techniques of the latter had been published in [1897] (a sophisticated interpretation of the intricate calculations on a field plan from the outgoing third millennium BCE); he was going to publish on metrological questions for decades to come.

So far, Assyriologists had been concerned with *mathematics in use,* namely in use in non-mathematical documents (including astronomy and texts serving in elementary mathematical training). The first to come to grips with what became known among historians of mathematics as "Babylonian mathematics" from the 1930s onward – namely mathematics which was complicated enough to be counted as mathematics by those same historians in the twentieth century – was the 25-years old Ernst Weidner (1891–1976) in [1916] (he had already published a volume on Babylonian astronomy in 1911 [Jaritz 1993: 15]). Weidner's article begins with the observation that

> As regards the knowledge of the Akkadians in the area of mathematics we are still quite badly informed.[25] Beyond a few tables containing square and cube numbers and rather many multiplication tables we have nothing real except building- and field-plans, which

[24] Outside Assyriology, in particular among natural scientists taking interest in Antiquity and its mysteries, the comparativist trend is still alive and kicking – see [Berriman 1953], [Lelgemann 2004] and [Rottlander 2006].

[25] [A footnote refers to Moritz Cantor's *Vorlesungen* I, on which below. / JH]

> already in quite early time makes us presuppose an Akkadian ability also to accomplish rather difficult calculations. ⟦[26]⟧

which summarizes the situation perfectly. Two difficult texts had been published in 1900, Weidner says[27] (only in cuneiform, with neither transliteration nor translation, which presupposes no understanding and conveys none); but these texts are then characterized as "probably the most difficult transmitted in cuneiform", which explains that nothing had been done on them. In the Berlin Museum he had now seen other texts of the same type, and he analyses two problems from one of them (VAT 6598): two different approximate calculations of the diagonal of a rectangle (a first and a corrupt second approximation, see [Høyrup 2002: 268–272]).

Compared to the analysis of the same text offered by Neugebauer in 1935, Weidner's interpretation contains some important mistakes, for which reason it can certainly be characterized as premature. However, Weidner's short paper, together with the commentaries of Heinrich Zimmern (1861–1931) [1916] and Arthur Ungnad (1879–1945) [1916; 1918] provided the first understanding of Babylonian mathematical *terminology*.[28]

In [1922], Cyril John Gadd (1893–1969) published a text dealing with subdivided squares, and added some further important terms (not least those for square, triangle and circle). Since the text in question contains no calculations, only terms for mathematical objects occur, none for operations. In [1928], finally, Carl Frank (1881–1945) published the collection of *Straßburger Keilschrifttexte* – a spelling that adequately reflects his working situation: he had made the copies before the War, when Strasbourg was Strassburg, and only received his own material in 1925, with no possibility of collating. None the less, Frank's book added another batch of terms. Because Frank's texts are even more difficult than the short ones dealt with by Weidner, Zimmern and Ungnad, and because Frank translated all sexagesimal place value numbers into modern numbers (repeatedly choosing a wrong order of magnitude), his understanding of the texts was rather defective.

This is how far Assyriologists went in the exploration of cuneiform mathematics until 1930 – when Assyriology was half its present age.

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26 ⟦Ueber die Kenntnisse der Akkader auf mathematischem Gebiete sind wir heute noch recht schlecht orientiert. Ausser einigen Tafeln mit Quadrat- und Kubikzahlen und verhältnismässig zahlreichen Multiplikationstabellen kommen eigentlich nur die Bau- und Felderpläne in Betracht, die uns schon für recht fruhe Zeit bei den Akkadern die Fähigkeit auch schwierigere Rechnungen auszuführen, voraussetzen lassen.⟧

27 Now known as BM 85194 and BM 85210.

28 Only the terms for (what can approximately be translated as) square and cube roots were known since Moritz Cantor's use of Hilprecht's material in [1908].

Quite a few of Weidner's readings later turned out to be philologically wrong while their technical interpretation was adequate. What was correct, however, was important later on, and some of the philological errors were still taken over in Neugebauer's early interpretations without great damage.

Historians of mathematics until c. 1930

On the whole, historians of mathematics depended during the same period not only on the material put a their disposal by Assyriologists but also on their interpretations.

In the posthumous [Hankel 1874], Hermann Hankel (1839–1873) dealt with "the Babylonians" (once, p. 65, accompanied by the Assyrians) on scattered pages of his discussion of the "pre-scientific period". Given the difficulties of Assyriologist with not only absolute but also relative chronologies until [Hommel 1885], it is no wonder that Hankel's observations are messy on this account. Substantially, he speaks about the sexagesimal divisions of metrologies (pp. *48f;* not mentioning that not all subdivisions are sexagesimal, which was known at least since [Smith 1872]); a hunch of sexagesimal fractions (pp. 63, 65; but only to one place, and understood as written with a denominator which is "usually omitted"); the existence of tables of squares and astronomical tables (the two texts used by Hincks and Rawlinson in 1854–55), from which the hypothesis is derived that the Babylonians were interested in arithmetical series (p. 67); and a low level of geometry, concluded on the basis of the "building art deprived of style" (p. 73). Iamblichos's claim that Pythagoras had his knowledge of the harmonic proportion from the Babylonians is mentioned but explicitly not endorsed (p. 105).

In the first edition of volume I of his *Vorlesungen* from [1880], Moritz Cantor (1829–1920) dedicates separate chapters to the Egyptians and the Babylonians – the latter on pp. 67–94. He is much better informed than Hankel – in part, it must be admitted, from publications that had appeared too late to be taken into account by Hankel, such as [Oppert 1872].[29] He offers an orderly exposition of the numerals and the "natural fractions" $^1/_6$, $^1/_3$, $^1/_2$, $^2/_3$ and $^5/_6$. Further, he describes the tables of squares and cubes (which in [1908] he was going to see as tables of the corresponding roots), and he discusses the sexagesimal place value principle in connection with astronomy. Geometry is dealt with on the basis of geometric decorations, Herodotos and other Greek authors, and the Old Testament. Babylonian numerology is also discussed, in particular the ascription of numbers to the gods.

In the third edition from [1907], Cantor deals with the Babylonians before the Egyptians (pp. 19–51). The five extra pages allow him to tell the historiography of the field, but apart from a suggestion of that reinterpretation of Rawlinson's tables of squares as tables of square roots which he was to publish in full in [1908], nothing substantial is changed in the account of Babylonian mathematics. There are, however, some remarks

[29] Already Cantor's "mathematical contributions to the cultural life of the nations" had contained a chapter on the Babylonians [Cantor 1863: 22–38]. At the time, however, he had only been able to speak about the decipherment; about "Oriental" culture in general; and about the writing system, about integer numerals and about the possible use of some kind of abacus (a hypothesis which he repeats in the *Vorlesungen*).

about the material published by Hilprecht in [1906], with faithful adoption of his immense numbers (pp. 28*f*).

Hans Georg Zeuthen's (1839–1920) *Geschichte der Mathematik im Altertum und im Mittelalter* [1896] dedicates a chapter (pp. 8–13) to what the Egyptians and the Babylonians knew in mathematics at the moment they came into touch with the Greeks, and which the Greeks might possibly have taken over from them (thus pp. 8*f*). Of the six pages, 26 *lines* deal with the Babylonians. 21 of these lines refer to astronomy and the division of the circle into 360°, and 5 to the possibility that Greek numerology was in debt to Babylonians and Chaldeans.

Johannes Tropfke (1866–1939) follows Hankel's pattern in the first volume of his *Geschichte der Elementarmathematik* [1902], mentioning the Babylonians now and then but not treating Babylonian mathematics per se – the obvious choice, given his full title "history of elementary mathematics presented systematically". But he only speaks about the sexagesimal system (mentioning Rawlinson's "square table" but without describing it). Only on two (quite dubious) points does he go beyond Hankel: he considers Iamblichos a certain source, and he claims (p. 304) that the Babylonians knew the solution 3–4–5 to the "Pythagorean equation"; he gives no source, and would have been unable to, since no pertinent text was known at the time. Most likely, he misremembers Cantor's idea [1880: 56] ("admittedly, for the moment without any foundation", thus Cantor) that *the Egyptians* might have used 3–4–5 triangles on ropes to construct right angles.

In general, historians of mathematics were not interested in Babylonian matters during the period. Inspection of 21 of the first 26 volumes of the series *Abhandlungen zur Geschichte der Mathematik*[30] (1877–1907) reveals no single article on the subject. For good reasons, as revealed by what Hankel and Cantor had been able to say about it – what Assyriologists had succeeded in finding out was still so tentative and so incoherent that it invited more to speculation than to solid work. The other possible explanation – that the authors should have been interested only in the higher level of mathematics – can be safely disregarded for the period before 1914, witness the many articles on elementary topics published in the same series.

The long 1930s – Neugebauer, Struve, Thureau-Dangin, and others

Beginning in 1929, the distinction between Assyriologists and historians of mathematics becomes irrelevant (for a while). This is the period when the advanced level of Old Babylonian and Seleucid mathematics was deciphered for good, after the modest but decisive beginnings made by Weidner, Zimmern, Ungnad, Gadd and Frank.

Otto Neugebauer (1899–1990), it is true, is normally counted as a historian of mathematics. If anything, historian of astronomy would be the correct denomination – as we shall see, mathematics only occupied a rather short stretch of his life. But he had

[30] The exceptions are vols 2, 16, 19–20 and 25, to which I have no access; there is no reason to believe they should change the general picture.

also been trained in Assyriology by none less than Anton Deimel (1865–1954), as he tells with gratitude in [1927: 5]. Vasilij Vasil'evič Struve (1889–1965) was an Egyptologist but had also been trained in Assyriology, which was soon to become his main field. Thureau-Dangin was one of the most eminent Assyriologists of his (and all) times, but the contrast between his works from the 1920s (and before) and those from the 1930s demonstrates how much the discussions (and competition) with Neugebauer and the perspective of the history of mathematics had changed his approach. Hans-Siegfried Schuster (1910–2002), who made an important contribution in c. 1929, became an Assyriologist but participated in Neugebauer's seminar in Göttingen (Kurt Vogel, personal information; [Neugebauer 1929: 80]); Heinz Waschow (1914–?) studied not only Oriental philology (including Assyriology) from 1930 until 1934 but also applied mathematics [Waschow 1936, unpaginated CV]. Albert Schott (1901–1945), the last of Neugebauer's contacts, had a strong interest in astronomy (but the numerous references to his assistance in [MKT] all refer to strictly philological matters). Kurt Vogel (1888–1985), who sometimes took part in the discussion, was a mathematician and historian of mathematics but also trained in Egyptian (as well as Greek, and later also medieval Italian and German) philology – not to speak of his war experience as a military engineer.

Since Neugebauer's person was all-important for what happened in the 1930s, some words about his background may be fitting. His *Doktorarbeit* from [1926] had dealt with the Egyptian fraction system, but already while working at it he had become interested in the mathematics of the Sumerian cultural orbit as a parallel that might throw light on Egyptian thought, and been convinced (with due reference to Thureau-Dangin) that metrology was all-important for the development of early mathematics [Neugebauer 1927: 5].

In 1929, he launched *Quellen und Studien zur Geschichte der Mathematik, Astronomie und Physik* together with Julius Stenzel (1883–1935) and Otto Toeplitz (1881–1940). Since the co-editors were 16 respectively 18 years older than Neugebauer, there can be little doubt that the initiative was his. He may not have written the *Geleitwort* ("accompanying observations"), but if not he must at least have agreed with it [Neugebauer et al 1929: 1–2]:[31] ⟦[32]⟧

[31] Some stylistic features do point to Neugebauer as the writer. As revealed by the quotation in note 51, at least Toeplitz also had an attitude in conflict with the present text. ⟦The third paragraph of the quotation may have been added by him, or meant to give space to his views.⟧

[32] [Durch den Titel "*Quellen und Studien*" wollen wir zum Ausdruck bringen, daß wir in der steten Bezugnahme auf die Originalquellen die notwendige Bedingung aller ernst zu nehmenden historischen Forschung erblicken. Es wird daher unser erstes Ziel sein, *Quellen* zu erschließen, d. h. sie nach Möglichkeit in einer Form darzubieten, die sowohl den Anforderungen der modernen Philologie genügen kann, als auch durch Übersetzung und Kommentar den Nichtphilologen in den Stand setzt, sich selbst in jedem Augenblick von dem Wortlaut des Originales zu überzeugen. Den berechtigten Ansprüchen *beider* Gruppen, Philologen und Mathematikern, nach wirklicher Sachkenntnis Genüge zu leisten, wird nur möglich sein, wenn es gelingt, eine enge *Zusammenarbeit* zwischen ihnen

[...]

Through the title “Sources and Studies” we want to express that we see in the constant reference to original sources the necessary condition for all historical research that is to be taken seriously. It will therefore be our first aim to make *sources* accessible, that is, in as far as possible to present them in a form that can meet the requests of modern philology but also, through translation and commentary, enable the non-philologist to always convince himself of the precise words of the original. To really meet the legitimate wishes of *both* groups, philologists and mathematicians, will only be possible if a genuine *collaboration* between them is brought about. To prepare that will be one of the most important tasks of our enterprise.

The technical realization of this programme we intend to achieve through the publication of two irregular publication series. One, A, will accommodate the major proper editions, containing the text in its original language, philological apparatus and commentary and a translation that is as faithful as possible, and which will make the contents of the text as effortless accessible also to the non-philologist as can reasonably be done. Each issue of these “Sources” is to be a self-contained piece. The issues of Section B, “Studies”, are to contain each a collection of articles having a closer or more distant relation to the material coming from the Sources.

The “Sources and Studies” are to provide contributions to the *history* of mathematics. But they do not address specialists of the history of science alone. They certainly intend to present the material that can *also* be useful for specialists. But they also address all those who think that mathematics and mathematical thinking is not only the concern of a particular science but also deeply connected with our total culture and its historical development, and that the attention to the *historical* genesis of mathematical thinking can provide a bridge between the so-called “sciences of the spirit” [*Geisteswissenschaften*] and the seemingly so a-historical “exact sciences”. Our final aim is to participate in the building of such a bridge.

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herzustellen. Diese anzubahnen soll eine der wichtigsten Aufgaben unseres Unternehmens sein.

Die technische Durchführung dieses Programmes denken wir uns so, daß in zwangloser Folge zwei Publikationsreihen erscheinen. Die eine, A, “*Quellen*”, soil die eigentlichen Editionen größeren Umfanges umfassen, enthaltend den Text in der Sprache des Originales, philologischen Apparat und Kommentar und eine möglichst getreue Übersetzung, die auch dem Nichtphilologen den Inhalt des Textes so bequem als irgend tunlich zugänglich macht. Jedes Heft dieser “Quellen” wird ein für sich geschlossenes Ganzes bilden – Die Hefte der Abteilung B, “*Studien*”, sollen jeweils eine Reihe von Abhandlungen zusammenfassen, die in engerem oder weiterem Zusammenhang mit dem aus den Quellen gewonnenen Material stehen können.

Die “Quellen und Studien” sollen Beiträge zur *Geschichte* der Mathematik liefern. Sie wenden sich aber nicht ausschließlich an Spezialisten der Wissenschaftsgeschichte. Sie wollen zwar ihr Material in einer Form darbieten, die *auch* dem Spezialisten nützen kann. Sie wenden sich aber weiter an alle jene, die fühlen, daß Mathematik und mathematisches Denken nicht nur Sache einer Spezialwissenschaft, sondern aufs tiefste mit unserer Gesamtkultur und ihrer geschichtlichen Entwicklung verbunden sind, daß in der Betrachtung des *geschichtlichen* Werdens mathematischen Denkens eine Brücke zwischen den sogenannten “Geisteswissenschaften” und den scheinbar so ahistorischen “exakten Wissenschaften” gefunden werden kann. Unser letztes Ziel ist, an einer solchen Brücke mit bauen zu können.⟧

[…]

So, a common endeavour between philologists and historians of mathematics was aimed at, for the benefit of both groups as well as a broader educated public. That those publications about Babylonian mathematics that appeared in the journal did not cast much light on the role of mathematics in general culture was not a result of failing will; as Neugebauer had to point out in [1934: 204], one should "not forget that we still know practically nothing about the whole setting of Babylonian mathematics within the context of general culture".

In the first issue, Neugebauer and Struve [1929] published an article "about the geometry of the circle in Babylonia" (actually also about other geometrical objects). Among the results is the identification of a technical term for the height of plane or solid geometric figures. The explanation is philologically mistaken, but as in the case of Weidner's similar errors this is not decisive – as pointed out by Thureau-Dangin [1932a: 80] in the note where he gives the correction.

The preceding article in the same issue is by Neugebauer alone [1929]. It offers a new analysis of some of Frank's texts, and manages to elucidate much which had remained in the dark for Frank. Neugebauer's main tool is of astonishing simplicity: he retains the sexagesimal shape of numbers, while Frank, in order to get something more familiar to a modern mathematical eye, had translated them into decimal numbers (and often translated them into a wrong order of magnitude, as observed above). Beyond that, Neugebauer offers a number of improved readings.[33] Some of the problems, it turns out, contain questions of the second degree. Neugebauer concludes (pp. 79*f*) in these words:

> One may legitimately say that the present text presents us with a piece of Babylonian mathematics that enriches our all too meagre knowledge of this field with essential features. Even if we forget about the use of formulas for triangle and trapezium, we see that complex linear equation systems were drawn up and solved, and that the Babylonians drew up systematically problems of *quadratic* character and certainly also knew to solve them – all of it with a computational technique that is wholly equivalent with ours. If this was the situation already in Old Babylonian times, hereafter even the later development will have to be looked at with different eyes. ⟦[34]⟧

A note added after the proofs had been finished then reveals that a text has been found

[33] "Moreover, even the readings themselves can be considerably improved, once the substantial contents has been elucidated" (p. 67)".

[34] ⟦Man darf wohl sagen, daß in den vorliegenden Texten ein gutes Stück babylonischer Mathematik zutage liegt, das geeignet ist, unsere nur allzu dürftigen Kenntnisse dieses Gebietes um wesentliche Züge zu bereichern. Ganz abgesehen von der Verwendung von Dreiecks- und Trapezformel sehen wir, daß komplizierte lineare Gleichungssysteme aufgestellt und gelöst werden, daß man ganz systematisch Aufgaben *quadratischen* charakters stellt und zweifellos auch zu losen verstand – und all dies mit einer Rechentechnik, die der Unseren völlig äquivalent ist. Bei einer solchen Lage der Dinge bereits in altbabyloniseher Zeit wird man in Hinkunft auch die spätere Entwicklung mit anderen Augen anzusehen lernen müssen.⟧

which *solves* mixed second-degree problems, referring to the essential role played Schuster for understanding this, while an article written by Schuster [1930] and appearing in the second issue analyses the solution of four such problems in a Seleucid text.

The conclusion just quoted announces the approach which was to be that of the 1930s. Since the meanings of terms for mathematical operations were derived from the numbers that resulted from their use, the operations were almost by necessity understood as arithmetical operations; as a rather natural consequence, problems were understood as (arithmetical) equations and equation systems. And of course Neugebauer, as everybody else, expressed amazement that complicated matters such as second-degree equations were dealt with correctly.

Neugebauer knew very well that Old Babylonian (1800–1600 BCE, according to the "middle chronology") and Seleucid (third-second century BCE) mathematics were formulated in different terminologies. But he believed that the difference was one of terminology and implicitly supposed, as we see, that there must have been steady progress of knowledge from the early to the late period.

A number of publications from Neugebauer's hand (and three from that of Waschow [1932a; 1932b; 1932c]) followed in *Quellen und Studien* B until 1936 (in vol. 4, from 1937–38, Neugebauer has turned completely to astronomy). In 1935–37, Neugebauer also published in *Quellen und Studien* A the monumental *Mathematische Keilschrifttexte* [MKT I–III]. They can be said to bring to completion the interpretation of his [1929]-paper; but they also make clear that Neugebauer had not left behind his interest in metrology and other simple matters – he was not looking merely after matters that might be seen as analogous to modern equation algebra. The conclusion of volume III [MKT III, 79*f*] gives two warnings to the reader. Firstly, that MKT is a *source edition* – "It does not belong among the tasks that I have proposed for myself in this edition to develop the consequences which can be drawn from this text material". ⟦[35]⟧ Secondly,

> Since our knowledge of these things is of relatively recent date, and current datings had to be pushed considerably, there is an obvious danger to overestimate the mathematics of the Babylonians. In order to somehow gloss over the lack of a basis in sources, many familiar books change elementary mathematical things into "propositions" and "discoveries" that must be ascribed to great men. It seems to me that we should not stamp the Babylonians as such discoverers. What is often overlooked and cannot be sufficiently emphasized is the terrible difficulty and slowness of the development of the very simplest fundamental mathematical concepts, first of all of a genuine computational technique. This, however, is not the achievement of a single person; it can only be understood within a historical process, inextricably attached to the emergence of a whole culture. Once this stage has been reached, then there is nothing in Babylonian mathematics that must be seen as an unexpected brilliant performance. ⟦[36]⟧

[35] ⟦Es gehört nicht zu den Aufgaben, die ich mir in dieser Edition gestellt habe, die Konsequenzen zu entwickeln, die sich nun aus diesem Textmaterial ziehen lassen.⟧

[36] [Da unsere Kenntnis von diesen Dingen relativ neu ist, und übliche Datierungen erheblich

The last sentence refers to Neugebauer's hypothesis (which he considers an established fact) [MKT III, 79],

> that Babylonian mathematics first grew out of the numerical methods of sexagesimal calculation, the practical advantage of which was fully understood, and then, decisively sustained by the possibilities offered by ideographic writing, soon reached a strongly "algebraic"[37] treatment of purely mathematical problems that were of or could be reduced to linear or quadratic character. ⟦[38]⟧

Thureau-Dangin, as we have seen, had been interested in metrology and mathematical techniques since [1897]. He started dialogue with Neugebauer in [1931] (making a philological correction that also concerns Frank, whom he does not mention). His weighty *Esquisse d'une histoire du système sexagésimal* [1932c], however, is rather a crown on his work from the 1920s, describing both the sexagesimal place-value system and the non-positional system and non-sexagesimal fractions, together with their uses.[39] But very soon, Thureau-Dangin moved from purely philological emendations and addenda to the publication of new mathematical texts and to considerations of their mathematical substance – for example in [1932b], [1934] and [1936] – and to a synthesis about "the

verschoben werden mußten, liegt die Gefahr nahe, die babylonische Mathematik zu überschätzen. Um die leere der quellenmäßigen Grundlagen einigermaßen zu überdecken, sind in vielen geläufigen Büchern oft die elementaren mathematischen Dinge zu "Sätzen" und "Entdeckungen" gemacht worden, die großen Männern zugeordnet werden mußten. Mir scheint, man muß jetzt nicht die Babylonier zu solchen Entdeckern stempeln. Was man oft übersieht und nicht genug hervorheben kann, ist die ungeheure Schwierigkeit und Langsamkeit der Entwicklung der *allereinfachsten* mathematischen Grundbegriffe, vor allem einer wirklichen Rechentechnik. Dies ist aber nicht die Leistung Einzelner, sondern nur aus historischen Prozessen verständlich, die mit der Entstehung einer Kultur überhaupt unlöslich verknüpt sind. Ist dieses Stadium erst einmal erreicht, so bedeutet die babylonische Mathematik an keiner Stelle etwas, was als unerwartete Glanzleistung angesehen werden müßte.]

37 [The quotes around the word *algebraic* indicate that Neugebauer refuses to make hypotheses about which kind of algebraic thought is involved in the texts. The many algebraic formulas in his commentary are not meant to map the thinking of the authors of the texts; they show why the calculations are pertinent (or, rarely, why they are not).]

⟦Neugebauer's explanations of his usage in other publications from the epoch are presented in article II.5.⟧

38 ⟦daß die babylonische Mathematik zunächst aus den numerischen Methoden des sexagesimalen Zahlenrechnens erwachsen ist, dessen praktische Vorteile man voll erkannt hat und dann rasch, entscheidend gefördert von der ideographischen Ausdrucksmöglichkeit, zu einer stark "algebraisch" gerichteten Behandlung rein mathematischer Aufgaben linearen und quadratischen (bzw. darauf reduzierbaren) Charakters gelangt ist⟧.

39 This booklet had no strong impact – it drowned in the fury surrounding the new discoveries of the time. However, a revised English translation (including much about the Babylonian "algebra") appeared in *Osiris* in [1939] on George Sarton's initiative (p. 99).

method of false position and the origin of algebra" [1938] along with the source edition *Textes mathématiques babyloniens* [TMB] from the same year.[40] In several of these works Thureau-Dangin can be seen to be much less wary than Neugebauer when speaking of the algebraic thinking of the Babylonians. He also shows himself familiar with a very wide range of later mathematical sources, from Diophantos, Ptolemy and al-Khwārizmī to Stevin and Wallis.

Then, in 1937–38, this "heroic period" ended abruptly. In 1945, it is true, Neugebauer and Abraham Sachs published *Mathematical Cuneform Texts* [MCT],[41] an edition of

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40 This is what Wolfram von Soden (1908–1995) has to say about the purpose of this parallel edition:

> This new work is not meant to replace Neugebauer's MKT; indeed, the phototypes and autographs are not repeated, nor are all texts treated anew. Th.-D.s aim was instead, leaving the arithmetical tables completely aside (only the introduction speaks briefly about them) to make those problem texts that are sufficiently well preserved to allow at least a generally satisfying understanding available to as many researchers as possible in a cheaper edition, since the exorbitant price of the MKT unfortunately hampers its wider diffusion.
>
> ⟦Dieses neue Werk hat nicht die Aufgabe, Neugebauer's MKT zu ersetzen; werden doch weder die Lichtdrucke und Autographien der Texte wiederholt noch auch alle Texte neubearbeitet. Th.-D. 's Ziel war es vielmehr, unter vollständiger Beiseitelassung der Rechentabellen (nur die Einleitung geht kurz auf sie ein) diejenigen Aufgabentexte, deren Erhaltungszustand ein wenigstens im großen und ganzen befriedigendes Verständnis ermöglicht, in einer wohlfeileren Ausgabe möglichst vielen Forschern zugänglich zu machen, da der leider so hohe Preis der MKT ihrer weiteren Verbreitung im Wege steht.⟧

But further:

> While thus the specialist researcher will also in future not be able to give up Neugebauer's MKT as the complete source collection, with the just mentioned exception [two small texts from Susa with area calculations published by Vincent Scheil in 1938], then precisely he will also not be able to pass over Th.-D.'s new edition, as nobody will be able to digest in brief the large number of corrected readings and the immensely weighty lexical, grammatical and substantial observations, masterly concise though they are.
>
> ⟦Wird also der Fachforscher nach wie vor auf Neugebauer's MKT als das, abgesehen von der eben erwahnten Ausnahme, immer noch vollstandige Quellenwerk nicht verzichten konnen, so kann gerade er aber auch nicht an Th.-D.'s neuer Ausgabe vorbeigehen, da niemand die große Zahl der berichtigten Lesungen und die vielen, bei aller meisterhaften Knappheit ungeheuer inhaltreichen lexikalischen, grammatischen und sachlichen Anmerkungen auf einmal verarbeiten kann.⟧

In Thureau-Dangin's own words [TMB, p. xl]:

> The present volume contains no text which has not been published elsewhere in its original form [that is, without a translation of ideograms into syllabic Akkadian]. The main task I have set myself while preparing it has been to make documents accessible to the historians of mathematical thought.
>
> ⟦Le present volume ne comprend aucun texte qui n'ait été édité ailleurs dans sa forme originale. Le principal objet que je me suis proposé en le rédigeant a été de mettre des documents à la disposition des historiens de la penseé mathématique.⟧

41 Curiously enough, MCT is much less afraid of ascribing modern mathematical concepts to the

texts from American collections that had not been included in MKT, and Neugebauer's popularization *The Exact Sciences in Antiquity* from [1951] (revised in 1957) contains a chapter on the topic; but apart from that Neugebauer only published two or three small items on Babylonian mathematics after 1937, dedicating instead himself wholly to the history of astronomy (and to the launching of the *Mathematical Reviews*, after the National Socialists had seized power over his earlier creation *Zentralblatt für Mathematik*). Schuster published nothing in the area after 1930 (he is better known as a Hittitologist), while Waschow entered the army in 1934, writing at the same time a dissertation on Kassite letters [1936].[42] In 1938 he published a book (*4000 Jahre Kamp fum die Mauer*) about siege techniques since Old Babylonian times, after which I have been unable to find information about his fate (I would guess that as an officer he lost his life during the war). Albert Schott concentrated on astronomy, while Kurt Vogel's *Habilitationsschrift* [1936] dealt with Greek logistics. Thureau-Dangin returned to other Assyriological questions.

In 1961, Evert Bruins (1909–1990) and Marguerite Rutten (1898–1984) published a volume with mathematical texts from Susa. They had started work around 1938, and Bruins was very proud of having been trained by Thureau-Dangin.[43] No wonder that the volume is wholly in the style of the 1930s – yet on a much lower philological level than what had been published during this epoch, and full of groundless speculations and misreadings (with interspersed good ideas, it should be added – ⟦in consequence, Bruins's interpretations and commentary should not be neglected but read with critical caution⟧).

Assyriologists, 1940–1980

After 1940, Assyriologists would usually put aside any tablet containing too many numbers in place-value notation as "a matter for Neugebauer" (thus Hans Nissen, at one of the Berlin workshops on "Concept Formation in Mesopotamian Mathematics" in the 1980s). In consequence, very few new texts (apart from the batch from Susa) were published during the following four decades.

There is one important exception to this generalization (and a few other less important

Babylonians than Neugebauer had been in the 1930s – such as logarithms, p. 35, cf. [MKT I, 363–365]. Whether this is due to Sachs's influence or Neugebauer himself had been convinced by what others had read into [MKT] I am unable to say.

42 The edition of one long Seleucid text (BM 34568) in [MKT III: 14–22] is also, according to Neugebauer, "apart from a few trifles due to Herrn Dr. Waschow". This work must be dated between 1935 and 1937.

43 He returns to this link time and again in the numerous angry letters I have from his hand. I suppose he can be believed on this account, his general unreliability notwithstanding.

According to the preface [TMS, xi], Rutten made the hand copies and collaborated with Bruins on the translation. However, already the translation of word signs into Akkadian contains so many blunders of a kind no competent Assyriologist would commit that Bruins can be clearly seen to have had the upper hand concerning everything apart from the hand copies.

ones). Between 1950 and 1962 the Iraqi Assyriologist Taha Baqir (1912–1984) published four papers in the journal *Sumer* with new texts excavated between 1945 and 1962 [Baqir 1950a; 1950b; 1951; 1962]. These were highly important for several reasons: They came from a region from which until then no mathematical texts were known; like the Susa texts their provenience was known, since they were regularly excavated; but unlike what had happened to the Susa texts, the excavations were carefully made, for which reason the texts can also be dated.[44] Von Soden suggested a number of improved readings with implications for the interpretation in [1952],[45] and Bruins [1953] tried (as usually) to show that everything von Soden had said was absurd; but the impact of Baqir's papers on historians of mathematics was almost imperceptible – one joint article by the mathematician Karl-Bernhard Gundlach (*1926) and Wolfram von Soden from [1963] deals with one of Baqir's texts and a text from Susa.

Already in 1945, Goetze had contributed a chapter "The Akkadian Dialects of the Old-Babylonian Mathematical Texts" to [MCT, 146–151]. In contrast to the volume as a whole, this chapter falls outside what had been done in the 1930s.[46] In these pages, Goetze makes a careful classification of all Old Babylonian mathematical texts known by then that contained enough syllabic writing to allow orthographic analysis.

Occasionally, some Assyriologist publication would touch at numero-metrological questions, but not very often.[47] We have to wait until the early 1970s before an Assyriologist took up systematically the kind of work which Thureau-Dangin and others had pursued in the 1920s. In [1971], Marvin Powell submitted his doctoral dissertation on *Sumerian Numeration and Metrology*, soon followed by a major paper on "Sumerian Area Measures and the Alleged Decimal Substratum" [1972a]. Also in [1972] followed a short paper from his hand [1972b] on "The Origin of the Sexagesimal System: the Interaction of Language and Writing", and in [1976] a longer one on "The Antecedents of

44 A further text covering three tablets was found on the ground, apparently left behind by illegal diggers as too damaged. It was published by Albrecht Goetze (1897–1971) in [1951].

45 Until then, von Soden had never worked directly on mathematical questions himself; but he had always been interested in the topic, as can be seen from his careful and extensive reviews of MKT [1937] and TMB [1939]. He also made a review of TMS in [1964], an indispensable companion piece to the edition itself.

46 The outcome can be seen as an elaboration of the division of the corpus into a "northern" and a "southern" group which Neugebauer had suggested in [1932: 6*f*]; but Neugebauer's arguments had been of a wholly different nature.

47 In 1978–79, Carlo Zaccagnini thus published at least four papers on the metrologies of peripheral areas.

I disregard publications in Russian, most noteworthy of which is [Vajman 1961] – my reading of Russian, which reached the level of "rudimentary" 25 years ago, has vanished completely since then for lack of practice.

An exhaustive survey, often with discussion, of all at least minimally pertinent publications (also those in Russian) for the period 1945–1980 will be found in [Friberg 1982: 67–130].

Old Babylonian Place Notation and the Early History of Babylonian Mathematics", published in *Historia Mathematica.* The latter two articles took up topics which both Thureau-Dangin and Neugebauer had tried their teeth on around 1930, yet for lack of adequate sources from the third millennium without reaching solid results. In [1978] and [1979], Jöran Friberg, paradoxically a mathematician of merit and no Assyriologist but using approaches and methods that had been characteristic of the Assyriological tradition, made a break-through on the numerical and metrological notation of the fourth millennium; with minor corrections, his results were later confirmed by the Berlin Uruk project [Damerow and Englund 1987].

Historians of mathematics

During the same decades, little original work on Mesopotamian mathematics was made by scholars who would primarily be classified as historians of mathematics. They can be seen to have regarded the analysis in MKT and MCT as exhaustive – as it actually was on most accounts, as long as Neugebauer's and Sachs's approach *as understood by historians of mathematics* was taken for granted.

There are again a few exceptions. The most substantial of these is a sequence of proposed interpretations of the famous text Plimpton 322, originally published in [MCT, 38–41] and considered there as an early instance of number theory. Most noteworthy during the early period is [Bruins 1957], where a derivation of its Pythagorean triples from pairs of reciprocals is proposed (an interpretation which has been confirmed with modifications and extra arguments since then by Friberg [1981] and Eleanor Robson [2001]). It may be considered a manifestation of the new "modernizing" orientation of MCT that this possibility had been overlooked, given that Neugebauer had believed in the 1930s that the whole second-degree "algebra" came from the place-value system (above, text around note 37).

Other exceptions are a publication of some merit by Solomon Gandz [1948], sent to the journal *Osiris* around 1938 but then delayed by the war; and a republication of one of Gandz's results in [1955] by Peter Huber, who had not noticed Gandz's work.

However, while little new research was done on Mesopotamian mathematics by historians of mathematics, "Babylonian mathematics" was close to becoming the standard introduction to histories of mathematics.

The way it was dealt with is well illustrated by Asger Aaboe's (1922–2007) *Episodes from the Early History of mathematics* [1964]. Aaboe starts by observing that a modern schoolboy transposed to Babylonia or ancient Greece would find the "physics" of classical Antiquity utterly unfamiliar (p. 1). Mathematics, however, would

> look familiar to our schoolboy: he could solve quadratic equations with his Babylonian fellows and perform geometrical constructions with the Greeks. This is not to say that he would see no differences, but they would be in form only, and not in content; the Babylonian number system was not the same as ours, but the Babylonian formula for solving quadratic equations is still in use.

That is, firstly: mathematics is a topic outside history, changing “in form” only. Secondly, the “contents” of mathematics consists in “formulae”. Aaboe himself may have believed to continue Neugebauer’s approach, but in reality the programme of *Quellen und Studien* has been betrayed. The “seemingly so a-historical ‘exact sciences’” have become, precisely, *a-historical.* The lack of information about the social context of Babylonian mathematics is no longer a deplorable fact, as for Neugebauer in 1934 – the absence of information about its creators is just taken note of, while institutional setting etc. constitute non-questions.[48]

Turning to the contents, we see that the reader learns that the sexagesimal place-value system is *the* Babylonian number system. Aaboe ignores that it was used only for intermediate calculations; in school; and in (late Babylonian) mathematical astronomy. He is unaware that a different system was used in “real-life” juridical and economical documents[49] – he only knows about inconsistency and failing rationality.[50]

When going beyond place-value computation Aaboe deals with three more advanced topics. The first is treated through two “algebraic” problems about square areas and appurtenant sides from the tablet BM 13901, quoted in Neugebauer’s translation but then immediately transformed into modern algebraic symbols; the second is YBC 7289, a tablet showing a square with diagonals and three inscribed numbers corresponding to the side, the diagonal and their approximate ratio, which allows immediate discussion in terms of $\sqrt{2}$; the third is the calculation of a height in an isosceles trapezium. It is mentioned (p. 23) that the first two are Old Babylonian and the third Seleucid, but it is claimed (as does *not* correspond to the information that could be extracted from MKT, and as is in any case quite wrong) that all three could have been written in any period. The conclusion discusses “algebra” once again, and states that

> Quadratic equations are often given in the equivalent form of two equations with two

[48] “Of the creators of Babylonian mathematics we know nothing whatsoever except the result of their work” (p. 6). That the texts are school texts is intimated by photos of presumed schoolrooms from Mari (which are actually store-rooms) and occasional references to a “schoolboy” – but schooling seems to be just as timeless as mathematics. In 1964, we may observe, more was known about the Old Babylonian scribe school than in 1934 – cf. [Kramer 1949], [Falkenstein 1953] and [Gadd 1956].

[49] However, all of this is described in [Thureau-Dangin 1939], who distinguishes the “abstract” (namely place-value) system “intended only to serve as an instrument of calculation” (p. 117) from the ordinary sexagesimal but non-positional system.

[50] “It should be added that an entirely consistent use of the sexagesimal system is to be found only in the mathematical and astronomical texts, and even in astronomical texts one can find year numbers written as, e.g., 1-*me* 15 (meaning 1 hundred 15) instead of 1,55. In practical life the Babylonians showed the same profound disregard for rationality in their use of units for weight and measure as does the modern English-speaking world” (p. 20). The year number in question is written in precisely that number system which Hincks had deciphered in 1847, cf. note 16 – the very first contribution to the study of Assyro-Babylonian mathematics!

unknowns, such as

$$x + y = a,\ xy = b,$$

whence one finds immediately that x and y are the solutions of

$$z^2 - az + b = 0$$

without mentioning that such problems deal with rectangular areas and sides, nor that the “one” who “finds immediately” is Aaboe himself or some other modern calculator, and that no corresponding step can be found in the original texts.

Aaboe’s book was intended as supplementary high-school reading, and can thus be understood according to Toeplitz’ “genetic method” [1927], the introduction of modern concepts through pedagogically motivating idealized quasi- or pseudo-history.[51] However, the typical general histories of mathematics published during the period share the basic character of Aaboe’s presentation – see my anatomies of [Hofmann 1953], [Boyer 1968] and [Kline 1972] in [Høyrup 2010]. Only another book written for the high-school level (but here the German *Gymnasium*), Vogel’s *Die Mathematik der Babylonier* from [1959] stands out – with its awareness that the place-value system was a scholarly system; because of its interest in metrologies and in computations dealing with everyday life; and with its discussions of ways of thought.[52] Vogel, indeed, had worked on the material himself already in the 1930s, and he had always been interested in ways of thought and in the mathematics of practical life, while Aaboe had only worked on Seleucid astronomical texts, and Joseph Ehrenfried Hofmann (1900–1973), Carl Boyer (1906–1976) and Morris Kline (1908–1992) at best on Neugebauer’s translations – but apparently more often on his popularizations and his explanatory commentaries without distinguishing the latter from what was done in the sources.

After 1980

After c. 1945, the historiography of Mesopotamian mathematics had thus been an almost dead topic, little considered by Assyriologists and treated under the point of view of “historical mathematics” by those who otherwise wrote about the history of mathematics.[53]

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51 “Nothing is farther from me than to teach a history of the infinitesimal calculus; I myself as a student ran away from a lecture of that kind. History is not at stake, but the genesis of problems, acts and demonstrations, and the decisive turning points in this genesis” [Toeplitz 1927: 94].

52 Dirk Struik's (1894–2000) *Concise History of Mathematics* from [1948] deals with Mesopotamian mathematics too briefly to allow description in similar depth (pp. 23–32). Struik’s layout, however, is similar: The analysis is embedded in general social history; non-positional as well as place-value system are described; but like Vogel, Struik has no possibility to go beyond Neugebauer.

53 Boyer had written about the “*concepts* of the calculus” [1949], and Kline’s title refers to “mathematical *thought*”. Hofmann had written among other things about Ramon Lull’s squaring of the circle in [1942], and had tried there to penetrate the thinking and motives of Lull (without which he would indeed have been unable to conclude anything of interest).

Beginning with the above-mentioned works of Powell and Friberg this situation was going to change once again. But this is where my own work in the field started, first on the connection between mathematics, general socio-cultural context and educational situation, from 1982 onward on the concepts and operations of Old Babylonian mathematics, so here I shall stop – adding only that in recent years a number of younger scholars trained in mathematics as well as Assyriology have entered the field, adding new approaches and returning to the Assyriological questions of the earlier twentieth century with the luggage of a century of extra textual and archaeological discoveries, thus being able to integrate the mathematical dimension with studies of social, political and economic history. The field remains alive – but mathematicians may not find it very interesting for their purpose.

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Chapter 28 (Article II.11)
Fibonacci – Protagonist or Witness? Who Taught Catholic Christian Europe About Mediterranean Commercial Arithmetic?

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

Fibonacci during his boyhood went to Bejaïa, learned about the Hindu-Arabic numerals there, and continued to collect information about their use during travels to the Arabic world. He then wrote the *Liber abbaci*, which with half a century's delay inspired the creation of Italian abbacus mathematics, later adopted in Catalonia, Provence, Germany etc. Hindu-Arabic numerals, and Arabic mathematics, were thus transmitted through a narrow and unique gate.

This piece of conventional wisdom is well known – too well known to be true, indeed. There is no doubt, of course, that Fibonacci learned about Arabic (and Byzantine) commercial arithmetic, and that he presented it in his book. He is thus a witness (with a degree of reliability which has to be determined) of the commercial mathematics thriving in the commercially developed parts of the Mediterranean world. However, much evidence – presented both in the pages of his own book, in later Italian abbacus books and in similar writings from the Iberian and the Provençal regions – shows that the *Liber abbaci* did not play a central role in the later adoption. Romance abbacus culture came about in a broad process of interaction with Arabic non-scholarly traditions, at least until c. 1350 within an open space, apparently concentrated around the Iberian region.

Contents

BENNO ARTMANN
in memoriam

A disclaimer

In these times of rampant publicity and rampant legal complaints, it is not uncommon to run into disclaimers explaining in small print what the wonderful product does *not* promise.

Let me start by stating, in normal font size however, that the following offers *elements* that have to go into a synthetic answer, but too few and too disparate to allow the construction of this synthesis.

Fibonacci's supposed role

In the introduction to Fibonacci's *Liber abbaci,* we read the following:[1]

> After my father's appointment by his homeland as state official in the customs house of Bugia for the Pisan merchants who thronged to it, he took charge; and, in view of its future usefulness and convenience, had me in my boyhood come to him and there wanted me to devote myself to and be instructed in the study of calculation for some days. There, following my introduction, as a consequence of marvelous instruction in the art, to the nine digits of the Hindus, the knowledge of the art very much appealed to me before all others, and for it I realized that all its aspects were studied in Egypt, Syria, Greece, Sicily, and Provence, with their varying methods; and at these places thereafter, while on business, I pursued my study in depth and learned the give-and-take of disputation.

This can be combined with the prevalent idea about the origin of Italian abbacus mathematics, for instance as expressed recently by Elisabetta Ulivi [2004: 44][2] in her explanation that

> the name "abbacus school" designates those secondary-level schools that were essentially dedicated to practical arithmetic and geometry and were in the tradition of Leonardo Pisano's *Liber abbaci* and *Practica geometriae.*

Similarly, Warren Van Egmond asserted in [1980: 7] that all abbacus writings "can be regarded as [...] direct descendants of Leonardo's book".[3]

If the Fibonacci-quotation (in particular in the usual reading, where neither "Greece" nor Provence are noticed) is combined with the opinion expressed by Ulivi and Van

[1] I quote Richard Grimm's translation [1976: 100], based on a critical edition of the introduction based on all the manuscripts that contain it.

[2] My translation, as everywhere else in the following unless a translator is identified.

[3] More examples, also drawn from respected colleagues, are quoted in [Høyrup 2005: 24–26] and [Høyrup 2007: 30 n.69].

Egmond, then it becomes clear that Fibonacci's *Liber abbaci* was the gate through which practical arithmetic was transmitted from the Arabic world to Italy (and from there to the rest of Christian Europe[4]).

In 1997–98, I discovered that this story is impossible if we look at the specific case of abbacus *algebra,* but apart from that I followed Descartes' strategy as set forth in the *Discours de la méthode* [ed. Adam & Tannery 1897: VI, 23], to observe the customs and opinions of those among whom I lived until close analysis of the matter would force me to change my observances. When subsequently I *was* forced to do so, I started thinking about the origin of the conventional wisdom.

Part of the explanation is of course that it is easier to look for the lost doorkey within the cone of the street lamp than outside it, in the darkness – to which comes what at another occasion [Høyrup 2003: 10] I have called "the syndrome of the great book", namely "the conviction that every intellectual current has to descend from a *Great Book* that is *known to us*".

The only apparently positive evidence comes from a *Livero de l'abbecho* from c. 1300 (Florence, Riccardiana ms. 2404, ed. [Arrighi 1989]) which claims in its first line to be "according to the opinion" of Fibonacci.[5] Close analysis of the treatise [Høyrup 2005] shows, however, that this evidence is fallacious. The text moves on two distinct levels, one elementary, the other advanced. The elementary level corresponds to the curriculum of the abbacus school as we know it from two documents.[6] Here we find the rule of three; metrological shortcuts; exchange and barter; elementary alligation; simple interest and elementary composite interest.[7] As can be seen both from the absence of shared problems and from the way mixed numbers are written, this level is fully independent of Fibonacci – except for a misshaped compromise between the normal writing of mixed concrete numbers and Fibonacci's notation for pure mixed numbers, on which below, note 21. On the other hand, everything on the advanced level is borrowed from the *Liber abbaci* (excepting a final chapter containing mixed sophisticated problems, some of which come from other sources), often demonstrably without understanding.[8] The

4 Here and everywhere in the paper, "Christian Europe" refers narrowly to *Catholic* Christian Europe.

5 "Quisto ène lo livero de l'abbecho secondo la oppenione de maiestro Leonardo de la chasa degli figluogle Bonaçie da Pisa" [ed. Arrighi 1989: 9]. The date of the manuscript is discussed in [Høyrup 2005: 27 n.5, 47 n.37].

6 One [ed. Arrighi 1967] is from the earlier 15th, the other [ed. Goldthwaite 1972: 421–425 n.15] from the early 16th century; however, nothing suggests the curriculum to have been reduced in the meantime (nor broadened, for that matter).

7 The curriculum also encompassed the Hindu-Arabic number system with appurtenant calculation, which Fibonacci is often supposed to have brought to Italy. This is absent from the treatise. I shall not discuss Fibonacci's role in this domain, just point out that even here there is no positive evidence that his influence was important.

8 Fibonacci's composite fractions are understood as normal fractions, which implies that the compiler

Fibonacci material thus serves as adornment; it is quite fitting that the copy we possess is a beautiful manuscript on vellum. It follows that the *Liber abbaci* was famous a small century after it was written, and Fibonacci's name a superb embroidered cloak in which the abbacus author in question found it convenient to wrap his book; but also that what this author taught in the abbacus school, and the mathematics he understood, had a different basis.

So far, this concerns a single author (better perhaps, compiler), albeit one of the two earliest abbacus authors whose work has reached us.[9] However, no other abbacus author raises similar claims except a 15th-century encyclopedia where the claim is even more misleading,[10] and no other author offers material directly copied from the *Liber abbaci,* except Benedetto da Firenze and a near-contemporary of his, who copy whole sections (the algebra, and chapter 15 part 1, on proportions), but whose own work remains independent of Fibonacci and well within the current abbacus tradition.[11] The situation is slightly different as regards Fibonacci's *Pratica geometrie,* inasfar as three 14th- and 15th-century treatises were drawn from it [Hughes 2010].[12] However, normal abbacus geometries borrow nothing directly (and plausible very little indirectly) from Fibonacci.

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can never have followed those numerous calculations where they occur. The occasional algebraic *cosa* of which Fibonacci makes use when applying the *regula recta* (first-degree algebra) is either skipped, or the role of this "thing" as a representative of an unknown quantity is not understood.

[9] The other early book is the *Columbia Algorism,* on which below. On the plausible re-dating of the original of this treatise (of which we possess a 14th-century copy) to the years 1285−90, see [Høyrup 2007: 31 n. 70] ⟦or article I.12⟧.

[10] Vatican, Ottobon. lat. 3307, which presents itself (fol. 1^r) as *Libro di praticha d'arismetrica, cioè fioretti tracti di più libri facti da Lionardo pisano*.

[11] Benedetto's original autograph of his *Praticha d'arismetricha* from 1463 is contained in Siena, Biblioteca degli Intronati, L IV 21; a detailed description is [Arrighi 2004: 129–159]; the other "abbacus encyclopedia", roughly contemporary and anonymous (Florence, Bibl. Naz., Palatino 573; also original autograph), is described in detail in [Arrighi 2004: 161–195]. The evidence that both manuscripts are their respective author's original autograph's is presented in [Høyrup 2010: 32, 39] ⟦= article II.13⟧.

It is possible (and even plausible) that both draw their copy of Fibonacci from Antonio de' Mazzinghi's lost *Gran trattato* from the later 14th century. But even Antonio's own algebra (as we know it from extracts in the two encyclopediae that were just mentioned) owes nothing to Fibonacci.

[12] Fibonacci's *Pratica* is also used so faithfully in Luca Pacioli's *Summa* [1494] that misprinted letters in Pacioli's diagrams can often be corrected by means of Boncompagni's edition of the *Pratica* [1862]! ⟦Cf. article I.3.⟧

Then whence? Algebra as initial evidence

This was the negative part of my argument, which invites a search for alternative gates (or even "open spaces").

I shall start where my own exploration began, with algebra. None of the two earliest texts contain any algebra – and the compiler of the *Livero*, as we have seen, does not even know enough about the topic to recognize an algebraic *cosa.* The earliest abbacus algebra is contained in the Vatican manuscript (Vat. lat. 4826) of Jacopo da Firenze's *Tractatus algorismi,* written in Montpellier in 1307.[13]

On all accounts (except that it deals with the six fundamental first- and second-degree "cases", but then not only with these, and that it uses the term *censo* for the second power), this algebra differs fundamentally not only from Fibonacci's algebra but also from the Latin translations of al-Khwārizmī – see [Høyrup 2007: 147−159]. It is also very different from Abū Kāmil's algebra, and has only few features in common with al-Karajī's *Kāfī.* Its ultimate root is obviously Arabic algebra. However, its closest Arabic source cannot be any of the erudite treatises that have come down to us; instead, we must think of a mathematical practice in which algebra and commercial calculation were merged.

Moreover, its closest Arabic source cannot be the *immediate* source. Technical works translated directly from the Arabic always contained Arabic loanwords for some of their technical terms; however, no such terminological borrowings are present in the Vatican (or other early abbacus) algebra. The *immediate* source must hence be an environment where algebra was already spoken of in a Romance language. Since Jacopo wrote his treatise in Montpellier (located in Provence, but politically belonging to the Aragon-Catalan kingdom), this environment can reasonably be assumed to have been situated somewhere in the Ibero-Provençal area.

That observation brings to mind Fibonacci's claim to have also learned about the use of the Hindu-Arabic numeral system *in Provence,* a claim that mostly goes unnoticed. Indeed, if 15th-century Provencal mathematics of the abbacus type took its inspiration from Italy, and the Italians had their practice from Fibonacci, what could Fibonacci have learned in Provence? One at least of the premises for this paradox has to be given up.

[13] The Vatican manuscript can be dated by watermarks to c. 1450. However, stylistic analysis strongly suggests that its algebra belongs together with the rest of the treatise, and that the two manuscripts from which the algebra section is absent are secondary redactions – see [Høyrup 2007: 5–25]. Van Egmond [2009] rejects this conclusion, but with arguments that are refuted by his own earlier publications (and by the sources to which he appeals) – see [Høyrup 2009]. In any case, the Vatican algebra must belong to the earlier 14th century. Moreover, other abbacus writing from the earlier 14th century share those of its characteristics that enter in the present argument; for our actual purpose, the identity of its author, and even the question whether it is really the earliest abbacus presentation of algebra, are therefore immaterial.

One early manuscript of the *Liber abbaci*[14] contains another reference to an unexpected locality: the ninth chapter does not simply begin with the words *Incipit capitulum nonum de baractis mercium atque earum similium* as found in [Boncompagni 1857: 118][15] but *Hic incipit magister castellanus. Incipit capitulum nonum de baractis mercium atque earum similium.* It is difficult to see why a copyist should insert a claim that a certain chapter was copied from a Castilian master if the claim was not in his original; if he did, it would at least show that he knew about such a Castilian treatise and believed to recognize its contents in Fibonacci's text. The passage thus offers evidence of Castilian writing on barter, probably before 1228 (or even 1202, the date of the first version of the *Liber abbaci,* now lost), and in any case before the end of the 13th century.

Direct evidence for Iberian algebra integrated with commercial arithmetic goes back to the 12th century – but to Arabic practice. I refer to the *Liber mahameleth,* a 12th-century Latin treatise whose title points to Arabic commercial mathematics (*mu'āmalāt*). A systematic presentation of algebra is lost in all manuscripts but repeatedly referred to; since it presents problem types and techniques not dealt with in al-Khwārizmī's algebra, this work cannot be what is spoken of.[16] Further, there are repeated references to Abū Kāmil. However, there are also copious algebraic problems of a kind which we do not find in al-Khwārizmī nor in Abū Kāmil, involving square roots of prices, profits and wages; such problems, though not very common, also turn up in abbacus algebra, starting with Jacopo (assuming that the Vatican algebra is really his).

There is some evidence for further influence from the Maghreb on 14th-century developments in abbacus algebra.[17] Firstly, apparent setoffs of the incipient symbolism developed in the Maghreb in the later 12th century turn up in various Italian manuscripts in the course of the 14th century, (whereas Jacopo's algebra is totally deprived of symbolism) – see [Høyrup 2010: 16–25] ⟦= article II.13⟧. The scattered character of these setoffs suggest interaction within an open space during the first half of the century,

[14] Vatican, Pal. lat 1343, (new) fol 47^r–47^v. This ⟦incomplete⟧ manuscript is from the late 13th century and thus one of the two oldest manuscripts (and older than the one used by Boncompagni for his edition. Boncompagni already mentioned the passage in [1851: 32], but even this piece of disturbing information has been displaced from historians' collective memory.

[15] Henceforth, I shall refer identify passages in the *Liber abbaci* by simple page number, always referring to [Boncompagni 1857].

[16] In one place [ed. Vlasschaert 2010: $_{\text{II}}$210*f*], a problem is solved by means of several unknowns called *res* and *nummus,* in a way told to have been taught in the algebra. Elsewhere, [ed. Vlasschaert 2010: $_{\text{II}}$427] the solution of an indeterminate problem is said to follow a method explained in this same chapter.

[17] Since key figures like ibn al-Yāsamin were active on both sides of the Gibraltar strait, here and elsewhere I use "Maghreb" in the original sense, indicating the whole Islamic West including al-Andalus.

interaction about which we are however unable to say any more. After the mid-century, Italian abbacus algebra appears to develop largely on its own premises, within its own closed space[18] (developing quite slowly, it must be said).

A *Tratato sopra l'arte della arismetricha,* written in Florence in c. 1390 (Bibl. Naz. Centr., fondo princ. II.V.152) contains an extensive algebraic section [ed. Franci & Pancanti 1988], which suggests a last case of (direct or indirect) inspiration from Arabic algebra. Firstly, in a wage problem, an unknown amount of money is posited to be a *censo;* Biagio *il vecchio* as quoted by Benedetto da Firenze [ed. Pieraccini 1983: 89*f*] had already presented the same problem before c. 1340, though positing the money to amount to a *cosa.* However, the present author does not understand that a *censo* can be a simple amount of money, and therefore feels obliged to find its square root – and then finds the solution as the square on this square root. The author hence cannot himself be familiar with the Arabic meaning of *māl*, nor can he however have taken it from Biagio. He thus uses the terminology without understanding it, and therefore cannot have invented it himself. On the other hand, this rather characteristic problem could not be shared with Biagio if the author's inspiration did not come from the same area – ultimately from the Maghreb, immediately from somewhere in the Romance Ibero-Provençal region.

Another highly plausible borrowing from Maghreb algebra in the same treatise is a scheme for the multiplication of three-term polynomials[19] which emulates the algorithm for multiplying multidigit numbers; the text itself justly refers to the multiplication *a chasella* [ed. Franci & Pancanti 1988: 11]. The "Jerba manuscript" of ibn al-Hā'im's *Šarḥ al-Urjūzah al-Yāsminīyā* does something very similar [Abdeljaouad 2002: 33].

Since these two borrowings occur in the same manuscript and nothing else from the period which I know of suggests any recent contact, interaction through a single gate seems more likely that exchanges in an open space.

I shall say little about an episode in the reception of Arabic algebra which goes back to the earlier 13th century but had negligible impact ⟦but cf. article I.12⟧. Benedetto refers in his *Trattato* [ed. Salomone 1982: 1] to a translation made by Guglielmo de Lunis (otherwise known as a translator of Aristotle); Raffaello Canacci [ed. Procissi 1954: 302] is more explicit, and speaks of a translation of "La regola dell'algibra" by Guglielmo "d'arabicho a nostra linghua". In 1521, Francesco Ghaligai copies Canacci [Karpinski 1910: 209], but with reference also to Benedetto; other features of his text confirm that he is familiar with both versions of the story [Høyrup 2008: 38]; finally, one manuscript

[18] Or even within a plurality of fairly closed spaces: schemes for calculation with polynomials, though present in some manuscripts before the mid-14th century, are not even mentioned in the Florentine school tradition culminating in Benedetto da Firenze's *Trattato de praticha d'arismetrica* from 1463 and referring back to Biagio il Vecchio, Paolo dell'abbaco and Antonio de' Mazzinghi.

[19] Earlier manuscripts only present schemes (wholly different in character) for the multiplication of binomials.

of Gerard's translation of al-Khwārizmī [Hughes 1986: 223] claims to represent Guglielmo's translation, the existence of which is thus confirmed, even though the ascription itself is obviously wrong.

It has been proposed that translation into "our language" should be understood as "into Latin", and in particular that Guglielmo's translation be identical with the version found in the manuscript Oxford, Bodleian, Ms. Lyell 52.[20] This idea can be safely discarded, since all our evidence (apart from the erroneous ascription) lists a number of Arabic terms together with Italian explanations; of these terms and explanations there are no traces in the Latin manuscript, which furthermore translates al-Khwārizmī's technical terms differently.

One of the Arabic terms is *elchal,* which according to the explanation must stand for *al-qabila.* As observed by Ulrich Rebstock (personal communication), the disappearance of the *b* indicates an Ibero-Arabic pronunciation. Apart from this very unspecific confirmation of the role of the Iberian (but probably Islamic-Iberian) environment, nothing is known about this lost translation – apart from a vague possibility that Fibonacci's occasional use of *avere* instead of *census* in the algebra section of the *Liber abbaci* could be borrowed from Guglielmo.

The *Columbia Algorism*

The *Columbia Algorism* (Columbia X 511 A13) is a 14th-century copy of a late 13th-century original (cf. note 9). It is interesting in the present context for several reasons.

Firstly, it makes (sparse) use of a notation for ascending continued fractions, known in Christian Europe primarily from Fibonacci's writings. For instance (p. 155), Fibonacci would write $\frac{9}{25}\frac{5}{12}16$ where our notation would be $16+\frac{5}{12}+\frac{9}{12\cdot 25}$ (Fibonacci's fractions may be much longer).[21] The *Columbia Algorism* does not write mixed numbers with the fraction to the left, nor does it follow the corrupted usage of the *Livero*.[22] However, it does use the notation for continued fractions, sometimes written from right to left (the Maghreb way), sometimes from left to right (an adaptation to the European writing direction).[23] Since nothing else in the treatise points toward Fibonacci (and since

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20 Without adopting the thesis, Wolfgang Kaunzner [1985: 10–14] gives an adequate survey.

21 This is the notation which the *Livero* (above, p. 768) mixes up with the normal notation for mixed concrete numbers, writing for instance [ed. Arrighi 1989: 18] "d. $\frac{17}{49}$7 de denaio", "*denari* $\frac{17}{49}$7 of *denaro*" where his source must have had "7 denari, $\frac{17}{49}$ de denaro" or "denari 7, $\frac{17}{49}$ de denaro").

Both the notation for ascending continued fractions and the habit to write the fractional part of a mixed number to the left are borrowings from the Maghreb, where the notations were created in the 12th century.

22 For instance, in #4 we find "9 e $\frac{1}{2}$" and "8$\frac{3}{4}$", and in #23 "d 11 e $\frac{22}{25}$ di d" [ed. Vogel 1977: 33, 51].

23 In #39, $\frac{1}{4}\,\frac{1}{2}$ stands for $^{5}/_{8}$, and $\frac{3}{4}\,\frac{1}{2}$ for $^{7}/_{8}$ – but in #60, $\frac{1}{4}\,\frac{1}{2}$ stands for $^{3}/_{8}$. In #39, moreover,

Fibonacci's continuous fraction line is broken here into two), we may safely assume that he is not the source.

Two other features of the treatise suggest an Iberian connection. Firstly, one of its problems is an atypical use of the dress of a purse. Usually, the purse is found by several persons, which gives rise to a complicated set of linear conditions; what we find in the *Columbia Algorism* [ed. Vogel 1977: 122] is much simpler (and analogous to what Fibonacci (p. 173) calls "tree problems", in accordance with the usual dress for this problem type): "Somebody had *denari* in the purse, and we do not know how many. He lost $^1/_3$ and $^1/_5$, and 10 *denari* remained for him". The same problem, only with the unlucky owner of the purse being "I" and the remaining *dineros* being only 5, is found in the *Libro de arismética que es dicho alguarismo,* an undated Castilian treatise known from a copy from 1393 (ed. Caunedo del Potro, in [Caunedo del Potro & Córdoba de la Llave 2000: 167]). Both treatises, moreover, solve the problem by way of a counterfactual question, "If 7 were 10 [respectively 5], what would 15 be?". This is the standard approach of the *Columbia Algorism* as well as the Castilian treatise, but not of other Italian treatises; since the *Columbia Algorism* appears not to have been widely known, the problem type is most likely to have circulated in the Ibero-Provençal area and to have been borrowed from there into the *Columbia Algorism,* even though the opposite passage cannot be excluded.

The rule of three

The second characteristic feature of the *Columbia Algorism,* on the other hand, can be quite safely attributed to Iberian (or at least Ibero-Provençal) influence: the way the rule of three is dealt with.[24]

The initial pages of the *Columbia Algorism* are missing; if the rule of three was presented here, we cannot known in which terms this was done. However, most references to the rule within problems are of the kind also encountered in the problem just quoted, through counterfactual questions, "if a were b, what would c be?" (on the exceptions, see below).

Such counterfactual questions are not absent from other Italian treatises. However, they always occur as secondary examples of the rule of three, after problems confronting two different species of coin, or coin and commodity – or they are found wholly outside

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$\frac{1}{gran}\,\frac{1}{2}$ stands for $1^1/_2$ gran [ed. Vogel 1977: 64, 81].

24 The "rule of three" is *a rule,* and to be kept apart from the sort of *problems* (problems of proportionality, "to a corresponds b, to c corresponds what?") to which it is applied. The rule can be identified through the order of operations to be performed: "first multiply b and c, then divide by a". The intermediate result bc has no concrete meaning, whereas the intermediate results of the alternatives (division first) have a concrete interpretation; either "to 1 corresponds $^b/_a$ " or "to c corresponds $^c/_a$ times as much as to a".

the presentation of the rule of three. The *Livero* [ed. Arrighi 1989: 14], for instance, introduces them separately and at a distance from the rule of three (its very first topic) as a "rule without a name". In all Ibero-Provençal treatises I have inspected,[25] however, such counterfactual questions (or related abstract number questions like "if $4^1/_2$ are worth $7^2/_3$, what are $13^3/_4$ worth?") always provide the first and basic exemplification of the rule of three.

This observation leaves little doubt about the dependence of the *Columbia Algorism* on an Iberian (or Ibero-Provençal) model, since standard Arabic treatises introduce the rule in a wholly different way. However, the rule of three has much more to say about our topic.

The earliest statement of the rule is found in the *Vedāṅgajyotiṣa* [Sarma 2002: 135], cautiously to be dated to c. 400 BCE [Pingree 1978: 536]. In Kuppanna Sastry's translation as quoted by Sarma, this version of the rule runs

> The known result is to be multiplied by the quantity for which the result is wanted, and divided by the quantity for which the known result is given.

The reference to "the result that is wanted" has some similarity to what we find in the abbacus books – for instance, in Jacopo's *Tractatus,*[26]

> If some computation should be given to us in which three things were proposed, then we should always multiply the thing that we want to know against that which is not similar, and divide in the third thing, that is, in the other that remains.

This was, with negligible variations, the standard formulation of the rule of three of all abbacus treatises from the *Livero* onward.

It is not clear from Sarma's quotation (but unlikely from the context of his discussion) whether already the *Vedāṅgajyotiṣa* refers to a "[rule of] three things", but so do at least Āryabhaṭa, Brahmagupta, Mahāvīra and Bhaskara II.[27] All of them also refer to that which is wanted (*iccha*). Āryabhaṭa's formulation (translated from Kurt Elfering's German) is

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25 That is, beyond the just-mentioned *Libro… dicho alguarismo*, in chronological order: the "Pamiers Algorism" from c. 1430 [Sesiano 1984a]; the anonymous mid-15th franco-Provençal *Traicté de la pratique* [ed. Lamassé 2007]; Barthélemy de Romans' slightly later, equally Franco-Provençal *Compendy de la praticque des nombres* [ed. Spiesser 2003]; Francesc Santcliment's Catalan *Summa de l'art d'Aritmètica* from 1482 [ed. Malet 1998]; and Francés Pellos' *Compendion de l'abaco* from 1492 [ed. Lafont & Tournerie 1967: 103–107]. The Pamiers Algorism, the *Traicté* and the *Compendy* are connected, but the others are independent of each other and of this group.

26 From [Høyrup 2007: 236*f*], with correction of an error ("in the third thing" instead of "in the other thing").

27 See [Elfering 1975: 140] (Āryabhaṭa), [Colebrooke 1817: 33, 283] (Bhāskara II, Brahmagupta), and [Raṅgācārya 1912: 86] (Mahāvīra).

> in the (rule of) three magnitudes, after one has multiplied the magnitude *phala* [“fruit”/“outcome”] with the magnitude *icchā,* the intermediate outcome is divided by the *pramāṇa* [“measure”].

Here, there is no reference to what is similar/not similar. However, this turns up as secondary information in the formulations of Brahmagupta, Mahāvīra and Bhāskara II[28] – but in ways so different that direct descent can be excluded.

The earliest reference to the rule in an extant Arabic work is in al-Khwārizmī’s algebra. Al-Khwārizmī speaks of four quantities, not three. For the rest, interpreters differ on the meaning of his words. For four quantities in proportion $\frac{a}{b}=\frac{c}{d}$, Rosen [1831: 68] takes al-Khwārizmī to claim that a is “inversely proportionate” to d, and b to d, while Rashed [2007: 196] states that a is “not proportional” to d (etc.). A slightly later passage states according to Rosen that among the three known quantities, two “must necessarily be inversely proportionate the one to the other”, according to Rashed that there are two numbers, each of which is not proportionate to its associate; in both cases, these two numbers have to be multiplied. None of this makes mathematical sense,[29] and the Latin translations of Gerard of Cremona [ed. Hughes 1986: 255] and Robert of Chester [ed. Hughes 1989: 64] are indeed different (while agreeing with each other). Both interpret the essential adjective as “opposite”;[30] as long as we restrict ourselves to the first statement, this “opposition” could refer to a graphical scheme (*our* scheme, and the scheme used in 12th-century Toledo, cf. note 31; al-Khwārizmī has nothing of the kind); the second passage, however, leaves only one possibility; that the term *mubāyn,* translated “différent” by Mohamed Souissi [1968: 96] with reference to exactly this passage, means *dissimilar* − in exact agreement with the secondary explanations of the Sanskrit mathematicians from Brahmagupta onwards.

Most Arabic treatments of the rule have as their primary examples problems confronting commodity and price, and designate the four terms accordingly. They also often present the rule after a short introduction of the proportion concept and the rule of cross multiplication. Sometimes proportions and rule of three are linked, sometimes they are not – and often a formulation including the similar/non similar is involved.

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[28] In Bhāskara I’s commentary to Āryabhaṭa [ed. Keller 2006: I, 107*f*], on the other hand, no such reference is present.

⟦This is only true inasfar as the presentation of the rule is concerned. In connection with the first example [ed., trans. Keller 2006: I, 109*f*] Bhāskara refers (apparently via an earlier commentary), to the similar and the dissimilar as something not yet known by “the [Sanskrit] wise”. See article I.5.⟧

[29] We may presume that both translators have drawn from their familiarity with Euclidean proportion theory, without asking themselves whether al-Khwārizmī would be likely to use the same resource.

[30] Boris Rozenfeld [1983: 45] also agrees, and translates *protiv.*

Al-Karajī's *Kāfī fi'l ḥisāb* introduces the rule of three separately from the preceding presentation of the proportion. His rule (translated from [Hochheim 1878: II, 16*f*]) runs as follows:

> You find the unknown magnitude by multiplying one of the known magnitudes, for instance the sum or the quantity, by that which is not similar to it, namely the measure or the price, and dividing the outcome by the magnitude which is of the same kind.

Ibn al-Bannā [ed., trans. Souissi 1969: 88] integrates the two topics and gives the rule in this shape:

> You multiply the isolated given number, (that is, the one which is) dissimilar from the two others, by the one whose counterpart one does not know, and divide by the third known number.

Ibn Thabāt [ed., trans. Rebstock 1993: 43–45] also integrates and first gives rules based on the former. Then comes this rule, almost identical with the Italian abbacus formulation:

> The fundament for all *mu'āmalāt*-computation is that you multiply a given magnitude by one which is not of the same kind, and divide the outcome by the one which is of the same kind.

Ibn Thabāt worked in Baghdad in the earlier 13th century and was a legal scholar rather than a "mathematician" or "astronomer-mathematician". That *his* words should have reached the abbacus school is not credible. We must assume that they reflect the formulation used by merchants in a wide area (apart of course from the passage "fundament for all *mu*'*āmalāt*-computation", which in the commercial milieu went by itself). If Fibonacci had been taught for more than "some days" in Bejaïa he might even have encountered it there; in any case, the Italian formulation cannot have been adopted from Fibonacci[31] nor probably from any other specific "gate", but by way of participation in a shared open space. The reference of Italian abbacus authors as well as Sanskrit mathematicians to a "rule of three" suggests that this open space encompassed not only the shores of the Mediterranean but also those of the Arabian Sea.

The origin of the Iberian recourse to counterfactual questions is more enigmatic. It could of course represent a local development; the abstract number question is not difficult to produce by simple abstraction, al-Khwārizmī's example "ten for eight, how much for four" is not very different; nor would the step from the merely abstract to the explicitly counterfactual be more difficult to make in the Iberian world than elsewhere.

[31] Fibonacci, when introducing the rule (p. 83*f*) does not speak of a "rule of three things", as done by the Sanskrit as well as Italian authors but (as common among Arabic mathematicians) of "four proportional numbers, of which three are known but the last unknown"; his rule prescribes the inscription of the numbers on a rectangular *tabula* (represented in the treatise by a rectangular frame). This method was also known to the compiler of the *Liber mahameleth,* and thus in 12th-century Toledo. It is likely to have inspired Robert's and Gerard's understanding of *mubāyn* as "opposite".

However, there is some reason to believe that at least the abstract formulation circulated in the Arabic commercial world. As it turns out, al-Khwārizmī's "ten for eight …" is found in Rosen's, Rashed's and Robert of Chester's translations – but Gerard has concrete numbers, "ten *cafficii* for six dragmas …".[32] The abstract formulation may thus very well have crept into the manuscript tradition after al-Khwārizmī's time. Moreover, ibn al-Khidr al-Qurašī, a little-known mid-11th-century author from Damascus, explains [ed., trans. Rebstock 2001: 64] that the foundation for "sale and purchase" is the seventh book of Euclid, and then goes on that "this corresponds to your formulation, 'So much, which is known, for so much, which is known; how much is the price for so much, which is also known?' ".

At this point we may return to the alternative formulation of the rule in the *Columbia Algorism,* first enunciated in general terms [ed. Vogel 1977: 39*f*, trans. JH],

> Remember, that you cannot state any computation where you do not mention three things; and it is fitting that one of these things must be mentioned by name two times; remember also that the first of the things that is mentioned two times by name must be the divisor, and the other two things must be multiplied together,

illustrated by an example and used a couple of times later [ed. Vogel 1977: 48, 50]. The "name" that is mentioned two times is obviously a reference to the "similar" things, so somehow this formulation also points back to the similar and the dissimilar. It was not adopted widely in the abbacus environment, but it must have survived as a undercurrent: Pacioli offers it as an alternative in the Perugia manuscript from 1478 [ed. Calzoni & Gavazzoni 1996: 19*f*], and in almost the same words in the *Summa de arithmetica* [1494: fol. 57^{r}]. Also in 1478, moreover, Pietro Paolo Muscharello mixes it into the standard formulation [ed. Chiarini et al 1972: 59, trans. JH]:

> This is the rule of 3, which is the fundament for all commercial computations. And in order to find the divisor, always look for the similar thing, which is mentioned twice, and one of these will be the divisor, and I say that it will be the one which is not your request, and this your request you will get by multiplying with the other not similar thing, and this multiplication [i.e., product] you will have to divide by your divisor, and from it will come that which you will require.

Muscharello's treatise was written in Nola, otherwise best known as the birthplace of Giordano Bruno. Nola is located in Campania, an area outside the core of abbacus culture (which flourished between Genova, Milan and Venice to the north and Umbria to the south) and in close contact with the Iberian world. Interestingly, Muscharello also uses

[32] This informs us about three manuscripts: the main Arabic manuscripts Oxford, Bodleian, Hunt 214, and the two lost manuscripts used by Gerard and Robert; since Rashed's critical apparatus is incomplete, it is not possible to know how the other Arabic manuscripts look. (Rashed has some references to Gerard's edition, but omits some of its variants on this point; what else he may omit is a guess.)

the counterfactual structure as his first example of the rule of three. His particular ways suggest that we should not consider Italy of his times as a single community.

Because of the possibility to identify specific markers in the formulations of the rule of three, scrutiny of a larger number of Sanskrit, Arabic and Christian-European presentations of the rule would probably yield more information about points of contact, transmission roads and communities.

Set phrases, abbacus culture, and Fibonacci

Whoever has read (in) a few abbacus books will be familiar with phrases like these:

- "make this computation for me";
- "this is its rule"
- "now say thus";
- "and it is done, and thus one makes the similar computations";
- "make similar computations thus";

They all point to a teaching concentrated on the solution of problems serving as paradigmatic examples, and they will only have made sense within an institution similar in that respect to the abbacus school. In the *Livero,* they are particularly copious in those problems that are not taken from Fibonacci, but some of them are glued onto Fibonacci problems without being present in the original.

Fibonacci himself mostly avoids these characteristic locutions; in general, he tries to emulate the style of "philosophical" mathematics; similarly, he often tries to reformulate the mathematical substance *magistraliter,* "in the way of [school] masters" – this word is found on pp. 163, 215, 364 (on p. 127 instead he refers to the scholarly way as being *secundum artem).* None the less, an occasional "make similar computations thus" *can* be found in the *Liber abbaci.*

The appearances of the set phrases in Fibonacci's works are by far too few to have inspired their ubiquitous presence in abbacus writings. We may conclude that Fibonacci was so immersed in the style that later unfolds in the abbacus books that he did not manage to eliminate it completely.

In some cases, he distinguishes his own style from what "we are used to do" or from what is done *vulgariter.* An example of the former distinction is in his exposition of the simple false position (p. 173*f*), taught by means of a tree, of which $^1/_4 + ^1/_3$ are below the ground, which is said to correspond to 21 palms. He searches for a number in which the fractions can be found (taking 12 as the obvious choice), and next argues that the tree has to be divided in 12 parts, 7 of which must amount to 21 palms, etc. But then he explains that there is another method "which we use" (*quo utimur*), namely to posit that the tree be 12. He concludes that

> therefore it is customary to say, for 12, which I posit, 7 result; what shall I posit so that 21 result?

and finds the solution by the rule of three. This corresponds exactly to what can be found in abbacus books – for instance, in the *Columbia Algorism* [ed. Vogel 1977: 79]:

> The $^1/_3$ and the $^1/_5$ of a tree is below the ground, and above 12 cubits appear. [...] If you want to know how long the whole tree is, then we should find a number in which $^1/_3$ $^1/_5$ is found, which is found in 3 times 5, that is, in 15. Calculate that the whole tree is 15 cubits long. And remove $^1/_3$ and $^1/_5$ of 15, and 7 remain, and say thus: 7 should be 12, what would 15 be?

Vulgariter, per modum vulgarem (etc.) are used (at least) four times (pp. 115, 127, 204, 364) to characterize simple stepwise calculation as opposed to a single combined operation (by means, e.g., of composite proportions); stepwise calculation would probably be what a practical reckoner preferred. On p. 63, addition of $^1/_3$ and $^1/_4$ *secundum vulgi modum* is made by taking both fractions of a convenient number (*in casu* 12), similarly a method easily understood by reckoners without theoretical training. More informative is what we find on p. 170. After having found the fourth proportional to 3–5–6 as $(5 \cdot 6) \div 3$, Fibonacci says that the same question is proposed "in our vernacular" (*ex usu nostri vulgaris*) as "if 3 were 5, what would then 6 be?". Next, he asks for the number to which 11 has the same ratio as 5 to 9, and restates the question *secundum modum vulgarem* as "if 5 were 9, what would 11 be?". This tells us that the vernacular practice in which Fibonacci participates (*vide* his repeated first person plural, "we use", "our vernacular") is of the Ibero-Provençal kind, not similar in its approach to what is later found in Italy. Actually, Santcliment [ed. Malet 1998: 163] introduces the presentation of the rule of three by saying "and this species begins in our vernacular, 'if so much is worth so much, how much is so much worth' ".[33]

Fibonacci is certainly no abbacus author, his scope as well as his ambition go much beyond that. But as we see, he knew that mathematical culture of which the Italian abbacus school became the most famous representative. His book, furthermore, informs us about how this culture looked at a moment it had not yet reached Italy – though not very specifically, it is up to us to try to sort out what comes from which place.[34]

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[33] "E comença la dita specia en nostre vulgar si tant val tant: que valra tant".

[34] That this culture had not yet reached Italy is illustrated by the yet another reference in the book to vernacular methods (the last one, if I am not mistaken), namely on p. 114. Here the Pisa method to find the profit corresponding to each *libra* invested in a commercial partnership (certainly a real-life method, since it starts by removing one fourth of the profit as some kind of tax or as payment for the shipping) is confronted with calculation *secundum vulgarem modum,* which turns out to be the usual partnership rule.

A number of apparent Italianisms in the text (*baracta; viadium/viagium; pesones* [Latinized plural of peso]; *avere;* and various names of goods) might be taken to suggest an Italian background to the *Liber abbaci.* However, apart from the possibility that Fibonacci – a Tuscan speaker – might introduce such loanwords on his own, it should be noticed that all may just as well come from the Catalan of his epoch.

Byzantium

As an example of what may perhaps be dug out by careful analysis I shall mention the question of Byzantium. As quoted, Fibonacci tells us in the introduction to the *Liber abbaci* that one of the places where he encountered study of the art of “the nine digits of the Hindus […] with their varying methods” was Greece, that is, Byzantium. On several occasions, moreover, he states that a particular problem was presented to him by a Byzantine master (pp. 188, 190, 249); finally, a number of problems tell stories taking place in Constantinople (pp. 161, 203, 274, 296). Of the former group, all examples but one state prices in *bizantii* (the one on p. 190 deals with unspecific “money”, *denarii*), and all the latter deal with the same Byzantine currency. We may infer that the metrologies occurring in the book, even in wholly artificial problems, were as a rule not chosen at random but thought of in connection to the location where they were in use. Since most problems do not specify where they are supposed to take place,[35] nor where Fibonacci was confronted with them,[36] the metrologies and currencies are likely to carry otherwise hidden information of one or the other kind – or both.

The *Liber abbaci* shares with later books in the abbacus tradition another kind of likely indirect information about the role of Byzantium. Regularly, they start alloying problems with a phrase “I have silver/gold” of this and this fineness.

Fibonacci uses the structure a few times. On p. 143 it stands in a reference to “our” (vernacular) way of expressing ourselves.[37] Then on p. 156 it stands as what “you” should say when stating a problem about alloying of silver. Finally, the locution is used to indicate that an alligation problem dealing with grain is equivalent to one about the alloying of silver (p. 163); obviously Fibonacci sees the “I have”-structure as characteristic for such problems.

In Jacopo’s *Tractatus, all* alloying problems start with “I have”; the locution is also used in one problem about exchange of money, and in one about money in two purses [Høyrup 2007: 125]. All alloying problems in the non-Fibonacci part of the *Livero* do as much. Later, we find the same opening for instance in Paolo Gherardi’s *Libro di ragioni*

35 *Bizantii,* for example, occur on these pages: 21, 83, 84, 93-96, 102, 103, 107-109, 113, 115, 119, 120, 121, 126, 131, 137, 138, 159-163, 170, 178, 180, 181, 203-207, 223-225, 228-258, 266, 273-277, 279, 281, 283, 310, 313-318, 323, 327-329, 334, 335, 347, 348, 349, 396, 401. Not all of these passages point by necessity to Byzantium, *bizantii* were also minted in Arabic and crusader countries [Travaini 2003: 245].

36 Actually, I am fairly sure that there are no specified references to locations for such confrontations other than Byzantium, in spite of the open-ended reference to “the give-and-take of disputation” of the introduction.

37 “When we say, I have bullion at some ounces, say at 2, we understand that one pound of it contains 2 ounces of silver”.

from 1328 [ed. Arrighi 1987a: 29–31, 89];[38] in a *Libro de molte ragioni d'abacho* from c. 1330 [ed. Arrighi 1973: 95–106];[39] in Giovanni de' Danti's *Tractato de l'algorismo* from c. 1370 [ed. Arrighi 1987b: 50–52]; in a *Libro di conti e mercatanzie* probably from c. 1390 [ed. Gregori & Grugnetti 1998: 72–74];[40] in Francesco Bartoli's *Memoriale* [ed. Sesiano 1984b: 134*f*], a private notebook written in Avignon before 1425 and containing excerpts from earlier abbacus works[41]; in Piero della Francesca's *Trattato d'abaco* [ed. Arrighi 1970: 56–59]; and (with the slight variation, also known by Piero della Francesca [ed. Arrighi 1970: 74], "Io mi trovo …") in Pacioli's *Summa* [1494: 184^{r}–185^{v}]. It is also found in a Castilian merchant handbook *De arismetica* (Real Academia Española, Ms. 155, ed. [Caunedo del Potro 2004: 45]), and it survives in Christoff Rudolff's *Coss* from 1525 [ed. Kaunzner & Röttel 2006: 201, 202, 215*f*].

This distribution of the opening "I have .." seems to point to an origin in a particular environment, distinct from that where abbacus problems in general were circulating (a money-dealers' environment, it would seem).

In one Byzantine treatise of abbacus-type (Ψηφηφορικὰ ζητήματα καὶ προβλήματα, "Calculation Questions and Problems") from the early 14th century [ed. Vogel 1968: 21–27], the first person singular serves not only for alloying problems but also for other problem types (mostly but far from always dealing with possession of or payment in gold coin). If this characterized Byzantine practical mathematics in broader general, it would be tempting to believe that the Italian and Iberian way to formulate alloying problems had its roots in a *Byzantine* money-dealers environment.[42]

Absence of Hebrew influence

A similar argument can be used to *rule out* another possible line of influence. In Roman Law, it was customary to represent participants in paradigmatic cases by the names Gaius and Titius[43] (and less often Maevius). The habit became so familiar in Medieval

38 Pp. 29–31, the first person only initiates problems about gold, whereas a silver problem starts "There is somebody who has …".

39 Alternating with the formula "A man has …".

40 Gold problems only. Problems about silver are neutral or start "Somebody has …".

41 Ten instances of "I have" regarding gold as well as silver, and a single of "Somebody has" (about gold).

42 Admittedly, a Byzantine treatise from the next century [ed. Hunger & Vogel 1963] shows no trace of the style. On the other hand, the older treatise is local Byzantine in its choice of coins referred to, whereas the younger one is heavily influenced by Italian treatises in this respect [Scholz 2001: 102]; it may therefore not say much about Byzantine habits in the 12th and 13th centuries.

43 A search for "Titius" in the electronic version of [Scott 1932] finds more than 1860 appearances. Often, of course, the name occurs repeatedly within the discussion of a single case – this is exactly

Italy that "some guy" is spoken of in modern Italian as *un tizio*. Similarly, the Babylonian Talmud sometimes (less pervasively) uses Jacob's oldest sons Reuben, Simeon, Levi and Judah for this purpose.[44] Medieval Hebrew practical mathematics[45] took over this usage and applied it much more systematically.[46]

However, with a single exception abbacus and related Ibero-Provençal writings I know of never adopted the stylistic scheme; the parallel of the "I have"-opening of alloying problems shows that they would certainly have done so if they had borrowed from the Hebrew tradition.

The single exception is, once again, Muscharello's *Algorismus;* in three problems dealing with the settling of accounts [ed. Chiarini et al 1972: 154–158, 193], the protagonists are, respectively, Piero+Martino, Rinaldi+Simoni and Roberto+Martino, and in one about four gamblers, these are Piero, Martino, Antonio and Francischo. Whether this exception is really a borrowing or an independent creation cannot be decided – in particular because German cossic writings begin at the same time to use capital letters for the same purpose – first in Magister Wolack's Latin university lecture about abbacus mathematics from 1467/68 [ed. Wappler 1900: 53f], later occasionally in Rudolff's *Coss* from 1525 [ed. Kaunzner & Röttel 2006: 211], and probably by others in between (in this case, inspiration from university teaching of Aristotelian logic is possible). There was thus a need for a way to identify the actors of a problem beyond the traditional "the first", "the second", etc., and Muscharello's use of names may have been a self-invented way to meet this need.

A pessimistic conclusion

In my initial disclaimer I promised that "the following offers elements that have to go into a synthetic answer, but too few and too disparate to allow the construction of this synthesis". I am afraid that in particular the negative second part of this pessimistic pledge has been respected. I am also afraid that further research may dig out more elements that

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why it is useful to have names to refer to. None the less, the omnipresence of this fictive person is impressing.

44 For instance, In *Yebamoth* [ed., trans. Slotki 1964: 28b], Reuben and Simeon have married two sisters, and Levi and Judah two strangers; in *Baba Kamma* [ed., trans. Kirzner 1964: 8b], Reuben sells all his lands to Simeon, who then sells one of the fields to Levi; none of this has anything to do with Genesis.

45 Represented by ibn Ezra's 12th-century *Sefer ha-mispar* [ed., trans. Silberberg 1895], written in Lucca or Rome in c. 1146 [Sela 2001: 96]; the problem collection accompanying Levi ben Geršom's *Sefer maaseh hoshev* [ed. Simonson 2000]; and Elia Misrachi's *Sefer ha-mispar* [ed., trans. Wertheim 1896] (early 16th-century).

46 Some of ben Geršom's examples, however, deal with anonymous "travellers", "merchants" etc., as usual in *mu'āmalāt-* and abbacus texts.

have to go into the answer, while making it even more difficult to produce a convincing synthesis – too much of the process has taken place in oral interaction and left no permanent traces. The only reason the Italian situation in itself is *somewhat* better documented is that the Italian merchant class was the effective ruling class of its cities, and eventually even nobility; for these merchant-patricians, mathematics books were objects of prestige – *sacred* objects, almost as the sword was a sacred object for other nobilities. For the ruling classes or culturally hegemonic strata of other areas of importance for our process they were not. The Italian books were therefore conserved with much higher probability than similar books elsewhere; and even in Italy, as one discovers at any attempt to trace *development* – for instance, the development of incipient symbolism − the holes predominate, and the cheese turns out to be all too scarce to satisfy our appetite.

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Chapter 29 (Article II.12) What Did the Abbacus Teachers Aim at When They (Sometimes) Ended Up Doing Mathematics? An Investigation of the Incentives and Norms of a Distinct Mathematical Practice

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Abstract

Italian 14th- and 15th-century abbacus algebra presents us with a number of deviations from what we would consider normal mathematical practice and proper mathematical behaviour: the invention of completely false algebraic rules for the solution of cubic and quartic equations, and of rules that pretend to be generally valid but in fact only hold in very special cases; and (in modern terms) an attempt to expand the multiplicative semigroup of non-negative algebraic powers into a complete group by identifying roots with negative powers. In both false-rule cases, the authors of the fallacies must have known they were cheating. Certain abbacus writers seem to have discovered, however, that something was wrong, and devised alternative approaches to the cubics and quartics; they also developed safeguards against the misconceived extension.

The paper analyses both phenomena, and correlates them with the general practice and norm system of abbacus mathematics as this can be extracted from the more elementary level of the abbacus treatises.

Contents

HUBERT L. L. BUSARD
in memoriam

Kreuger, Enron and scandalous abbacus algebra: three instances of fraud

There may still be Swedes who consider Ivar Kreuger a businessman of genius (at least when I was young there were). After his suicide in 1932 and the opening of his books the rest of the world, in so far as it remembers him and his attempt to create a world monopoly of matches, tends to agree that he was a crook blown up into heroic wide-screen format. That he succeeded as a star for so long – and that the Enron directors did so seven decades later[1] − depended on the construction of a scheme so complex that nobody was able to look through it.

The history of abbacus mathematics presents us with a similar episode, and some members of the tribe of historians of mathematics wave a patriotism that recalls that of certain Swedes − a phenomenon which illuminates particular features of the mathematical endeavour, just as Kreuger and Enron illuminate particular aspects of the market economy. But before that story can be told, the notion of "abbacus mathematics" should itself be explained.

Abbacus mathematics (Italian *abbaco*) is a distinct mathematical practice, known from Italy (primarily from the region between the Genova-Milan-Venice arc to the north and Umbria to the south) from the late 13th to the mid-16th century (but with an aftermath which makes much of its contents familiar to anybody who learned arithmetic in junior secondary school in the 1950s, as I did). Its social base was the "abbacus school", a school frequented by merchant and artisan youth (including also sons of the mercantile aristocracy) for two years around age 11−13. In smaller cities, the abbacus masters were often employees of the city, in large cities like Florence and Venice the schools were run on a completely private basis.[2]

It has been commonly assumed that the abbacus school and its mathematics descended, at most with minor secondary contributions, from Leonardo Fibonacci, his *Liber abbaci* and his *Pratica de geometria.* Thus, according to Elisabetta Ulivi [2002b: 10], the *libri*

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[1] I abstain from referring to corresponding Danish affairs, not because I do not recognize that they exist (they do, and mostly have as protagonists leading members of our major, "liberal" government party, reduced to ex-members only after they have been discovered or convicted) but because non-Danes know so little about them that they are allowed to continue their good deeds abroad after having been convicted at home. Those who are curious and read Danish may find information on specific cases at http://da.wikipedia.org/wiki/Klaus_Risk%C3%A6r_Pedersen and http://da.wikipedia.org/wiki/Peter_Brixtofte ⟦both accessed 2.6.2018⟧.

⟦This was originally written in March 2007, half a year before the financial crisis of 2008. I leave thought about Lehman Brothers etc. to the reader.⟧

[2] A convenient survey of the topic is [Ulivi 2002a].

d'abbaco "were written in the vernaculars of the various regions, often in Tuscan vernacular, taking as their models the two important works of Leonardo Pisano, the *Liber abaci* and the *Practica geometriae*" − while, as Warren Van Egmond sees it [1980: 7], all abbacus writings "can be regarded as […] direct descendants of Leonardo's book".

On close analysis of the texts involved − early Italian abbacus books, texts of a similar kind from the Ibero-Provençal area, and the *Liber abbaci* − this turns out to be a mistake, due to what at another occasion I called "the syndrome of the Great Book": the "conviction that every intellectual current has to descend from a *Great Book* that is *known to us*" [Høyrup 2003: 11]. ⟦[3]⟧ Instead, as argued in [Høyrup 2005b], the beginning of abbacus mathematics must be traced to an environment which precedes the *Liber abbaci;* which was known to Fibonacci; which (if it had not fully reached Italy in his days) he may have encountered in Provençal area; but which is likely to have spanned both sides of the maritime and the religious divide of the Mediterranean world. The beginning of abbacus *algebra,* taking place in the early 14th century, seems to be inspired by borrowings from an environment located in the Provençal-Catalan area, with a Catalan rather than a Provençal barycentre. This is argued in [Høyrup 2006; 2007{b}], on which I draw for the present outline. The precise location of the area is unimportant for what follows; it is more important that the inspiration did *not* come directly from Arabic "scientific" algebra as represented for instance by the treatises of al-Khwārizmī, Abū Kāmil and al-Karajī.

The "scandal" belongs precisely within the field of algebra. The earliest extant treatment of the subject (and plausibly the earliest treatment at all in an Italian vernacular) is found in Jacopo da Firenze's *Tractatus algorismi,* written in Montpellier in 1307.[4] In what in my transcription of the manuscript is labelled chapters 16–17 – the algebra section proper – rules are given for the following cases

(1) $\alpha t = n$	(3) $\alpha C = \beta t$	(5) $\beta t = \alpha C + n$
(2) $\alpha C = n$	(4) $\alpha C + \beta t = n$	(6) $\alpha C = \beta t + n$
(7) $\alpha K = n$	(12) $\alpha K = \beta C + \gamma t$	(17) $\alpha CC + \beta K = \gamma C$
(8) $\alpha K = \beta t$	(13) $\alpha CC = n$	(18) $\beta K = \alpha CC + \gamma C$
(9) $\alpha K = \beta C$	(14) $\alpha CC = \beta t$	(19) $\alpha CC = \beta K + \gamma C$
(10) $\alpha K + \beta C = \gamma t$	(15) $\alpha CC = \beta C$	(20) $\alpha CC + \beta C = n$
(11) $\beta C = \alpha K + \gamma t$	(16) $\alpha CC = \beta K$	

Here, t stands for *thing* (*cosa*), C for *censo,* n for *number* (*numero*), K for *cube* (*cubo*),

3 ⟦Documentation for the present paragraph can be found in article II.11⟧

4 [Høyrup 2000a] is an edition of the algebraic chapter with mathematical commentary, [Høyrup 1999] is a preliminary transcription of the complete Vatican manuscript (Vat. lat. 4826); the other two extant manuscripts of the treatise (Milan, Trivulziana MS 90; Florence, Riccardiana MS 2236), of which [Høyrup 2007a] is a semi-critical edition, represent a redaction from which the algebra chapter is eliminated. Both are also included in my [2007b].

CC for *censo di censo. Censo* is the product of *thing* with *thing, cubo* the product of *censo* with *thing,* and *censo di censo* the product of *censo* with *censo.*[5] This shorthand has the advantage over x, x^2, x_3, … that it leaves it as non-obvious for us as it was for the medieval reckoner that the cases (8–12), (14–20) are reducible.

For the first six cases, one or more illustrating examples are given, for the rest only rules. All twenty rules are valid, since all the cubic and quartic cases (7)–(20) are either homogeneous, biquadratic or reducible to one of the cases (1)–(6) through division. No mathematical scandal so far.

Yet scandal was not far away, in neither time nor space. In 1328, and still in Montpellier, a certain Paolo Gherardi wrote a *Libro di ragioni,* another abbacus book containing an algebra section.[6] Gherardi repeats most of Jacopo's rules and examples – dropping however those of the fourth degree, offering only one example for each case, changing the numerical parameters in some cases, and replacing two of the examples by entirely different ones.

The important innovations are two. Firstly, Gherardi introduces four new cases, one of which (G1) is rather trivial and the other three (G2−G4) not resolvable by means of techniques known at the time:

$$(\text{G1})\ \alpha K=\sqrt{n} \qquad\qquad (\text{G3})\ \alpha K=\beta C+n$$
$$(\text{G2})\ \alpha K=\beta t+n \qquad\qquad (\text{G4})\ \alpha K=\gamma t+\beta C+n$$

For the latter three, Gherardi gives rules modelled after those for the second degree −

5 These terms come from Arabic algebra, which is the evident ultimate root of all abbacus algebra. *Cosa* translates *šay'*, *censo* comes from Latin *census,* a translation of *māl,* "possession" or "amount of money" ⟦apparently the standard translation used by the 12th-century Toledo translators⟧. Originally, Arabic *al-jabr* was centred around riddles dealing with a possession and its (square) root, for instance "a possession and ten of its roots equal 39 dinars". Al-Khwārizmī, in his presentation of the topic (which may be the earliest presentation at all in a systematic written treatise) still remembers this: when he has found the root, he multiplies it by itself in order to find also the possession. But already in *his* treatise these riddles with their solutions serve as representation of second-degree problems, in which the fundamental unknown is a *šay'*, whose second power is identified with a *māl* (whence the *šay'* becomes its root).

Almost all abbacus algebras do as Jacopo: the "roots" are replaced by "things" in the formulation of the rules, and the number is a number, not (as in the Latin translations of al-Khwārizmī) a quantity of dragmas. This is one of several reasons that abbacus algebra (in particular Jacopo's algebra) can be seen not to descend directly from the "learned" level of Arabic algebra but from a type which has disappeared from the sources − probably from a practice which was integrated with the teaching of commercial arithmetic, just as abbacus algebra itself.

6 An edition of this chapter, with translation and mathematical commentary, is [Van Egmond 1978]. An edition of the complete treatise (without translation and mathematical commentary) is in [Arrighi 1987].

for (G2) and (G3) those which hold if K is replaced by C, for (G4) the rule for the equation

$$\mathrm{a}C=\beta t+(\gamma+n)$$

Finally, Gherardi offers illustrative examples for those higher-degree cases where Jacopo had given none − all of a kind that is easily constructed (see imminently), whereas some of those proposed by Jacopo are so intricate that a modern reader does not immediately see that they lead to second-degree equations. For instance, Jacopo's illustration of case (6) runs as follows [ed., trans. Høyrup 2007b: 318*f*]:

> Somebody has 40 *fiorini* of gold and changed them to *venetiani.* And then from those *venetiani* he grasped 60 and changed them back into *fiorini* at one *venetiano* more per *fiorino* than he changed them at first for me. And when he has changed thus, he found that the *venetiani* which remained with him when he detracted 60, and the *fiorini* he got for the 60 *venetiani,* joined together made 100. I want to know how much was worth the *fiorino* in *venetiani.*

Gherardi's examples for the third-degree cases all follow the model used in Jacopo's illustration for case (3) [ed., trans. Høyrup 2007b: 307]:

> Find me 2 numbers that are in the same proportion as is 4 of 9. And when one is multiplied against the other, it makes as much as when they are joined together. I want to know which are these numbers.

Such illustrations are of course easily constructed for any given polynomial equation and look more complex than the equation itself without really being so – for instance Gherardi's illustration [ed. Arrighi 1987: 107] of the case (G4) "cubes are equal to things and *censi* and number":

> Find me three numbers which are in proportion as 2 to 3 and as 3 to 4, and that the first multiplied by itself and then by the [same] number makes as much as when the second is multiplied by itself and the third number is added above, and then 12 are added above.

As we shall see, Gherardi cannot have invented all of this, he must have copied it from an earlier source. We may ask why he did not discover that he was filling his treatise with nonsense. The answer is that all the wrong solutions contain irreducible radicals, and that Gherardi made no attempt to find the approximate value of the solutions. This was no idiosyncrasy.⟦[7]⟧ Even Jacopo, when finding a correct irrational solution to one of his examples, leaves things there. Being satisfied with exactly expressed but

7 ⟦Another way to discover the nonsense would have been the observation that (6) and (G2) can only be solved by the same rule if C equals K, that is, if t = 1 (0 being no number at the time) – and that can only be the case if $\alpha=\beta+n$. But that argument, though it should have been possible in the 14th century, seems never to have been advanced − at least I do not remember having seen it in writing.⟧

irrational solutions remained the habit of abbacus algebra. In contrast, abbacus *geometry* always approximated the square roots that turned up in applications of the "Pythagorean rule" (as it must be called in a context where it was always presented as a rule without proof).[8] This difference already tells us that algebra and geometry served different purposes: geometry (as a whole, not necessarily each single problem) had to lead to results that were applicable in practice, and which could thus be compared to the reading of a yard stick. Abbacus algebra, at least beyond the first degree, must in some sense (which we shall get closer at below) have been a purely theoretical discipline without intended practical application.

Almost honest business

If Gherardi does not represent the beginning of false solutions, nor is he their end. In 1344, Master Dardi da Pisa (as unidentified as Jacopo and Gherardi) wrote a treatise *Aliabraa argibra,* the earliest extant European-vernacular treatise dedicated to algebra alone.[9] After presenting the arithmetic of roots and binomials and giving geometric demonstrations for the correctness of the rules (the latter are very rare in abbacus algebra, and in particular not present in any of the earlier treatises) Dardi deals with 194 "regular" cases and 4 whose rules are told only to hold under special conditions (conditions which are not analyzed).[10] The huge number of regular cases (all with the exception of two lapses solved correctly) is reached because of ample use of radicals − for instance in these ways:

$$\alpha t+\beta\sqrt{k}=\gamma\,\mathrm{C} \qquad\qquad \alpha t+\beta\sqrt{C}=\gamma\,C$$

$$\alpha\,\mathrm{CC}=n+\sqrt[3]{m} \qquad\qquad \alpha\,\mathrm{CC}+n+\sqrt[3]{m}=\beta\,C$$

For a generation which has come to see no difference between rational and irrational numbers, which sees all the "cossic numbers" (as *thing*, *censo* etc. were to be called when abbacus algebra reached Germany under the name of *Coss*) as powers of the same

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[8] This role of square roots and their approximation was so important for geometry that the topic was mostly taught in the geometry chapter of abbacus treatises (when these were ordered in separate chapters and geometry was actually covered). In the Latin algorisms, in contrast, root extraction (not approximation) was one of the arithmetical "species"; they contain no geometry.

[9] The extant complete manuscripts are younger. One is from c. 1395 (Vatican, Chigi M.VIII 170), one from 1429 (Arizona State University Library, Tempe), and one from c. 1470 (Siena, Biblioteca Comunale, I.VII.17). Apart from lost sheets and some reordering of the material in the last manuscript, there are no major differences between the three. Of a fourth manuscript from c. 1495 (Florence, Biblioteca Mediceo-Laurenziana, Ash. 1199) I have only seen the extract in [Libri 1838: III, 349−356], but to judge from this it appears to be very close to the Siena manuscript.

[10] [Van Egmond 1983] lists all the cases in symbolic transcription.

unknown, and which expresses everything in symbols and not in words, these are trivial extensions – and by reducing many of the cases to other cases that are dealt with previously, Dardi shows that he understood things in the same way without having access to our tools (tools without which the extensions are often *not* trivial).

All of these cases are illustrated by one or more examples. All are pure-number problems, with a few exceptions either about a single number, about two numbers with sum 10, or about numbers in given proportion.

Then there are the four "irregular" cases, cases governed by non-general rules. It is clear from Dardi's words that he knows these rules to be valid only when the equations to which they correspond have particular properties – but he states that "by some accident the said rules may appear in some computation". The cases in question are these:

(I1) $\gamma t+\beta C+\alpha K=n$	(I3) $\alpha t+\gamma C+\alpha CC=n+\beta K$
(I2) $\delta t+\gamma C+\beta K+\alpha CC=n$	(I4) $\delta t+\alpha CC=n+\gamma C+\beta K$

All four are provided with examples, the former two of which reveal how the rules have been found. We may look at the first example − a capital grows in three years with composite interest from 100 £[11] to 150 £ (Jacopo has the same problem, only with two years; it illustrates his case (4)). If the value of the capital after 1 year – or, even simpler, the value of 1 £ after one year – had been taken as the *thing*, we would have been led to a homogeneous equation,

$$t^3 = 1500000 \qquad \text{respectively} \qquad t^3 = 1^1/_2.$$

Instead, Dardi takes the monthly interest of 1 ß expressed in δ as his *thing*. The yearly interest of 1 £ is therefore $^1/_{20}$ *thing* £. The same choice had been made by Jacopo, and in Dardi's present case it leads to the equation

$$100+15t+{}^3/_4\,C+{}^1/_{80}\,K = 150.$$

The rule used to solve it is

$$t=\sqrt[3]{\left(\frac{\gamma/\alpha}{\beta/\alpha}\right)^3+\frac{n}{\alpha}}-\frac{\gamma/\alpha}{\beta/\alpha},$$

– or rather, since the rule first tells to divide by [the coefficient of] the cubes and afterwards speaks only of the resulting new coefficients,

[11] £ stands for *lira/lire*. 1 £ = 20 ß (*soldi*), 1 ß = 12 δ (*denari*). Whoever is familiar with the traditional British pound-shilling-penny system will recognize it.

$$t = \sqrt[3]{\left(\frac{\gamma'}{\beta'}\right)^3 + n'} - \frac{\gamma'}{\beta'},$$

where $\beta' = \beta/\alpha$, etc. At first view, this may seem an astonishingly good guess (since it works), but it requires nothing beyond some training in the arithmetic of polynomials and awareness that a different position for the *thing* leads to a homogeneous equation:

For simplicity, let us consider the homogeneous equation

$$(t + \phi)^3 = \mu$$

(in the actual problem, $\phi = 20$, $\mu = 12000$). Performing the multiplication we get

$$\phi^3 + 3\phi^2 t + 3\phi\, C + K = \mu \quad \text{or} \quad 3\phi^2 t + 3\phi\, C + K = \mu - \phi^3,$$

which should correspond to

$$\gamma' t + \beta' C + K = n'.$$

Therefore, $\phi = \gamma'/\beta', n' = \mu - \phi^3$, whence $\mu = \phi^3 + n' = (\gamma'/\beta')^3 + n'$. Now, the solution obtained from the homogeneous equation is

$$t = \sqrt[3]{\mu} - \phi,$$

that is

$$t = \sqrt[3]{\left(\frac{\gamma'}{\beta'}\right)^3 + n'} - \frac{\gamma'}{\beta'},$$

exactly Dardi's rule. Whoever invented the rule must have done so from a numerical example, but following the numerical steps precisely and seeing from which operations the coefficients arise it would not be too difficult to see that the 20 of our example results, in the words of the rule, "when the quantity [coefficient] of the *things* is divided by the quantity of the *censi*"; similarly for the rest of the rule – and similarly for the remaining three irregular rules.

The inventor of Gherardi's rules may have been a pure bluffer – for imitating the rules for the second degree it was not even necessary to know how these were derived, all that was needed was to know the rules themselves. In contrast, the rules for Dardi's irregular cases, guesses though they are in a certain sense, can only have been guessed by someone who understood polynomial operations quite well.

The irregular rules turn up in many later manuscripts, mostly without the warning about their restricted validity ⟦see article I.12⟧, One of these, an anonymous *Libro di conti e mercatanzie* [ed. Gregori & Grugnetti 1998] from c. 1395, is related to Gherardi's *Libro di ragioni* in a way which shows them to build on common sources (also shared

with an equally anonymous *Trattato dell'Alcibra amuchabile* from c. 1365 [ed. Simi 1994]).[12] Quite apart from the internal evidence (the use of a business dress when all other examples are in pure numbers, and the reservations expressed by Dardi himself), this is strong evidence that these rules were borrowed by Dardi and thus that they antedate 1344, just as the false rules in Gherardi's *Libro di ragioni* must have been borrowed by Gherardi from a source shared with the *Trattato dell'Alcibra amuchabile*. We may conclude that the presence of regular higher-degree cases in Jacopo's algebra created a fashion or a need to do even better – a need which was then fulfilled, first by the invention of false rules that could not be checked,[13] and then by the construction of irregular rules that worked if tested on the proposed example.[14] We shall discuss this process below, but for the moment only observe that false solutions survived for long. Luca Pacioli, after having made the check proposed in note 13, pointed out in his *Summa de Arithmetica* [1494: 150^{r}] that so far no rule had been found for the solution of cases where, as he says, the three algebraic powers that are present are not "equidistant". On that background, del Ferro's genuine solution of the cubic equation and Cardano's publication of a corresponding proof can be seen not only to be mathematically impressing but to deliver what others were known by then to have promised in vain for two centuries. But Pacioli's book did not kill off the fraud completely – in 1555, the Portuguese Bento Fernandes still included them in his *Tratado da arte de arismetica* [Silva 2006: 16, 30–33].

Aiming high – and failing honestly

If we are to learn from the abbacus masters about what mathematics *is* or *may be* as a practice it does not serve to consider solely such aspects of their activity as correspond to what we routinely expect from a mathematician. The false solutions constitute one aspect of abbacus mathematics which is anomalous with respect to our routine expectations. We may go on with another anomaly.

It is found in yet another Vatican manuscript, Vat. Lat. 10488 (fol. 29^{v}–30^{v}):[15]

12 For this, see [Høyrup 2006: 18–25].

13 That is, unless one constructed alternative examples with (most conveniently *from*) a known integer solution; and that seems not to have been a widespread idea.

14 This test was easy: in the example that was analyzed above, $t = \sqrt[3]{12000} - 20$. Since this is the yearly interest, the yearly growth factor of the capital is $1 + {}^{1}/_{20}t = \sqrt[3]{3/2}$. After three years the capital is thus multiplied by ${}^{3}/_{2}$, just as required. An abbacus master would have had to perform the calculation stepwise, but the principle is the same.

15 I use the most recent of the two discordant foliations.

Algebra

¶ These are some computations collected from a book made by the hand of Giovanni di Davizzo dell'abbaco from Florence written the 15th of September 1339, and this is 1424.

¶[§1] Know that to multiply number by cube makes cube
and number by censo makes censo
and number by thing makes thing

¶[§2] And plus times plus makes plus
and less times less makes plus
and plus times less makes less
and less times plus makes less.

¶[§3] And know that a thing times a thing makes 1 censo
and censo times censo makes censo of censo
and thing times censo makes cube
and cube times cube makes cube of cube
and censo times cube makes censo of cube

¶[§4] And know that dividing number by thing gives number
and dividing number by censo gives root
and dividing thing by censo gives number
and dividing number by cube gives cube root
and dividing thing by cube gives root
and dividing censo by cube gives number
and dividing number by censo of censo gives root of root
and dividing thing by censo of censo gives cube root
and dividing censo by censo of censo gives root
and dividing cube by censo of censo gives number
and dividing number by cube of cube gives cube root of cube root
and dividing thing by cube of cube gives root of cube root
and dividing censo by cube of cube gives root of root
and dividing cube by cube of cube gives cube root
and dividing censo of censo by cube of cube gives root
and dividing censo of cube by cube gives ~~number~~ censo[16]
and dividing number by censo of censo of censo of censo gives root of root of root of root
and dividing number by cube of cube of cube of cube gives cube root of cube root of cube root of cube root.

¶[§5] If you want to multiply root by root, multiply root of 9 times root of 9, say, 9 times 9 makes 81, and it will make the root of 81, and it is done.

---

16 From later versions it can be seen that this line was originally
"and dividing censo of cube by cube *of cube* gives number"
Somewhere in the process, this had become
"and dividing censo of cube by cube gives number"
Noticing the error, somebody – almost certainly the writer of the manuscript, since the correction is made there – discovered that this was wrong, and stated a correct result (but of a division Giovanni had not intended).

To divide root of 40 by root of 8, divide 40 by 8, it gives 5, and root of 5 let it be.
To divide root of 25 by root of 9, divide 25 by 9, it gives root of $2^7/_9$, done.
If you want to multiply 7 less root of 6 by itself, do 7 times 7, it makes 49, join 6 with ⟨49, it makes⟩ 55, and 7 times 6 makes 42, then multiply 7 times 42, it makes 294, and multiply then 4 times 294, it makes 1176, I say that 55 less root of 1176 will it make when 7 less root of 6 is multiplied by itself.

¶[§6] If you want to detract root of 8 from root of 18, do 8 times 18, it makes 144, its root is 12, and say, 8 and 18 makes 26, detract 24 from 26, and root of 2 will remain, done.
It you want to join root of 8 with root of 18, do 8 times 18, it makes 144, its root is 12, and say, 12 and 12 makes 24, and say, 8 and 18 makes 26, and join 24 and 26, it makes 50, and root of 50 will the number be.
If you want to multiply 5 and root of 4 times 5 less root of 4, do thus and say, 5 times 5 makes 25, and say, 5 times root of 4, do thus, bring 5 to root, it makes 25, and do root of 25 times root of 4, it makes root of 100, and make 5 times less root of 4, it makes less root of 100, 25 still remains, now detract 4 from 25, 21 remains, and 21 they make.
If you want to multiply 7 and root of 9 times 7 and root of 9, do 7 times 7, it makes 49, put (above) this 9, you have 58, and 9 times 49 makes 441, multiply by 4, it makes 1764, you have that it will make 58 and root of 1764, which is 42, done.
If you want to divide 35 by root of 4 and by root of 9, do thus, from 4 to 9 there is 5, multiply 5 times 5, it makes 25, and say, bring 35 to root, it makes 1225, now say, 4 times 1225 makes 4900, divide by 25, it makes 196, and do 9 times 1225, it makes 11025, divide by 25, it gives 441. We have that dividing 35 by root of 4 and by root of 9 gives root of 441 less root of 196, and it is done.

This is followed by 19 rules for solving reduced equations of the first, second, third and fourth degree: Jacopo's 20 cases, with two omissions, and a new false case which cannot be read because somebody discovered that it did not work and glued a paper slip over it; this slip has been removed or fallen off, but the glue has made the paper as dark as the ink.

First of all we should know that Giovanni's composition of the "cossic numbers" is multiplicative and not made by nesting: *cube of cube* stands for $t^3 \cdot t^3$, not for $(t^3)^3$. This corresponds to what we find with Diophantos and in Arabic algebra.[17] Once we know this we see that §1 and §3 present what we might call the multiplicative semi-group of

[17] In the present case κυβόκυβος respectively *ka'b ka'b*. None of these involve the genitive – in the case of the Arabic already because a possibly spoken genitive ending was not written, but a genitive would also ask for the article, *ka'b al-ka'b*. None of them therefore suggests nesting, as does the genitive used in the Italian and Latin translations. In the short run this caused a problem to the abbacus writers; for instance, it probably lays behind Dardi's two lapses, cf. [Van Egmond 1983: 417]. ⟦A more precise explanation is given in article II.14.⟧ In the longer run, however, the linguistic trouble was probably what drove the trend toward an interpretation through nesting (common in the later 15th century, and practised for instance by Pacioli). Since the creation of new names for the fifth and seventh power (etc.) then caused new confusion, this may have been one of the driving forces behind the eventual introduction of numerical exponents (first in Chuquet and Bombelli).

non-negative algebraic powers through examples; the interrupting §2 gives the "sign rules". So far, everything goes well; from the correction made in the Vatican manuscript at a later point (see note 16) it is also clear that the author of this manuscript understood it well, and was able to perform divisions within the semi-group to the extent they can be performed.

But Giovanni does not stop here. Skipping the divisions that correspond to multiplications within the semi-group (which he may have considered unproblematic) he jumps to those that have no such solution. Obviously what he does is wrong, and he should have discovered that if he had been a bit careful. Indeed, if "dividing number by thing gives number", then, since the quotient multiplied by the divisor gives the dividend (any abbacus algebraist would know that, it is often told explicitly in the texts), number multiplied by thing should give number. But "number by thing makes thing", Giovanni knows it well and states it in §2.

However, the nonsense conceals a system. If *in this paragraph and nowhere else* we read "root" as t^{-2}, "cube root" as t^{-3}, if we compose these "roots" multiplicatively, and if we finally interpret "number" *when occurring as a result* as t^{-1} – then everything is perfect, and the semi-group is extended into a group.

We shall return to the implications of Giovanni's undertaking as a whole. At this point we may try to trace how he thought. The background appears to be an intuitive and only implicit arithmetization of the series of algebraic powers. Multiplying by *censo,* so more or less he may have reasoned, we take two steps "upwards"; multiplying by cube we take three steps. Multiplying cube by *censo* we get *censo* of cube (this is stated). Dividing *censo* of cube by *censo* we therefore get cube, two steps "downwards". Dividing instead by cube we have to take three step downwards. Now multiplying the thing by itself we get a *censo,* and taking the root of the *censo* we return to the thing; similarly, the cube root of a cube is a thing. Therefore "root" must be some kind of opposite of the *censo,* and cube root some kind of opposite of cube. Taking two steps upwards from number (number by *censo*) gives us *censo,* taking two steps downwards (number divided by *censo*) therefore its opposite, root; taking three steps downward must give us cube root.

This explains everything except those rules where the result is "number" − for instance "dividing *censo* by cube". Here the idea must more or less have been that "root" is a "second root", just as *censo* is a second (being thing times thing, and corresponding to two steps in multiplication and division); correspondingly, the cube root is a "third root". Therefore, the result of *censo* divided by cube must be a "first root", which Giovanni then identifies with number (perhaps because it seemed to him that "thing" was an impossible choice, being the result of the division of cube by *censo*). If we take care that this is only the meaning of "number" when it results from a division, everything becomes correct − but like William Hogarth's famous false-perspective engraving only locally correct, and absurd as soon as one tries to move back and forth through the whole network of possible operations.

William Hogarth, “False Perspective”, from John Trusler (ed.), *The Works of William Hogarth*, vol. II. London & New York: London Printing and Publishing Company (c. 1860).

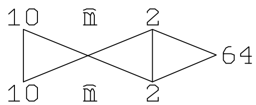
Even Giovanni’s fallacies were borrowed faithfully. As we have seen, his text was copied in 1424 by somebody who understood it well enough to repair a copying error meaningfully. Later Giovanni’s system turns up in Piero della Francesca’s *Trattato d’abaco* (earlier than c. 1480) [ed. Arrighi 1970: 84*f*] with some change of the order and without the mistake discussed in note 16, and almost identically in Giovanni Guiducci’s *Libro d’arismetricha* from c. 1465 − see [Giusti 1993: 205]. Finally, Giovanni’s first 15 rules turn up in exactly the same order in Bento Fernandes’ *Tratado da arte de arismetica* from 1555 [Silva 2006: 14] (which thus stops just before the corrupted line, which may be no accident). Piero, like Fernandes, also repeats the false algebraic rules, apparently without suspecting that something is rotten. Evidently Piero, claiming to write about “certain abbacus things that are necessary for merchants” [ed. Arrighi 1970: 39], could do so because neither he nor any merchant had the least operatory need for it.

A better intuition

Intuitions like those which can be read from Giovanni’s system can be found in other abbacus writings, and they often work better. One which is also made much more explicit gives a proof for the sign rule “less times less makes plus”. The earliest known occurrence is in Dardi’s *Aliabraa argibra*:[18]

> Now I want to demonstrate by number how less times less makes plus, so that every times you have in a construction to multiply less times less you see with certainty that it makes plus, of which I shall give you an obvious example. 8 times 8 makes 64, and this 8 is 2 less than 10, and to multiply by the other 8, which is still 2 less than 10, it should similarly make 64. This is the proof. Multiply 10 by 10, it makes 100, and 10 times 2 less makes 20 less, and the other 10 times 2 less makes 40 less, which 40 less detract from 100, and there remains 60. Now it is left for the completion of the multiplication to multiply 2 less times 2 less, it amounts to 4 plus, which 4 plus join above 60, it amounts to 64. And if 2 less times two less had been 4 less, this 4 less should have been detracted from 60, and 56 would remain, and thus it would appear that 10 less 2 times 10 less two had been 56, which is not true. And so also if 2 less times 2 less had been nothing, then the multiplication of 10 less 2 times 10 less 2 would come to be 60, which is still false. Hence less times less by necessity comes to be plus.

The passage is followed by a diagram:



The reason this must be characterized at least to some extent as an intuition and not as a genuine piece of analysis is the final part: instead of finding that the contribution of

[18] I translate from the Vatican manuscript, Chigi M.VIII.170, fol. 5v.

less 2 by less 2 *must* be the lacking 64−60 = 4, Dardi expects (from similarity) that it must be either an additive or a subtractive contribution of 4, or possibly nothing at all, and then eliminates the second and the last possibility, leaving only the first one.⟦[19]⟧

Luca Pacioli repeats the argument in his *Summa* [1494: 113r], now with the diagram in the margin, and with an explicit reference to the cross-multiplication. He finds the very concept to be *absurda* and an abuse but none the less necessary − Pacioli, indeed, thinks in terms of *negative numbers,* not merely subtractive contributions to an equation as does Dardi (he explains that a number of this kind is "less than zero and in consequence a debt"). Apart from that the only innovation is that even the possibility $(-2)\cdot(-2) = -2$ is now eliminated.

An alternative to the false solutions

Some abbacus authors thus had better intuitions than others. Similarly, some of them understood better than others that the false solutions to the higher-degree equations *were* false and even devised alternatives.

One such alternative is described in yet another anonymous manuscript, this one from the outgoing 14th century (Florence, Biblioteca Nazionale, Fond. princ. II.V.152, *Tratato sopra l'arte della arismetricha*). After the presentation of the "22 rules of algebra" (Jacopo's 20 rules, and the two biquadratics that are absent from his list), the author goes on to explain [ed. Franci & Pancanti 1988: 98] that other rules can be made for certain other cases. He continues:[20]

> Wanting to treat of this it is needed first to show how there are other roots than those one normally speaks about, that is, there are other roots than square roots and cube roots, and among these there is one which is called cube root with addition of some number, and about this I intend to show something.

The concept is then explained through several examples, starting with "the cube root of 44 with addition of 5". This root is 4, because $4^3 = 44+5\cdot4$; in general, expressed in our shorthand, the cube root of n with addition α – say, $\sqrt{}^{c}(\alpha,n)$ – is t if

$$K = n+\alpha t.$$

(We recognize the normalized version of equation (G2)). Evidently, this allows us to give a name to the solution of the above equation; but if we follow Blaise Pascal's advice about how one should understand definitions, this name is just an abbreviation of "the solution to the equation $K = n+\alpha t$", which makes the whole thing rather circular.

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19 ⟦It is worth noticing that Dardi makes use of a double indirect proof as a matter of course. The indirect proof is often supposed to be difficult to grasp and something mathematicians had to learn from philosophers. As we can see, this is not the case.⟧

20 See also [Franci 1985].

However, several further observations must be added to this. Firstly, as long as irrational square and cube roots were not approximated in abbacus algebra, expressing the solution to the equation $C = 3$ as "root of 3" was just as circular. Secondly, the trick is also used in much more recent mathematics – elliptic functions could be said to suffer from the same defect. What makes square roots and elliptic functions mathematically interesting (beyond the possibility of numerical approximation) is the network of relations they allow us to establish.

What can we say about our author and his "cube root with addition" in this respect? Firstly, that he[21] must have been aware of the objection just discussed. He does not find it worthwhile to discuss a single problem of the type which is immediately solved by his particular root; instead he explains that it is of limited use, since for many numbers this root cannot be expressed. What he does beyond that is to establish a (limited) network of relations: he gives (correct) rules for reducing equations of the types $K+\beta C = m$, $K = \beta C+m$ and $\beta C = K+m$ to the form $K = n+\alpha t$, and in the ensuing example he then makes use of the cube root with addition.[22] He also shows in the examples that solutions may exist even if the number term turns out to be "a debt", that is, negative. In order to find this reduction rule, the author must have performed manipulations similar to those behind Dardi's first irregular rule.[23] The author must have been an adroit mathematician.[24]

21 Or the one from whom he borrows − a reservation which must always be made for the abbacus authors when they seem to be original; I shall not repeat it but ask the reader to keep it in mind.

22 He does *not* show that α can be eliminated and thus that a single table of $\sqrt{}^{c}(1, n)$ is all that is needed. The reason could be that tables did not enter his mind, but it could also be that the transformation was too difficult. It asks indeed for a substitution $z = t/\sqrt{\alpha}$, which gives the equation

$$z^3 = \frac{n}{\alpha\sqrt{\alpha}} + z.$$

This is more difficult to find and explain without symbolic algebra than the additive substitutions needed for the transformations which *are* explained: finding the transformation factor to be $\sqrt{\alpha}$ asks for manipulation of several powers of two variables at a time, something which was so far beyond the horizon of abbacus algebra that even Bombelli when creating his new formalism happened to exclude it (cf. below, p. 812). *Vive* Descartes!

23 This is not fully explicit, but obvious from the detailed appearance of the rule. If, for convenience, we reformulate the first equation as

$$t^3 + 3at^2 = m$$

completion gives

$$(t+a)^3 = m + a^3 + 3a^2t = m + a^3 + 3a^2(t+a) - 3a^2 \cdot a,$$

which is exactly what the rule tells, in this order and without contraction of any kind of the expression $m+a^3+3a^2(t+a)-3a^2\cdot$ a. Similarly for the other two cases.

24 He was also more honest than many colleagues. He not only avoids the false rules, when dealing with the problem type to which Dardi applies his second irregular rule the present author [ed. Franci & Pancanti 1988: 76] takes the *thing* to be the value of the capital after one year, thus showing that the problem is fundamentally homogeneous. Further, when presenting [ed. Franci & Pancanti 1988: 3−6]

Luca Pacioli may have heard about the solution of particular higher cases by means of these specious roots, but in that case he does not seem to have appreciated them. In any case he goes on, after the statement that cases where the three algebraic powers that are present are not "equidistant" had not been solved so far, to admit that certain particular cases can be solved *a tastoni,* "feeling one's way". There is another trace in Pacioli's text of these solutions by special roots, which however he may not have recognized as such. Our anonymous author, as we remember, refers to "other roots than those one normally speaks about" in the plural, but only mentions one. In particular he does not speak about the *radice pronica* which is referred to by several other authors. Pacioli [1494: 115$^{\mathrm{v}}$] explains that by "pronic root"

> one normally understands a number multiplied by itself and above its square add the root of the said number; of this sum that number is called the pronic root. As 9 multiplied by itself makes 81, and above 81 add the root of 9, which is 3, makes 84, the pronic root is said by practitioners to be 9.

This does not seem very useful, and does not seem even loosely related to the notion of "pronic numbers", numbers of the type $n{\cdot}(n{+}1)$. However, in Pierpaolo Muscharello's *Algorismus* from 1478 [ed. Chiarini et al 1972: 163] we read that

> Pronic root is as if you say, 9 times 9 makes 81. And now take the root of 9, which is 3, and this 3 is added above 81: it makes 84, so that the pronic root of 84 is said to be 3.

This makes better sense − according to Muscharello, n is the pronic root of $n^4{+}n = n^3(n{+}1)$. Moreover, as we see, this pronic root can be used to "solve" equations of the type $CC{+}\alpha t = n$. It therefore seems plausible that the cube root with addition was not the only non-fraudulent attack on higher-degree equations made by abbacus authors before Pacioli's time.[25]

the arithmetic of the algebraic powers he accompanies the rules by numerical examples that show how things really work. If Giovanni di Davizzo had done that, his marvellous construction would have collapsed immediately.

25 Benedetto da Firenze [ed. Pieraccini 1983: 26] also mentions the pronic root in his discussion of Biagio il Vecchio's solution of the problem CC+t = 18, which he points out to be valid only for this particular parameter. It is not clear, however, whether the pronic root to which he refers is 4 (as Pacioli would have it), 2 (in agreement with Muscharello), or perhaps Biagio's solution $\sqrt{18+({}^1/_2)^4}-({}^1/_2)^2$ which is 4 (not 2, as claimed by Pieraccini in her preface [1983: vi]; she overlooks that what Biagio asks for and expresses in that solution is a number which is posited as C). The coincidence of Biagio's formula with Pacioli's interpretation depends on the specific parameter 18, it should be noted.

Some general characteristics of abbacus mathematics

Before we discuss the implications of the material presented so far − which after all represents only a small although prestigious corner of abbacus mathematics,[26] far too difficult to be taught to the young students of the standard two-year course.

Some of the orderly abbacus treatises start by presenting the Hindu-Arabic numerals and their use (in multiplication tables, in the algorithms for numerical computation, and/or in particular divisions); others start directly by the rule of three.[27] In both cases they show how things *are* or *are to be done*, without giving arguments for this. In particular in the case of the rule of three, this is noteworthy. We may look at the way the presentation is done in Jacopo's *Tractatus algorismi* [ed. Høyrup 2007b: 236*f*, minor misreading corrected]:

> If some computation should be given to us in which three things were proposed, then we should always multiply the thing that we want to know against that which is not similar, and divide in the third thing, that is, in the other that remains.

Then follows the first example (*tornesi* and *parigini* are coins minted in Tours and Paris, respectively):

> VII *tornesi* are worth VIII *parigini.* Say me, how much will 20 *tornesi* be worth. Do thus, the thing that you want to know is that which 20 *tornesi* will be worth. And the not similar (thing) is that which VII *tornesi* are worth, that is, they are worth 9 *parigini.* And therefore we should multiply 9 *parigini* times 20, they make 180 *parigini,* and divide in 7, which is the third thing. Divide 180, from which results 25 and $^5/_7$. And 25 *parigini* and $^5/_7$ will 20 *tornesi* be worth.

We notice that the intermediate product has no concrete interpretation (apart from the awkward and intuitively unattractive "as many times *p parigini* as there are *tornesi* in 20 *tornesi*"). If instead the division had been performed first, it would have been easy to explain 9/7 to be the value of 1 *tornese* in *parigini,* for which reason 20 *tornesi* must be worth 20·(9/7) *parigini.* Alternatively, one might have explained that 20 *tornesi* must

26 An expression of the prestige of algebra is found in Pacioli's words when he comes to the presentation of the rules for the algebraic cases [1494: 144^r],

> "having come with the help of God to the much desired place, that is, to the mother of all the cases called popularly "the rule of the thing", or the Great art, that is, a theoretical practice also called Algebra and almucabala in the Arabic tongue …".

The words "theoretical practice" (*pratica speculativa*) confirm what was derived from internal evidence on p. 795, *viz* that algebra was "a purely theoretical discipline without intended practical application". We shall still need to ascribe a more precise meaning to this

27 Still other treatises are less ordered problem collections. Finally, some of the "abbacus books" are not treatises at all in even the vaguest sense but private notebooks.

be worth 20/7 times as much as 7 *tornesi,* and hence $(20/7)\cdot 9$. These methods are not totally absent from the abbacus record, but they are uncommon.[28]

In a somewhat similar vein, the Pythagorean rule is always presented as a naked rule, and the perimeter of the circle is simply stated to be $3^1/_7$ times the diameter.

This does not mean that the abbacus treatises contain nothing but isolated rules. Firstly, dressed problems have to be analyzed in such a way that they can be reduced to the application of a standard rule, which means that abbacus mathematics is argued though not as thoroughly so as philosophers or modern mathematicians might prefer; secondly, the rules may also serve in more theoretical contexts. For instance, when Dardi wants to show how to divide 8 by $3+\sqrt{4}$,[29] he first makes the calculation $(3+\sqrt{4})\cdot(3-\sqrt{4})=5$ and concludes that $\underline{5}$ divided by $3+\sqrt{4}$ gives $\underline{3-\sqrt{4}}$. <u>What</u>, he next asks, will result if $\underline{8}$ is divided similarly, finding the answer by means of the rule of three ($5, 3-\sqrt{4}$ and 8 being the three numbers involved).[30]

Even though abbacus mathematics does not in any way attempt to construct an axiomatic structure, these rules offered without proof but serving to justify other procedures hence function as axioms or postulates.[31] But this is not the sole expression of the norm that mathematics should be not only argued but also consistent.[32] Firstly, when different ways to solve a problem are presented, the identity of the outcomes may be

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28 In contrast, the latter method is so common in Arabic treatises belonging to the same genre that it has a specific name, namely "by *nisba*" ("relation", specifically "ratio"); thus, for instance, al-Karajī in the *Kāfī* [ed., trans. Hochheim 1878: II, 17], who states that he prefers it over the (rule-of-three) solution by "multiplication and division".

29 Vatican manuscript, Chigi M.VIII.170, fol. 12v.

30 Similarly, the *Istratto di ragioni* [ed. Arrighi 1964: 26] from c. 1440, plausibly consisting of extracts from Paolo dell'Abbaco who wrote a century before, teaches how to divide $^4/_5$ by $^1/_3$ by means of the rule of three.

31 A rather explicit and very simple instance of this function is found in Jacopo's *Tractatus algorismi* 15.2 [ed. Høyrup 2007b: 285] when the circle is treated:

> Always do, that when you know its circumference around, that is, its measure, and you want to know how much is its straight in middle, then divide its circumference by 3 and $^1/_7$. And that which results from it, so much will its diameter be, that is, the straight in middle. And similarly when you know the straight in middle of a circumference and you want to know in how much it goes around, then multiply the straight in middle by 3 and $^1/_7$, and as much as it makes, in so much does the said round go around. And if you should want to know for which cause you divide and multiply by 3 and $^1/_7$, then I say to you that the reason is that every round of whatever measure it might be is around 3 times and $^1/_7$ as much as is its diameter, that is, the straight in middle. And for this cause you have to multiply and divide as I have said to you above.

32 There are indeed good reasons to maintain that being reasoned is an "institutional imperative" [Merton 1942] for any institutionalized and cognitively autonomous teaching of mathematics − see [Høyrup 2005a: 109−112] ⟦= article I.3⟧.

followed by an explanation like the one Jacopo gives after having found the circular area first according to the normal "Arabic" formula $(1-{}^{1}/_{2}\cdot{}^{1}/_{7})$ diameter^2 and next as (diameter$\times$perimeter)/4 [ed. Høyrup 2007b: 352*f*]:

> And you see that it becomes as the one above, which we make without knowing the circulation around, which is also *braccia* 44, and they become the same. And therefore I have made this beside that, so that you understand well one as well as the other, and that one as well as the other is a valid rule. And they go well.

Secondly, solutions to problems are regularly followed by numerical proofs (in the sense of "verifications"). At times these check directly that the application of the rule actually gives what is asked for; at times, however, only a more indirect check is possible, which means that the proof shows the compatibility of two approaches. In one such case, the Milan-Florence redaction of Jacopo's *Tractatus* observes [ed. Høyrup 2007b: 454] that "Thus we have made the alloy well, since we have found again the said 700 δ. It would have been a pity if we had found more or less".[33]

At times, the computations lead to approximate results − not only when the roots of non-square numbers are found but also, for instance, when discounting of a debt is computed by means of an iterative procedure or in application of *welsche Praktik* (a kind of combined division-cum-multiplication by stepwise emptying used in practical trade). In such cases I do not remember ever to have seen a proof. Consistency was apparently meant to be exact, and once approximations were made exactness could no longer be expected[34] − approximation, so to speak, was a one-way street leading away from the world of consistency toward that of measurement and business.

All in all (and many more arguments could be given from the texts), the following norms or expectations[35] can be seen to have regulated abbacus mathematics:[36]

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33 In modern elementary arithmetic we are accustomed to the need for rounding called forth by the use of decimal fractions, for which reason checks of many practical calculations will not be exact however correct the calculations. Since abbacus mathematics operated with genuine fractions it did not encounter that problem, and exactness was therefore possible.

34 In particular, I have never seen an analogue of the reversal of the approximate determination of a diagonal in the Old Babylonian text BM 96957+VAT 6598 # xxv [ed. Robson 1999: 259], made by reversal of the approximation formula.

35 "Norms or expectations": indeed, expectations concerning the *object* of the activity of abbacus masters ("mathematics") are involved along with norms for the way these masters *should act.* It might be better to speak of an "ideology" belonging with *abbaco* mathematical practice, since an ideology is exactly to be characterized as an inextricable fusion of descriptive and prescriptive (supposed) knowledge. Cf. [Høyrup 2000b: 342].

36 It may seem somewhat circular to read norms from a text corpus and then (as we shall do) apply them to understand the mathematical practice on which the same the same corpus is based. However, the norms are read out of one part of the corpus (the scattered casual remarks, the basic level), and we shall discuss their impact on other parts of the corpus, in particular the algebra.

- it should, *in so far as authors and users could do it respectively follow it*, be argued;
- it should be consistent;
- and it should be exact, unless some real-world application asked for approximation.

False rules revisited

How do these norms agree with the invention of the false algebraic rules? At the surface of things, not at all. Those of Gherardi can never have been argued in a pertinent way, and their inventor should have known so. Dardi's irregular rules were certainly derived from arguments, but arguments which could never be told publicly because they would show *how* restricted their validity was, while only Dardi and those who copied them from him reveal at all that their validity was restricted. They were never tested by the inventors (or if they were, the inventors did not betray themselves by telling the negative outcome), so the consistency they fulfil is merely that of superficial similarity.

The display of wrong results is thus not to be understood (as is the sweeping unacknowledged copying from the writings of predecessors) as "what was generally done and accepted at the time/within the environment". Instead it should be understood as a parallel of scientific fraud nowadays, which also exists, *in spite of* its conflict with what is expected from its perpetrators, and in the likeness of the economic fraud of Kreuger and the managers of Enron and Parmalat.

The background is also the same. Abbacus masters were in liberal profession, and had to impress municipal authorities or the fathers of prospective students if they wanted to earn their living. That could at best be done by solving problems that were too difficult for competitors; the prestige of algebraic problem solving (see note 26) made it an adequate instrument in that fight for distinction, and the inability of the judges to distinguish gilt lead from gold made it profitable to choose the easy way of fraud.

However, the fraud could only succeed *because of* the existence of those very norms which it violated. The general predilection for exactness barred check of the *approximate* validity of the false solutions, and faith was instilled by the expectation that abbacus authors had arguments for their mathematical claims even if their public − whether municipal councillors or fathers, perhaps even less brash competitors − felt themselves to be unable to follow these.[37] In the same way, Enron could generate faith by being the client of prestigious accountant firms like Arthur Andersen and PriceWaterhouse-Cooper,[38] and by being apparently successful operators on a market supposed to be transparent by nature even though common citizens cannot look through it.[39]

[37] More or less in the same vein, readers of the present pages probably suppose that I have really consulted the unpublished manuscripts I quote and to which they have no access (I promise I have!).

[38] See, for instance, [McNamee 2002].

[39] This is one aspect of what Robert Merton [1973: 439−459] baptized the "Matthew effect".

Norm systems, indeed, are double-edged. They keep together a social body and regulate the behaviour of most members of the body; but they also allow those who hide behind them without complying with them to be far more successful than they could have been without the trust of others in the norms and their effectiveness − beyond regulation, norm systems provide expectations, namely regarding the behaviour of others. No Tartuffe without religion and reverence for it!

Understanding Giovanni, and understanding more through Giovanni

Giovanni di Davizzo apparently did not know that his marvellous complete group had "no existence, if not that on the paper", in Georg Cantor's vicious words [1895: 501] about Veronese's transfinite numbers. In so far he may have profited from the cover of the norm system without actually knowing that he disobeyed it. This is not very illuminating, scientific mistakes are still made today, and if nobody discovers them to *be* mistakes their authors may earn degrees, positions and prestige from them in good faith.

But there is something more to say about what Giovanni did. His expansion of the semi-group may be seen as a search for consistency − but then not only for consistency as a condition that had to be obeyed but as something which should be actively created. Since his scheme was taken over by others, a fair number of abbacus writers seem to have shared the norm that mathematical knowledge ought to expand − since the scheme was completely useless for practical as well as mathematically-theoretical purposes, they can have adopted it for no other reason. This norm agrees well with a passage in the introduction to Jacopo's *Tractatus* [ed. Høyrup 2007b: 195] (copied more often than any other introduction by other abbacus authors and thus likely to correspond to prevailing moods):

> … by mind and good and subtle intelligence men make many investigations and compose many treatises which were not made by other people, and know to make many artifices and written arguments which for us bring to greater perfection things that were made by the first men.

This wish for expansion of the art throws further light on the creation of the false solutions: whereas being able to solve (or give pretended solutions) to complicated algebraic problems gave prestige, prestige (probably more prestige) was specifically conferred to those who expanded the reach of existing algebraic knowledge. This is also the reason that many historians of mathematics tend spontaneously to see the fraud as praiseworthy because

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Similarly, because Cyril Burt was already famous when he started making his glaring statistical fabrications, for decades nobody noticed their character (admittedly, it also played a role that his "conclusions" − the intellectual superiority of the better classes − were politically convenient). Cf. [Kamin 1977, *passim*].

of the cognitive ambition it reveals, as pointing toward the breakthroughs of del Ferro, Tartaglia and Cardano. However, we should rather reverse this verdict. Those who committed the fraud consciously had no ambition to expand knowledge − just as modern scientific swindlers they were parasites on the cognitive ambition of others. They gained their prestige because of an existing norm system but in fact, in so far as they succeeded in having their fraud accepted as good knowledge (and the abbacus frauds went undetected much longer than the Piltdown fabrication) they undermined the creation of genuine new knowledge.

The power of the norm system

Some palaeontologists doubted the Piltdown man from the very beginning, and in the end this notorious potpourri of man and ape was exposed.[40] Similarly, the invention of the "cube root with addition" shows us that not all abbacus authors believed in the Gherardi solution to equation (G2). Further, the reductions of other equation types in the anonymous treatise in which we find this peculiar "root" explained shows that the norm for expanding the art consistently could lead to genuinely extensions of mathematical insight − extensions which, when combined with the breakthrough of del Ferro etc., led to the solution of *all* cubics and quartics in the 16th century.

A similar argument could be made (now in contrast to Giovanni's "group") around the way the same text (as well as Pacioli in his *Summa*) correlates the algebraic powers with powers of a number (see note 24). This led directly toward the arithmetization of the sequence of such powers − for instance, Bombelli's arithmetical notation for powers, in which $\overset{n}{\smile}$ corresponds to our x^n.

Not to be contrasted with any fraud or fallacy is the use of purely formal algebraic operations − another consequence of the faith in the consistency and expandability of mathematics.

In the above-mentioned *Trattato dell'Alcibra amuchabile* from c. 1365 it is stated in direct words [ed. Simi 1994: 41*f*] that the addition

$$\frac{100}{a\ thing}+\frac{100}{a\ thing\ plus\ 5}$$

is to be performed "in the mode of a fraction", explained with the parallel $\frac{24}{4}+\frac{24}{6}$. It is thus taken for granted that operations with algebraic expressions could be handled exactly as numbers, and thus that for instance the notation for fractions was a mere form that could be filled out by any contents, numerical as well as algebraic.[41] This formal use

40 J. V. Field suggests as a possible reference J. S. Weiner, *The Piltdown Forgery* (Oxford University Press, 1955), read with thrill in young age. My own original familiarity with the affair comes from Danish popular-science articles from the same epoch. A recent very full bibliography is [Turrittin 2006].

41 We take note that formal operations could be made without abbreviations, even though the

of the fraction notation could not be used by Dardi, since he had already chosen to use the same notation for multiples of *ç* (*censo*) and *c* (*cosa*/"thing"), writing the "denominator" below the "numerator" with a stroke in between – for instance, $\frac{10}{c}$ for "10 things".[42] Nor was the usage broadly accepted at first (nor understood by all those who copied material where it was used[43]). In the longer run, however, mathematical writers got accustomed to it, and when Viète makes use of it in his *In artem analyticen isagoge* [ed. van Schooten 1646: 7*f*], all he feels the need to explain is his geometrical interpretation – for instance, that $\frac{B\ cubus}{A\ plano}$ is "the latitude which B cube makes when applied to A plane". When coming to the arithmetic of such fractions he just prescribes the customary operations for numerical fractions without mentioning this parallel as an argument − he appears simply to discover no difference.

The norm system which governed the practice of abbacus mathematics was not identical with that of Greek-inspired Humanist and university mathematics, and it could not be already because the practices they governed were different in spite of similarities. For instance, a request for exactness could not mean the same in numerical computation and in geometry made exclusively by ruler and compass.[44] But the two systems were sufficiently similar to one another to allow a merger, not only of the two types of mathematical knowledge but also of the two norm sets. We may remember that both Maurolico and Clavius in their voluminous production also wrote on abbacus matters although from the Humanist perspective, and that Clavius's stance on the matter of exactness was more tolerant than that of, for example, Viète and the classicist Kepler, at least for a while.[45] Without the partial merger of norms for what constituted legitimate

introduction of standard abbreviations was a prerequisite for maturation of the technique. Even though letter abbreviations had been used by both scholastic philosophers and Jordanus of Nemore, the true precursors of later mathematical symbols are the formal operations and standard abbreviations of abbacus algebra.

42 This notation (which Dardi did not invent, it is already in a manuscripts from c. 1334) had to remain unproductive because it did not use the fraction as a symbol for an arithmetical operation (*viz* a division) but linked it instead to something like the medieval *denominatio* for ratios, or (more likely) saw $/_3$ simply as an abbreviation for the (ordinal form of the) number 3. But it lived on for at least a century and a half alongside the formal operations, being still used in a German algebra from 1481 [Vogel 1981: 10].

43 In the *Libro di conti e mercatanzie* from c. 1395 (see p. 797), 100 divided by a *thing* plus 5 is thus stated [ed. Gregori & Grugnetti 1998: 103] to be "$\frac{1}{1\ thing}$ and 5", but afterwards the operations – copied from elsewhere – are performed correctly. The same treatise, it should be noted, solves the problem $C =\ \ C= \alpha+\sqrt{\beta}$ by taking the root of α and $\sqrt{\beta}$ separately, claiming the solution to be $t = \sqrt{\alpha}+\sqrt{\sqrt{\beta}}$ [ed. Gregori & Grugnetti 1998: 115*f*].

44 On the conflicts around the concept of exactness in the latter context, see [Bos 1993]. The conflict can also be seen when Viète – as much a Humanist mathematician as there ever was – insists on a meaningful geometric interpretation of the algebraic powers.

45 See [Bos 1993: 33−35] for a convenient confrontation, and [Bos 2001: 159−166] for the details

practice, 17th-century scientific mathematics would hardly have been able to integrate the tools created by abbacus algebra − and without the heritage from abbacus algebra, it would have remained restricted to the possibility of finding something *more of the same kind* (perhaps brilliant, but not very much more) with respect to the Greek heritage, just as had been the case for medieval Islamic theoretical geometry. The total transformation of the mathematical enterprise taking place from Descartes to (say) Bernoulli would not have been possible.

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Chapter 30 (Article II.13)
Hesitating Progress – The Slow Development Toward Algebraic Symbolization in Abbacus- and Related Manuscripts, C. 1300 to C. 1550

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Abstract

From the early 14th century onward, some Italian abbacus manuscripts begin to use particular abbreviations for algebraic operations and objects and, to be distinguished from that, examples of symbolic operation. The algebraic abbreviations and symbolic operations we find in German *Rechenmeister* writings can further be seen to have antecedents in Italian manuscripts. This might suggest a continuous trend or perhaps even an inherent logic in the process. Without negating the possibility of such a trend or logic, the paper will show that it becomes invisible in a close-up picture, and that it was thus not understood – nor intended – by the participants in the process.

Contents

IAN MUELLER and JEAN CASSINET
in memoriam

1. Before Italy

Ultimately, Italian abbacus algebra[1] descended from Arabic algebra – this is obvious from its terminology and techniques. I shall return very briefly to some of the details of this genealogy – not so much in order to tell what happened as to point out how things *did not* happen; this is indeed the best we can do for the moment.

First, however, let us have a look at Arabic algebra itself under the perspective of "symbolism".[2]

The earliest surviving Arabic treatise on the topic was written by al-Khwārizmī somewhere around the year 820.[3] It is clear from the introduction that al-Khwārizmī did not invent the technique: the caliph al-Ma'mūn, so he tells, had asked him to write a compendious introduction to it, so it must have existed and been so conspicuous that the caliph knew about it; but it may have existed as a technique, not in treatise form. If we are to believe al-Khwārizmī's claim that he choose to write about what was subtle and what was noble in the art (and why not believe him?), al-Khwārizmī's treatise is likely not to contain everything belonging to it but to leave out elementary matters.

It is not certain that al-Khwārizmī's treatise was the first of its kind, but of the rival to this title (written by the otherwise little known ibn Turk) only a fragment survives [ed. Sayılı 1962]. In any case it is clear that one of the treatises has influenced the other, and for our purpose we may take al-Khwārizmī's work to represent the beginning of written Arabic algebra well.

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[1] The "abbacus school" was a school training merchant youth and a number of other boys, 11-12 years of age, in practical mathematics. It flourished in Italy, between Genoa-Milan-Venice to the north and Umbria to the south, from c. 1260 to c. 1550. It taught calculation with Hindu numerals, the rule of three, partnership, barter, alligation, simple and composite interest, and single false position. Beyond this curriculum, many of the abbacus books (teachers' handbooks and notes, etc.) deal with the double false position, and from the 14th century onward also with algebra.

[2] 1 shall leave open the question of what constitutes an algebraic "symbolism", and adopt a rather tolerant stance. Instead of delimiting by definition I shall describe the actual character and use of notations.

[3] The treatise is known from several Arabic manuscripts, which have now appeared in a critical edition [Rashed 2007], and from several Latin translations, of which the one due to Gerard of Cremona [ed. Hughes 1986] is not only superior to the other translations as a witness of the original but also a better witness of the original Arabic text than the extant Arabic manuscripts as far as it goes (it omits the geometry and the chapter on legacies, as well as the introduction) – both regarding the grammatical format [Høyrup 1998] and as far as the contents is concerned [Rashed 2007: 89].

Al-Khwārizmī's algebra (proper) is consistently rhetorical. As al-Khwārizmī starts by saying [ed. Hughes 1986: 233], the numbers that are necessary in *al-jabr wa'l-muqābalah* are *roots, census* and simple numbers. *Census* (eventually *censo* in Italian) translates Arabic *māl*, a "possession" or "amount of money"; the *root* (*radix/jiḏr*, eventually *radice*) is its square root. As al-Khwārizmī explains, the root is something which is to be multiplied by itself, and the *census* that which results when the root is multiplied by itself; while the fundamental second-degree problems (on which presently) are likely to have *originated* as riddles concerned with a real amount of money and its square root (similar to what one finds, for instance, in Indian problem collections),[4] we see that the root is on its way to take over the role as basic unknown quantity (but only on its way), whereas "dirham" serves in al-Khwārizmī's exposition simply as the denomination for the number term, similarly to Diophantos's *monás*. In the first steps of a problem solution, the basic unknown may be posited as a *res* or *šay'*, "a thing" (*cosa* in Italian); but in second-degree problems it eventually becomes a *root*, as we shall see.

As an example of this we may look at the following problem [ed. Hughes 1986: 250]:[5]

> I have divided ten into two parts. Next I multiplied one of them by the other, and twenty-one resulted. Then you now know that one of the two sections of ten is a thing.[6] Therefore multiply that with ten with a thing removed, and you say: Ten with a thing removed times a thing are ten things, with a *census* removed, which are made equal to twenty-one. Therefore restore ten things by a census, and add a *census* to twenty-one; and say: t e n t h i n g s a r e m a d e e q u a l t o t w e n t y - o n e a n d a *c e n s u s*. T h e r e f o r e h a l v e t h e r o o t s, a n d t h e y w i l l b e f i v e, w h i c h y o u m u l t i p l y w i t h i t s e l f, a n d t w e n t y - f i v e r e s u l t s. F r o m t h i s y o u t h e n t a k e a w a y t w e n t y - o n e, a n d f o u r r e m a i n s. W h o s e r o o t y o u t a k e, w h i c h i s t w o, a n d y o u s u b t r a c t i t f r o m t h e h a l f o f t h e t h i n g s. T h e r e t h u s r e m a i n s t h r e e, w h i c h i s o n e o f t h e p a r t s.

This falls into two sections. The first is a rhetorical-algebraic reduction which more or less explains itself.[7] There is not a single symbol here, not even a Hindu-Arabic numeral. The second section, marked in spaced writing, is an unexplained algorithm, and indeed

[4] Correspondingly, the "number term" is originally an amount of *dirham* (in Latin *dragmata*), no pure number.

[5] My translation into English, as everywhere in the following when no translator into English is identified.

[6] This position was already made in the previous problem about a "divided ten".

[7] However, those who are already somewhat familiar with the technique may take note of a detail: we are to restore ten things with a *census, and then* add a *census* to 21. "Restoring" (*al-jabr*) is thus not the addition to both sides of the equation (as normally assumed, in agreement with later usage) but a reparation of the deficiency on that side where something is lacking; this is followed by a corresponding addition to the other side.

a reference to one of six such algorithms for the solution of reduced and normalized first- and second-degree equations which have been presented earlier on.

Al-Khwārizmī is perfectly able to multiply two binomials just in the way he multiplies a monomial and a binomial here. He would thus have no difficulty in finding that a "root diminished by five" multiplied by itself gives a "*census* and twenty-five, diminished by ten roots". But he cannot go the other way, the rhetorical style and the way the powers of the unknown are labelled makes the dissolution of a trinomial into a product of two binomials too opaque either for al-Khwārizmī himself or for his "model reader". In consequence, when after presenting the algorithms al-Khwārizmī wants to give proofs for these, his proofs are geometric, not algebraic – geometric proofs not of his own making (as are his geometric illustrations of how to deal with binomials), but that is of no importance here.

It is not uncommon that rhetorical algebra like that of al-Khwārizmī is translated into letter symbols, the *thing* becoming x and the *census* becoming x^2. The above problem and its solution thereby becomes

$$10 = x + (10-x)\,,\ x\cdot(10-x) = 21$$

$$10x - x^2 = 21$$

$$10x = 21 + x^2$$

$$x = \frac{10}{2} - \sqrt{(\frac{10}{2})^2 - 21}$$

To the extent that this allows us to follow the steps in a medium to which we are as accustomed as the medieval algebraic calculators were to the use of words, it may be regarded as adequate. But only to this extent: the letter symbolism makes it so much easier to understand the dissolution of trinomials into products that the need for geometric proofs becomes incomprehensible – which has to do with the theme of our meeting.

Geometric proofs recur in many later Arabic expositions of algebra – not only in Abū Kāmil but also in al-Karajī's *Fakhrī* [Woepcke 1853: 65–71], even though al-Karajī's insight in the arithmetic of polynomials[8] would certainly have allowed him to offer purely algebraic proofs (his *Al-Badī'* explicitly shows how to find the square root of a polynomial [ed. Hebeisen 2008: 117–137]). What is more: he brings not only the type of proof that goes back to al-Khwārizmī but also the type based directly on *Elements* II (as introduced by Thābit ibn Qurrah, ed. [Luckey 1941]).

Some Arabic writers on algebra give no geometric proofs – for instance, ibn Badr and ibn al-Bannā'. That, however, is because they give no proofs at all; algebraic proofs

8 Carried by a purely rhetorical exposition, only supplemented by use of the particle *illā* ("less") – still a word, but used contrary to the rules of grammar in the phrase *wa illā*, "and less" – to mark a subtractive contribution. As pointed out by Mahdi Abdeljaouad [2002: 38], this implies that *illā* has become an attribute (namely subtractivity) of the number.

فإذا قيل لك اضرب ثمانية أشياء إلاّ أربعة من العدد في ستة أموال إلاّ ثلاثة أشياء فأنزل ذلك هكذا 8ش الا 4 في 6م الا 3ش .

ثم اضرب الثمانية في الستة يخرج لك ثمانية وأربعون (171) كعبا ، لأن أس المضروبين 6م و8ش ثلاثة ، احفظها أولا ، ثم اضرب الثمانية في الثلاثة يخرج لك أربعة وعشرون مالا ، وهو ناقص ، لأنه من ضرب زائد في ناقص ، احفظه بحرف الاستثناء ، ثم اضرب الأربعة في الستة يخرج لك اربعة وعشرون مالا ناقصا أيضا ، ضعه مع نظيره ، ثم اضرب أيضا الاربعة في الثلاثة يخرج لك اثنا عشر شيئا زائدا لأنه من ضرب [/33و] ناقص في مثله ، اجعله مع المحفوظ الأول ، فيكون الخارج اثني عشر شيئا وثمانية واربعين كعبا إلاّ ثمانية واربعين مالا هكذا : 12ش 48ك الا 48م

Al-Qalasādī's explanation of how to multiply «8 things less 4» by «6 *census* less 3 things» in Souissi's edition [1988: Ar. 96] – symbolic notations in frames

Donc si l'on vous dit : multipliez huit choses moins quatre en nombre par six carrés moins trois choses, posez cela ainsi :

C
4 moins 8
C Q
3 moins 6

Ensuite multipliez le huit par le six. Vous aurez pour résultat quarante huit cubes , parce que le fond des deux facteurs est trois. Réservez cela. Après cela multipliez de nouveau le huit par les trois choses. Vous aurez pour résultat vingt quatre carrés, ce qui est négatif, parce que cela (provient) de la multiplication du positif par le négatif. Réservez cela (en le plaçant) après la particule de l'exception. Puis multipliez le quatre par le six. Vous aurez pour résultat vingt quatre carrés. Mais cela est de nouveau négatif. Placez-le avec son analogue (***). Ensuite multipliez encore le quatre par le trois. Vous aurez pour résultat douze choses positives, parce que cela (provient) de la multiplication du négatif par le négatif. Réservez cela avec le premier (produit) réservé. Le résultat sera douze choses et quarante huit cubes moins quarante huit carrés, ainsi :

Q K C
48 moins 48 12

Figure 1

for the solution of the basic equations are absent from the entire Arabic tradition.[9]

This complete absence is interesting by showing that we should expect no direct connection between the existence of an algebraic symbolism and the creation of the kind of reasoning it seems with hindsight to make possible. It has indeed been known since Franz Woepcke's work in [1854] that elements of algebraic symbolism were present in the Maghreb, at least in the mid-15th century (they are found in al-Qalaṣādi's *Kašf*,[10] but also referred to by ibn Khaldūn). Woepcke points to symbols for powers of the unknown and to signs for subtraction, square root and equality; symbols for the powers[11] are written above their coefficient, and the root sign above the radicand. He shows that these symbols (derived from the initial letters of the corresponding words, prolonged so as to be able to cover composite expressions, that is, to delimit algebraic parentheses) are used to write polynomials and equations, and even to operate on the equations.[12] Making the observation (p. 355) that

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9 An interesting variant is found in ibn al-Hā'im's *Šarh al-Urjūzah al-Yasmīnya*, "Commentary to al-Yāsamin's *Urjuza*" from 1387 [ed. trans. Abdeljaouad 2004: 18*f*]. Ibn al-Hā' im explains that the specialists have a tradition for giving geometric proofs, by lines (*viz*, as Thābit) or by areas (*viz*, as al-Khwārizmī), which however presuppose familiarity with Euclid. He therefore gives an arithmetical argument, fashioned after *Elements* II.4. For use of this theorem he is likely to have had precursors, since Fibonacci also seems to model his first *geometric* proof after this proposition [ed. Boncompagni 1857: 408] (his second proof is "by lines").

10 The use of the symbols can thus be seen in Mohamed Souissi's edition [1988]. His translation renders the same expressions in post-Cartesian symbols; edition as well as translation change the format of the text (unless this change of format has already taken place in the manuscript he uses, which is not to be excluded). Woepcke's translation [1859] renders the formulae more faithfully (using **K** for the cube, **Q** for the square and **C** for the unknown itself), and also renders the original format better (putting the symbolic notations outside the text). Figure 1 confronts Woepcke's translation with Souissi's Arabic text.

11 There are individual signs for the *thing*, the *census* and the *cube*. Higher powers are represented by products of these (the fifth power thus with the signs for *census* and *cube*, one written above or in continuation of the other, corresponding to the verbal name *māl ka'b*. However, the arithmetization of the sequence of "powers" (i.e., exponents) was present. Ibn al-Bannā' must have known it, since he says (he was a purist) that it is not "allowed" to speak of the power of the *māl* (as 2), *viz* because it is an entity of its own; ibn Qunfudh (1339–1407), in the commentary from which we know this prohibition, states that other writers on algebra did not agree, and speaks himself of the power of the *number* as "nothing", that is, 0 [Djebbar 2005: 95*f*]. The individual names for the powers should thus not have been a serious impediment for the development of algebraic proofs, had the intention been there to develop them.

12 Three points should perhaps be made here. One concerns terminology. "Parenthesis" does not designate the bracket but the expression that is marked off, *for example* by a pair of brackets; but pauses may also mark off a parenthesis in the flow of spoken words, and a couple of dashes may do so in written prose. What characterizes an algebraic parenthesis is that it marks off a single entity which can be submitted to operations as a whole, and therefore has to be calculated first in the

> the indispensable condition for characterizing conventional signs as a notation is that they are always employed when it fits, and always in the same way

he shows that one manuscript at his disposal fulfils this condition (another one not, probably because of "the negligence of a copyist or a sequence of copyists").

Ibn Khaldūn's description made Woepcke suspect that the notation goes back to the 12th century, as has now been confirmed by two isolated passages in ibn al-Yāsamin's *Talqīḥ al-afkār* reproduced by Abdeljaouad [2002: 11] after Touhami Zemmouli's master thesis and corresponding exactly to what al-Qalaṣādi was going to do – one of them is shown in Figure 2.

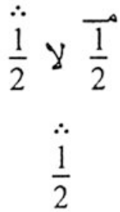

Figure 2. Ibn al-Yāsamin's scheme for multiplying $^1/_2$ *māl* less $^1/_2$ *šay'* by $^1/_2$ *šay'*.

Though manuscripts differ in this respect (as observed by Woepcke), the symbolic calculations appears to have been often made separate from the running text (as shown in Woepcke's translation of al-Qalaṣādi), usually preceded by the expression "its image is". They illustrate and duplicate the expressions used by words. They may also stand as marginal commentaries, as in the "Jerba manuscript" (written in Istanbul in 1747) of ibn al-Hā'im's *Šarh al-Urjūzah al-Yasmīnya*, "Commentary to al-Yāsamin's *Urjuza*" (originally from 1387) [ed. Abdeljaouad 2004], of which Figure 3 shows a page. According to ibn Mun'im (†1228) and al-Qalaṣādī, these marginal calculations may correspond to what was to be written in a *takht* (a dustboard, in particular used for calculation with Hindu numerals) or a *lawha* (a clayboard used for temporary writing) – see [Lamrabet 1994: 203] and [Abdeljaouad 2002: 14, 19*f*]. The use of such a device would explain that the examples of symbolic notation we find in manuscripts normally do not contain intermediate calculations, nor erasures [Abdeljaouad 2002: 20].

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case of calculations. When division is indicated by a fraction line, this line delimits the numerator as well as the denominator as parentheses if they happen to be composite expressions (for instance, polynomials). Similarly, the modern root sign marks off the radicand as a parenthesis.

The remaining points are substantial, one of them general. The possibility of "embedding" is fundamental for the unrestricted development of mathematical thought, as I discuss in [Høyrup 2000]. An algebraic language without full ability to form parentheses and manipulate them is bound to remain "close to earth". ⟦Article II.14 goes much deeper into the parenthesis topic.⟧

The last point, also substantial, is specific and concerns the Maghreb notation. It did not use the parenthesis function to the full. The fraction line and the root sign might mark off polynomials as parentheses; the signs for powers of the unknown, on the other hand, might at most mark off a composite numerical expression as coefficient – see [Abdeljaouad 2002: 25–34] for a much more detailed exposition. This should not surprise us: even Descartes eschewed general use of the parenthesis, for instance, expressions like $(y–3)^2$, as pointed out by Michel Serfati [1998: 259].

Figure 3. A page from the "Jerba manuscript" of ibn al-Hā'im's *Šarh al-Urjuūzah al-Yasmīya* [ed. Abdeljaouad 2004: Ar. 45].

We are accustomed to consider the notation for fractions as something quite separate from algebraic symbolism. In 12th-century Maghreb, the two probably belonged together,[13] and from al-Ḥaṣṣār's *Kitāb al-bayān wa'l-taḏhkār* onward Maghreb mathematicians used the various notations with which we are familiar from Fibonacci's *Liber abbaci* (and other works of his) (simple fractions written with the fraction line, ascending continued fractions ($\frac{e}{f}\frac{c}{d}\frac{a}{b}$ meaning $\frac{a}{b}+\frac{c}{bd}+\frac{e}{bdf}$), and additively and multiplicatively compounded fractions – see [Lamrabet 1994: 180*f*] and [Djebbar 1992: 231–234].

2. Latin algebra: *Liber mahameleth, Liber abbaci*, translations of al-Khwārizmī – and Jordanus

The earliest documents in our possession from "Christian Europe" which speak of algebra are the *Liber mahameleth* and, with a proviso, Robert of Chester's translation of al-Khwārizmī's *Algebra* (c. 1145); slightly later is Gerard of Cremona's translation of al-Khwārizmī's treatise. All of these are from the 12th century. From 1228 we have the algebra chapter in Fibonacci's *Liber abbaci* (the first edition from 1202 was probably rather similar, but we do not know *how* similar). In his *De numeris datis*, Jordanus of Nemore presented an *alternative to algebra*, showing how its familiar results could be based in (rather) strictly deductive manner on his *Elements of Arithmetic* – but he avoided to speak about algebra (hinting only for connoisseurs at the algebraic sub-text by using many of the familiar numerical examples); see the analysis in [Høyrup 1988: 332–336]. Finally, around 1300 a revised version of al-Khwārizmī's *Algebra* of interest for our topic was produced (ed. [Kaunzner 1986], cf. [Kaunzner 1985]).

The *Liber mahameleth* and the *Liber abbaci* share certain characteristics, and may therefore be dealt with first.

All extant manuscripts of the *Liber mahameleth*[14] have lost an introductory systematic presentation of algebra, which however is regularly referred to.[15] There are also references to Abū Kāmil,[16] and a number of problem solutions make use of algebra. Fractions are written in the Maghreb way, with Hindu numerals and fraction line;[17]

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13 Cf. the hypothesis of Abdeljaouad [2002: 16–18], that "l'algèbre symbolique est un chapitre de l'arithmétique indienne maghrébine".

14 1 have consulted [Sesiano 1988] and a photocopy of the manuscript Paris, Bibliothèque Nazionale, ms. latin 7377A. ⟦The two critical editions, [Vlasschaert 2010] and [Sesiano 2014], published in the meantime do not change the situation.⟧

15 Thus fol. 154^{v}, "sicut docuimus in algebra"; fol. 161^{r}, "sicut ostensum est in algebra".

16 Thus fol. 203^{r}, "modum agendi secundum algebra, non tamen secundum Auoqamel"; cf. [Sesiano 1988: 73*f* 95*f*].

We may observe that the spelling "Auoqamel" reflects an Iberian pronunciation.

17 However, ascending continued fractions are written in a mixed system and not in Maghreb-notation

there are also copious marginal calculations in rectangular frames probably rendering computation on a *lawha.* However, one finds no more traces of algebraic symbolism than in al-Khwārizmī's and Abū Kāmil's algebraic writings.

Fibonacci uses Maghreb fraction notations to the full in the *Liber abbaci* [ed. Boncompagni 1857], writing composite fractions from right to left and mixed numbers with the fraction to the left – all in agreement with Arabic custom. Further, he often illustrates non-algebraic calculations in rectangular marginal frames suggesting a *lawha.* That systematic presentation of the algebraic technique which has been lost from the *Liber mahameleth* is present in the *Liber abbaci;* there is no explicit reference to Abū Kāmil, but there are unmistakeable borrowings (which could of course be indirect, mediated by one or more of the many lost treatises). When the "thing" technique is used in the solution of commercial or recreational first-degree problems,[18] it is referred to as *regula recta*, not as algebra. But in one respect their algebras are similar: they are totally devoid of any hint of algebraic symbolism.[19] Inasfar as the *Liber mahameleth* is concerned, this could hardly be otherwise – it antedates the probable creation of the Maghreb algebraic notation.

Equally devoid of any trace of symbolism is Gerard's translation of al-Khwārizmī, which is indeed very faithful to the original – to the extent that no Hindu numerals nor fraction lines occur, everything is completely verbal.

Robert does use Hindu numerals heavily in his translation (as we know it), but apart from that his translation is also fully verbal. It has often been believed, on the faith of Louis C. Karpinski's edition [1915: 126] that his translation describes an algebraic formalism. It is true that the manuscripts contain a final list of *Regule 6 capitulis algabre correspondentes* making use of symbols for *census, thing* and *dragma* (the "unit" for the number term, we remember); they are classified as an appendix by Barnabas Hughes [1989: 67], but even he appears (p. 26) to accept them as genuine. However, the symbols are those known from the southern Germanic area of the later 15th century, and all three manuscripts were indeed written in this area during that very period [Hughes 1989:

– e.g., "$\frac{4}{5}$ et $\frac{2}{5}$ unius sue $\frac{e}{5}$" (fol. 167r) for - $\frac{4}{5} + \frac{2}{5} \cdot \frac{1}{5}$ ($\frac{e}{5}$ means "quinte").

18 The *Liber mahameleth* contains several pseudo-commercial problems involving the square root of an amount of money, leading to second-degree problems – see [Sesiano 1988: 80, 83]. The *Liber abbaci* contains nothing of the kind, and no second-degree problems outside the final chapter 15.

19 Florian Cajori [1928: I, 90] has observed a single appearance of ℞ in the *Pratica geometrie* [ed. Boncompagni 1862: 209]. Given how systematically Fibonacci uses his notations for composite fractions we may be sure that this isolated abbreviation is a copyist's slip of the pen (the manuscript is from the 14th century, when this abbreviation began to spread). Marginal reader's notes in a manuscript of the *Flos* are no better evidence of what Fibonacci did himself.

11–13].[20] The appendix has clearly crept in some three centuries after Robert made his translation.

Far more interesting from the point of view of symbolism is the anonymous redaction from around 1300. It contains a short section *Qualiter figurentur census*, *radices et dragma*, "How *census*, *roots* and dragmas are represented" [ed. Kaunzner 1986: 63*f*].[21] Here, *census* is written as *c*, roots as *r*, and *dragmata* (the unit for number) as *d* or not written at all. If a term is subtractive, a dot is put under it. These symbols are written below the coefficient, not above, as in the Maghreb notation. In Figure 4 we see (redrawn from photo and following Wolfgang Kaunzner's transcription) "2 *census less* 3 *roots*", "2 *census less* 4 *dragmata*", "5 *roots less* 2 *census*, and "5 *roots less* 4 *dragmata*". Outside this section, the notation is not used, which speaks against its being an invention of the author of the redaction; it rather looks as if he reports something he knows from elsewhere, and which, as he says, facilitates the teaching of algebraic computation. He refers not only to additive-subtractive operations but also to multiplication, stating however only the product of *thing* by *thing* and of *thing* by *number*. He can indeed do nothing more, he has not yet explained the multiplication of binomials. The notation is certainly not identical with what we find in the Maghreb texts; the similarity to what we find in al-Ḥaṣṣār and al-Qalaṣādī is sufficiently great, however, to suggest some kind of inspiration – very possibly indirect. However that may be: apart from an Italian translation from c. 1400 (Vatican, Urb. lat. 291), where *c* is replaced by *s* (for *senso*) and *r* by *c* (for *cose*), no influence in later writings can be traced. A brief description of a notation which is not used for anything was obviously not understood to be of great importance (whether

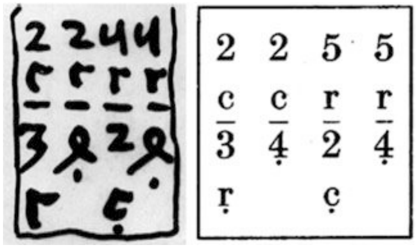

Figure 4. From Oxford, Bodleian Library, Lyell 52, fol. 45r [Kaunzner 1986: 64*f*].

20 One of them is an abbreviation of the spelling *zenso/zensus*, the spelling of many manuscripts from northern Italy (below, note 87). The spelling *zensus* as well as the abbreviation were taken over in Germany (as the north-Italian spelling *cossa* was taken over as *coss*); the spelling was unknown in 12th-century Spain, and the corresponding abbreviation could therefore never have been invented in Spain in 1145.

21 This redaction is often supposed to be identical with a translation made by Guglielmo de Lunis. However, all references to this translation (except a false ascription of a manuscript of the Gerard translation) borrow from it a list of Arabic terms with vernacular explanation which is absent from the present Latin treatise. It is a safe conclusion that Guglielmo translated into Italian; that his translation is lost; and that the present redaction is to be considered anonymous. ⟦See article I.12.⟧

the redactor believed it to be can also be doubted, given that he does not insist by using it in the rest of the treatise).

Jordanus of Nemore's *De numeris datis* precedes this redaction of al-Khwārizmī by a small century of so.[22] It is commonly cited as an early instance of symbolic algebra, and as a matter of fact it employs letters as general representatives of numbers. At the same time it is claimed be very clumsy – which might suggest that the interpretation as symbolic algebra could be mistaken. We may look at an example:[23]

> If a given number is divided into two and if the product of one with the other is given, each of them will also be given by necessity.
>
> Let the given number *abc* be divided into *ab* and *c*, and let the product of *ab* with *c* be given as *d*, and let similarly the product of *abc* with itself be *e*. Then the quadruple of *d* is taken, which is *f*. When this is withdrawn from *e, g* remains, and this will be the square on the difference between *ab* and *c*. Therefore the root of *g* is extracted, and it will be *b*, the difference between *ab* and *c*. And since *b* will be given, *c* and *ab* will also be given.

As we see, Jordanus does not operate on his symbols, every calculation leads to the introduction of a *new* letter. What Jordanus has invented here is a symbolic representation of an *algorithm*, not clumsy symbolic algebra.

The same letter symbolism is used in Jordanus's *De elementis arithmetice artis*, which is presupposed by the *De numeris datis* and must hence be earlier. In the algorithm treatises, letters are used to represent unspecified digits [Eneström 1907: 146]; in the two demonstrations that are quoted by Eneström (pp. 140*f*), the revised version can be seen also to use the mature notation, while it is absent from the early version. The assumption is close at hand that Jordanus developed the notation from the representation of digits by letters in his earliest work; it is hard to imagine that it can have been inspired in any way by the Maghreb notations. This representation of digits *might* have given rise to an algebraic symbolism – but as we see, that was not what Jordanus aimed at. Actually –

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[22] As well known, the only certain date *ante quem* for Jordanus is that all his known works appear in Richard de Fournival's *Biblionomia* [ed. de Vleeschauwer 1965], which was certainly written some time before Richard's death in 1260 [Rouse 1973: 257]. However, one manuscript of Jordanus's *Demonstratio de algorismo* (Oxford, Bodleian Library, Savile 21) seems to be written by Robert Grosseteste in 1215–16, and in any case at that moment [Hunt 1955: 134]. This is the revised version of Jordanus's treatise on algorism. In consequence, Jordanus must have been beyond his first juvenile period by then. It seems likely (but of course is not certain) that the arithmetical works (the *Elements* and the *Data* of arithmetic) are closer in time to the beginning of his career that works on statics and on the geometry of the astrolabe, and that they should therefore antedate 1230.

[23] Translated from [Hughes 1981: 58] (Hughes' own English translation is free and therefore unfit for the present purpose).

Juxtaposition of letters is meant as aggregation, that is, addition (in agreement with the Euclidean understanding of number and addition).

as mentioned above – he did not characterize his *De numeris datis* as algebra even though he shows that he knows it to be at least a (theoretically better founded) *alternative to algebra.*

There are few echoes of this alternative in the following centuries. When taking up algebra in the mid-14th century in his *Quadripartitum numerorum* ([ed. l'Huillier 1990], cf. [l'Huillier 1980]), Jean de Murs borrows from the *Liber abbaci*, not from Jordanus. Somewhere around 1450, Georg Peurbach refers in a poem to "what algebra calculates, what Jordanus demonstrates" [ed. Größing 1983:210], and in his Padua lecture from 1464 [ed. Schmeidler 1972: 46], Johannes Regiomontanus refers in parallel to Jordanus's "three most beautiful books about given numbers" and to "the thirteen most subtle books of Diophantos, in which the flower of the whole of arithmetic is hidden, namely the art of the thing and the *census*, which today is called algebra by an Arabic name". Regiomontanus thus seems to have been aware of Jordanus's connection to algebra, and he also planned to print Jordanus's work (but suddenly died before his printing plans were realized).[24]

Two German algebraists from the 16th century knew, and used, Jordanus's quasi-algebra: Adam Ries and Johann Scheubel. The codex known as "Adam Ries' *Coß*" [ed. Kaunzner and Wußing 1992] includes a fragment of an originally complete redaction of the *De numeris datis*, containing the statements of the propositions in Latin and in German translation, and for each statement an alternative solution of a numerical example by cossic technique; Jordanus's general proofs as well as his letter symbols have disappeared [Kaunzner and Wußing 1992, II: 92–100]. From Scheubel's hand, a complete manuscript has survived. It has the same character – as Barnabas Hughes says in his description [1972: 222*f*], "Scheubel's revision and elucidation [...] has all the characteristics of an original work save one: he used the statements of the propositions enunciated by Jordanus". Both thus did to Jordanus exactly what Jordanus had done to Arabic algebra: they took over his problems and showed how their own technique (basically that of Arabic algebra) allowed them to deal with them in what *they* saw as a more satisfactory manner. Jordanus's treatise must thus have had a certain prestige, even though his technique appealed to nobody.[25]

24 As we shall see, these prestigious representatives of Ancient and university culture had no impact on Regiomontanus's own algebraic practice.

25 Vague evidence for prestige can also be read from the catalogue the books belonging to a third Vienna astronomer (Andreas Stiborius, c. 1500). Three neighbouring items in the list are *dedomenorum euclidis. Iordanus de datis. Demonstrationes cosse* [Clagett 1978: 347]. Whether it was Stiborius (in the ordering of his books) or Georg Tannstetter (who made the list) who understood *De numeris datis* as belonging midway between Euclid's *Data* and algebra remains a guess.

I only know of two works where Jordanus's letter formalism turns up after his own times, both from France. One is Jacques Lefèvre d'Étaples' edition of Jordanus's *De elementis arithmetice artis* ([1514], first edition 1496). The other is Claude Gaspar Bachet's *Problemes plaisans et delectables, que se font par les nombres* ([1624], first edition 1612), where (for the first and only time?) Jordanus's technique is used actively and creatively by a later mathematician.[26]

3. Abbacus writings before algebra

The earliest extant abbacus treatises are roughly contemporary with the al-Khwārizmī-redaction (at least the originals – what we have are later copies). They contain no algebra, but their use of the notations for fractions is of some interest.

Traditionally, a *Livero dell'abbecho* [ed. Arrighi 1989] conserved in the codex Florence, Ricc. 2404, has been supposed to be the earliest extant abbacus book, "internal evidence" suggesting a date in the years 1288–90. Since closer analysis reveals this internal evidence to be copied from elsewhere, all we can say on this foundation is that the treatise postdates 1290 [Høyrup 2005: 47 n. 57] – but not by many decades, see imminently. ⟦What we have, however, is a 14h-century *de luxe* vellum copy.⟧

The treatise claims in its incipit to be "according to the opinion" of Fibonacci. Actually, it consists of two strata – see the analysis in [Høyrup 2005]. One corresponds to the basic curriculum, and has nothing to do with Fibonacci; the other contains advanced matters, translated from the *Liber abbaci* but demonstrably often without understanding.

The Fibonacci-stratum copies his numbers, not only his mixed numbers with the fraction written to the left ($\frac{2}{7}10$ where we would write $10\frac{2}{7}$) but also his ascending continued fractions (written, we remember, in Maghreb notation, and indeed from right to left, as done by al-Ḥaṣṣār, cf. above). However, the compiler does not understand the notation, at one place [ed. Arrighi 1989: 112], for instance, he changes $\frac{33}{53}\frac{6}{53}\frac{42}{53}\frac{46}{53}$, standing in the *Liber abbaci* [ed. Boncompagni 1857: 273] for

$$\frac{46+\frac{42+\frac{6+\frac{33}{53}}{53}}{53}}{53},$$

into $\frac{3364246}{53535353}$. It is obvious, moreover, that he has not got the faintest idea about algebra: he mostly omits Fibonacci's alternative solutions by means of *regula recta;* on one occasion where he does not (fol. 83^{r}, ed. [Arrighi 1989: 89]) he skips the initial position and afterwards translates *res* as an ordinary, not an algebraic *cosa.*[27]

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[26] In order to discover that, one has to go to the 17th-century editions. A. Labosne's "edition" [1959] is a paraphrase in modern algebraic symbolism. Ries and Stifel were not the last of their kind.

[27] This total ignorance of everything algebraic allows us to conclude that the treatise cannot be

The basic stratum contains none of the composite fractions (but ordinary fractions written with fraction line). Very strange is its way to speak of concrete mixed numbers. On the first few pages they look quite regular – e.g. "d. 6 $\frac{27}{28}$ de denaio", meaning "*denari* 6, $\frac{27}{28}$ of a *denaro*". Then, suddenly (with some slips that show the compiler to copy from material written in the normal way) the system changes, and we find expressions like "d $\frac{2}{7}$ 4 de denaio", "*denari* $\frac{2}{7}$ 4 of a *denaro*" – obviously a misshaped compromise between Fibonacci's way to write mixed numbers with the way of the source material, which hence can *not* have been produced by Fibonacci (all his extant works write simple and composite fractions as well as mixed numbers in the same way as the *Liber abbaci*). All in all, the *Livero dell'abbecho* is thus evidence, on one hand, that the Maghreb notations adopted by Fibonacci had not gained foothold in the early Italian abbacus environment; and on the other, that the aspiration of the compiler to dress himself in the robes of the famous culture hero was not accompanied by understanding of these notations (nor of other advanced matters presented by Fibonacci).

The other early abbacus book is the *Columbia Algorism* (New York, Columbia University, MS X 511 A113, ed. [Vogel 1977]). The manuscript was written in the 14th century, but a new examination of a coin list which it contains dates this list to the years 1278–1284 [Travaini 2003: 88–92]. Since the shapes of numerals are mostly those of the 13th century (with occasional slips, where the scribe uses those of his own epoch) [Vogel 1977: 12], a dating of the original close to the coin list seems plausible – for which reason we must suppose the *Columbia Algorism* to be (a fairly scrupulous copy of) the oldest extant abbacus book.

There is no trace of familiarity with algebra, neither a systematic exposition nor an occasional algebraic *cosa*. *A fortiori*, there is no algebraic symbolism whatsoever, not even rudiments. Another one of the Maghreb innovations is present, however [Vogel 1977: 13]. Ascending continued fractions turn up several times, sometimes in Maghreb notation, but once reversed and thus to be read from left to right ($\frac{1\ 1}{4\ 2}$ standing for $\frac{3}{8}$). Nothing else suggests any link to Fibonacci. Moreover, the notation is used in a way never found in the *Liber abbaci*, the first "denominator" being sometimes the metrological denomination – thus $\frac{1\ \ \ 1}{gran\ 2}$ being used for $1\frac{1}{2}$ *gran* (or rather, as it would be written elsewhere in the manuscript, for 1 *gran* $\frac{1}{2}$ (of *gran*)). Next, the *Columbia Algorism* differs from all other Italian treatises (including those written in Provence by Italians) in its formulation of the rule of three – but in a way which approaches it to Ibero-Provençal writings of abbacus type – see [Høyrup 2008: 5*f*] ⟦see also article I.5⟧. Finally, at least one problem in the *Columbia Algorism* is strikingly similar to a problem found in a Castilian manuscript written in 1393 (copied from an earlier original) while not appearing elsewhere in sources I have inspected – see [Høyrup 2005: 42 n. 32]. In conclusion it seems reasonable to

written many decades after 1290.

assume that the *Columbia Algorism* has learned the Maghreb notation for ascending continued fractions *not* from Fibonacci but from the Iberian area.

4. The beginning of abbacus algebra

The earliest abbacus *algebra* we know of was written in Montpellier in 1307 by one Jacopo da Firenze (or Jacobus de Florentia; otherwise unknown as a person). It is contained in one of three manuscripts claiming to represent his *Tractatus algorismi* (Vatican, Vat. lat. 4826; the others are Florence, Ricc. 2236, and Milan, Trivulziana 90).[28] As it follows from in-depth analysis of the texts [Høyrup 2007a: 5–25 and *passim*], the Florence and Milan manuscripts represent a revised and abridged version of the original, while the Vatican manuscript is a meticulous copy of a meticulous copy of the shared archetype for all three manuscripts (extra intermediate steps not being excluded, but they must have been equally meticulous if they exist); this shared archetype could be Jacopo's original, but also a copy written well before 1328.[29]

Jacopo may have been aware of presenting something new. Whereas the rest of the treatise (and the rest of the vocabulary in the algebra chapter) employs the standard abbreviations of the epoch and genre, the algebraic technical vocabulary is never abbreviated.[30] Even *meno* ("less"), abbreviated m̃ in the coin list, is written in full in the algebra section. Everything here is rhetorical, there is not the slightest hint of any symbolism. We may probably take this as evidence that Jacopo was aware of writing about a topic the reader would not know about in advance (the book is stated also to be intended for independent study), and that his algebra is not only the earliest extant Italian algebra

[28] The Vatican manuscript can be dated by watermarks to c. 1450, the Milan manuscript in the same way to c. 1410. The Florence manuscript ⟦written on vellum and hence carrying no watermarks⟧ is undated but slightly more removed from the precursor it has in common with the Milan manuscript (which of course does not automatically make it younger but disqualifies it as a better source for the original).

[29] Comparing only lists of the equation types dealt with in various abbacus algebras and believing in a steady progress of their number within each family, Warren Van Egmond claims [2008: 313] that the algebra of the Vatican manuscript "falls entirely within the much later and securely dated Benedetto tradition and was undoubtedly added to a manuscript containing some sections from JACOPO's earlier work" (actually it contains fewer types than the manuscript from c. 1390 which Van Egmond takes as the starting point for this tradition). If he had looked at the words used in the manuscripts he refers to he would have discovered that the Vatican algebra agrees verbatim with a section of an algebra manuscript from c. 1365, which however fills out a calculational lacuna left open in the Vatican manuscript and therefore represents a more developed form of the text (and combines it with other material – details in [Høyrup 2007a: 163*f*]). Van Egmond's dating can be safely dismissed.

[30] There is *one* instance of ℞ (fol. 44^{r}, ed. [Høyrup 2007a: 326]); as the single appearance of ℞ in Fibonacci's *Pratica geometrie* (see note 19), this is likely to be a copyist's *lapsus calami.*

but also the first that was written. As we shall see, however, several manuscripts certainly written later also avoid the abbreviation of algebraic core terms – even around 1400, authors of general abbacus treatises may have suspected their readers to possess no preliminary knowledge of algebra.

Not only symbolism but also the Maghreb notations for composite fractions are absent from the treatise, even though they turned up in the *Columbia Algorism.* None the less, Jacopo's algebra must be presumed to have its direct roots in the Ibero-Provençal area, with further ancestry in al-Andalus and the Maghreb; there is absolutely no trace of inspiration from Fibonacci nor of direct influence of Arabic classics like al-Khwārizmī or Abū Kāmil. Jacopo offers no geometric proofs but only rules, and the very mixture of commercial and algebraic mathematics is characteristic of the Maghreb–al-Andalus tradition (as also reflected in the *Liber mahameleth*). A particular multiplicative writing for Roman numerals (for example $\overset{\text{m}}{\text{cccc}}$, used as explanation of the Hindu number 400000) *could* also be inspired by the Maghreb algebraic notation (it may also have been an independent invention, Middle Kingdom Egyptian scribes and Diophantos sometimes put the "denomination" above the "coefficient" in a similar way, and there is no reason to believe that this was connected to the Maghreb invention).

In 1328, also in Montpellier, a certain Paolo Gherardi (as Jacopo, unknown apart from the name) wrote a *Libro di ragioni*, known from a later copy (Florence, Bibl. Naz. Centr., Magl. XI, 87, ed. [Arrighi 1987: 13–107]). Its final section is another presentation of algebra.[31] Part of this presentation is so close to Jacopo's algebra that it must descend either from that text (by reduction) or from a close source; but whereas Jacopo only deals (correctly) with 20 (of the possible 22) quadratic, cubic and quartic basic equations ("cases") that can be solved by reduction to quadratic equations or by simple root extraction,[32] Gherardi (omitting all quartics) introduces false rules for the solution of several cubics that cannot be solved in these ways (with examples that are "solved" by means of the false rules). Comparison with later sources show that they are not of his own invention. A couple of the cases he shares with Jacopo also differ from the latter in their choice of examples while corresponding to what can be found in a slightly later Provençal treatise (see imminently).

Gherardi's algebra is almost as rhetorical as Jacopo's, but not fully. Firstly, the abbreviation ℞ is used copiously though not systematically. This *may* be due to the copyist – the effort of Jacopo's and Fibonacci's copyists to conserve the features of the

[31] Beyond Arrighi's complete edition of the treatise [1987: 97–107], there is an edition of the algebra text with translation and mathematical commentary in [Van Egmond 1978].

[32] The lacking equations are the two mixed biquadratics that correspond to al-Khwārizmī's (and Jacopo's) fifth and sixth case. Only the six simple cases (linear and quadratic) are provided with examples – ten in total, half of which are dressed as commercial problems. For the others, only rules are offered.

original was no general rule; but it could also correspond to Gherardi's own text. More important is the reference to a diagram in one example (100 is first divided by some number, next by five more, and the sum of the quotients is given); this diagram is actually missing in the copy, but so clearly described in the text that it can be seen to correspond to the diagram found in a parallel text:[33]

```
100 ╲ ╱ 1 cosa
     ╳
100 ╱ ╲ 1 cosa piu 5
```

The operations performed on the diagram ("cross-multiplication" and the other operations needed to add fractions) are described in a way that implies underlying operations with the "formal fractions" $\frac{100}{1\,\textit{cosa}}$ and $\frac{100}{1\,\textit{cosa piu}\,5}$. No abbreviations being used, we may speak of what goes on as a beginning of symbolic *syntax* without symbolic *vocabulary*.

Such formal fractions, we may observe, constitute an element of "symbolic algebra" that does not presuppose that "*cosa*" itself be replaced by a symbol, but certainly an isolated element only. It must be acknowledged, on the other hand, that this isolated element already made possible calculations that were impossible within a purely rhetorical framework. Jacopo, as already al-Khwārizmī, could get rid of one division by a binomial via multiplication. However, problems of the type where Gherardi and later abbacus algebra use two formal fractions were either solved geometrically by al-Khwārizmī, Abū Kāmil and Fibonacci, as I discuss in a forthcoming paper,[34] or they were replaced before being expressed algebraically *without explanation* by a different problem, namely the one resulting from multiplication by the denominators (al-Khwārizmī, ed. [Hughes 1986: 51]).

A third abbacus book written in Provence (this one in Avignon) is the *Trattato di tutta l'arte dell'abbacho*. As shown by Jean Cassinet [2001], it must be dated to 1334. Cassinet also shows that the traditional ascription to Paolo dell'Abbaco is unfounded.[35] Exactly how much should be counted to the treatise is not clear. The codex Florence, Bibl. Naz. Centr., fond. princ. II.IX.57 (the author's own draft according to [Van Egmond 1980: 140]) contains a part that is not found in the other copies[36] but which is informative about algebra and algebraic notation; however, since this extra part is in the same

33 Florence, Ricc. 2252, see [Van Egmond 1978: 169].

34 " 'Proportions' in the *Liber abbaci*", to appear in the proceedings of the meeting "Proportions: Arts – Architecture – Musique – Mathématiques – Sciences", Centre d'Études Supérieures de la Renaissance, Tours, 30 juin au 4 juillet 2008. ⟦See [Høyrup 2011: 94*f*].⟧

Al-Khwārizmī [ed. Hughes 1986: 255] does not make the geometric argument explicit, but a division by 1 betrays his use of the same diagram as Abū Kāmil [ed. Sesiano 1993: 370].

35 Arguments speaking *against* the ascription are given in [Høyrup 2008: 11 n. 29] ⟦translated as article I.12⟧.

36 I have compared with Rome, Acc. Naz. dei Lincei, Cors. 1875, from c. 1340. For other manuscripts, see [Cassinet 2001] and [Van Egmond 1980, *passim*].

hand as the main treatise [Van Egmond 1980: 140], it is unimportant whether it went into what the author eventually decided to put into the final version.

There is no systematic presentation of algebra nor listing of rules in this part,[37] only a number of problems solved by a rhetorical *censo-cosa* technique.[38] The author uses no abbreviations for *cosa*, *censo* and *radice* – but at one point (fol. 159^{r}) an astonishing notation turns up: $\frac{10}{\textit{cose}}$, meaning “10 *cose*”. The idea is the same as we encountered in the *Columbia Algorism* when it writes $\frac{1}{\textit{gran}}\frac{1}{2}$ meaning “1 *gran* $\frac{1}{2}$”: that what is written below the line is a denomination; indeed, many manuscripts write “il $\frac{1}{3}$” in the sense of “the third” (as ordinal number as well as fraction) – that is, the notation for the fraction was understood as an *image of the spoken form*, not of the division procedure (cf. also the writing of *quinte* as $\frac{e}{5}$ in the *Liber mahameleth*, see note 17).

The compiler of the *Trattato di tutta l’arte* was certainly not the first to use this algebraic notation – who introduces a new notation does not restrict himself to using it a single time in a passage well hidden in an odd corner of a text. He just happens to be our earliest witness of a notation which for long was in the way of the development of one that could serve symbolic calculation.

This compiler was, indeed, not only not the first but also not the last to use this writing of monomials as quasi-fractions. It is used profusely in Dardi of Pisa’s *Aliabraa Argibra* from 1344,[39] better known for being the first Italian-vernacular treatise dedicated exclusively to algebra and for its presentation of rules for solving no less than 194+4

37 The codex contains a list of four rules (fol. 171^{v}), three provided with examples, written on paper from the same years (according to the watermark) but in a different hand than the recto of the sheet and thus apparently added by a user of the manuscript. It contains one of the examples which Gherardi had not borrowed from Jacopo, confirming that his extra examples came from what circulated in the Provençal area. It contains no algebraic abbreviations nor anything else suggesting symbolism.

38 Jean Cassinet [2001: 124–127] gives an almost complete list.

39 See [Van Egmond 1983]. The three principal manuscripts are Vatican, Chigi M.VIII.170 written in Venetian in c. 1395; Siena, Biblioteca Comunale I.VII.17 from c. 1470 [ed. Franci 2001]; and a manuscript from Mantua written in 1429 and actually held by Arizona State University Temple, which I am grateful to know from Van Egmond’s personal transcription. In some of the details, the Arizona manuscript appears to be superior to the others, but at the level of overall structure the Chigi manuscript is demonstrably better – see [Høyrup 2007a: 169*f*]. Considerations of consistency suggests it to be better also in its use of abbreviations and other quasi-symbolism, for which reason I shall build my presentation on this manuscript (cross-checking with the transcription of the Arizona-manuscript – differences on this account are minimal); for references I shall use the original foliation.

A fourth manuscript from c. 1495 (Florence, Bibl. Med.-Laur., Ash. 1199, partial ed. [Libri 1838: III, 349–356]) appears to be very close to the Siena manuscript.

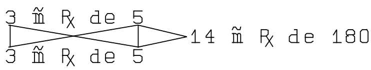
A critical edition of the treatise should be forthcoming from Van Egmond’s hand.

algebraic cases, 194 of which are solved according to generally valid rules (with two slips, explained by Van Egmond [1983: 417]), while the rules for the last four cases are pointed out by Dardi to hold only under particular (unspecified) circumstances.[40]

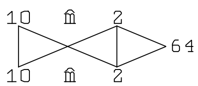
Dardi uses algebraic abbreviations systematically. *Radice* is always ℞, *meno* is $\widehat{m}$, *cosa* is *c*, *censo* is *ç*, *numero/numeri* are *ñũo/ñũi*. *Cubo* is unabridged, *censo de censo* (the fourth power) appears as *ç de ç* (an expanded linguistic form which we may take as an indication that Dardi merely thinks in terms of abbreviation and nothing more). Roots of composite entities are written by a partially rhetorical expression, for instance (fol. 9^{v}) "℞ *de zonto* $\frac{1}{4}$ *cô* ℞ *de* 12" (meaning $\sqrt{\frac{1}{4}+\sqrt{12}}$; *zonto* corresponds to Tuscan *gionto*, "joined", and the whole expression thus means "root of, joined $\frac{1}{4}$ with root of 12").

As just mentioned, Dardi also employs the quasi-fraction notation for monomials, and does so quite systematically in the rules and the examples (but only here).[41] When coefficients are mixed numbers Dardi also uses the formalism systematically in a way which suggests ascending continued fractions, writing for instance $2\frac{1}{2}c$ not quite as $\frac{2\,1}{c\,2}$ but as $\frac{2}{c}\frac{1}{2}$ (which however *could* also mean simply "2 *censi* and $\frac{1}{2}$". Often, a number term is written as a quasi-fraction, for example as $\frac{325}{n}$. How far this notation is from any operative symbolism is revealed by the way multiples of the *censo de censo* are sometimes written – namely for example as $\frac{81}{ç}$ *de ç* (fol. 46^{v}).

None the less, symbolic operations are not absent from Dardi's treatise. They turn up when he teaches the multiplication of binomials (either algebraic or containing numbers and square roots) – for instance, for $(3-\sqrt{5})\cdot(3-\sqrt{5})$,



Noteworthy is also Dardi's use of a similar scheme



as support for his proof of the sign rule "less times less makes plus" on fol. 5^{v}:

> Now I want to demonstrate by number how less times less makes plus, so that every times you have in a construction to multiply less times less you see with certainty that it makes

[40] Dardi reaches this impressive number of resolvable cases by making ample use of radicals. ⟦See article I.12.⟧

[41] This notation appears to be present in the Chigi and Arizona manuscripts only; Franci does not mention it in her edition of the much later Siena manuscript, and composite expressions where their presence might be revealed show no trace of them. They are also absent from Guglielmo Libri's extract of the Florence manuscript.

> plus, of which I shall give you an obvious example. 8 times 8 makes 64, and this 8 is 2 less than 10, and to multiply by the other 8, which is still 2 less than 10, it should similarly make 64. This is the proof. Multiply 10 by 10, it makes 100, and 10 times 2 less makes 20 less, and the other 10 times 2 less makes 40 less, which 40 less detract from 100, and there remains 60. Now it is left for the completion of the multiplication to multiply 2 less times 2 less, it amounts to 4 plus, which 4 plus join above 60, it amounts to 64. And if 2 less times two less had been 4 less, this 4 less should have been detracted from 60, and 56 would remain, and thus it would appear that 10 less 2 times 10 less two had been 56, which is not true. And so also if 2 less times 2 less had been nothing, then the multiplication of 10 less 2 times 10 less 2 would come to be 60, which is still false. Hence less times less by necessity comes to be plus.

Such schemes were no more Dardi's invention than the quasi-fraction notation (even though he may well have been more systematic in the use of both than his precursors). The clearest evidence for this is offered by an anonymous *Trattato dell'alcibra amuchabile* from c. 1365 [ed. Simi 1994], contained in the codex Florence, Ricc. 2263. This is the treatise referred to in note 29, part of which agrees verbatim with Jacopo's algebra. It also has Gherardi's false rules. However, the agreement is not verbatim, showing Gherardi not to be the immediate source (a compiler who follows one source verbatim will not use another one freely) – cf. [Høyrup 2007a: 163].

The treatise consists of several parts. The first presents the arithmetic of monomials and binomials, the second contains rules and examples for 24 algebraic cases (mostly shared with Jacopo or Gherardi), the third a collection of 40 algebraic problems. All are purely rhetorical in formulation, except for using ℞ in the schemes of the first part (see imminently). However, the first and third part contain the same kinds of non-verbal operations as we have encountered in Gherardi and Dardi, and throws more light on the former.

In part 3, there are indeed a number of additions of formal fractions, for example (in problem #13) $\frac{100}{1\,cosa} + \frac{100}{1\,cosa+5}$. This is shown as

$$\frac{100 \qquad\qquad 100}{\textit{per una cosa} \quad \textit{per una cosa e}\ 5}$$

and explained with reference to the parallel $\frac{24}{4}+\frac{24}{6}$ (cross-multiplication of denominators with numerators followed by addition, multiplication of the denominators, etc.). Gherardi's small scheme (see just after note 33) must build on the same insights (whether shared by Gherardi or not).

Part 1 explains the multiplication of binomials with schemes similar to those used by Dardi – for example

```
5 e  piu  ℞ di 20
via
5 e  meno ℞ di 20
```

As we see, the scheme is very similar to those of Dardi but more rudimentary. It also differs from Dardi in its use of the ungrammatical expressions “e più” and “e meno”, where Dardi uses the grammatical “e” for addition and the abbreviation $\widehat{m}$ for subtraction.[42] There is thus no reason to suppose it should be borrowed from Dardi’s earlier treatise – influence from which is on the whole totally absent. Schemes of this kind must hence have been around in the environment or in the source area for early abbacus algebra before 1340, just as the calculation with formal fractions must have been around before 1328, and the quasi-fractions for monomials before 1334.[43] On the whole, this tells us how far the development of algebraic symbolic operations had gone in abbacus algebra in the early 14th century – and that all that was taken over from the Maghreb symbolism was the calculation with formal fractions; a very dubious use of the ascending continued fractions; and possibly the idea of presenting *radice, cosa* and *censo* by single-letter abbreviations (implemented consistently by Dardi but not broadly, and not necessarily a borrowing).

5. The decades around 1400

The Venetian manuscript Vatican, Vat. lat. 10488 (*Alchune ragione*), written in 1424, connects the early phase of abbacus algebra with its own times. The manuscript is written by several hands, but clearly as a single project (hands may change in the middle of a page; we should perhaps think of an abbacus master and his assistants). From fol. 29^v to fol. 32^r it contains a short introduction to algebra, taken from a text written in 1339 by Giovanni di Davizzo, a member of a well-known Florentine abbacist family – see [Ulivi 2002: 39, 197, 200]. At first come sign rules and rules for the multiplication of algebraic powers, next a strange section with rules for the division of algebraic powers where “roots” take the place of negative powers;[44] then a short section about the arithmetic of roots

42 The expression “e meno *n*”, as we remember, corresponds to what was done by al-Karajī, see note 8. The appearance of parallel expression “e più *n*” shows that the attribute “subtractivity” was seen to ask for the existence of a corresponding attribute “addivity” – another instance of “symbolic syntax” without “symbolic vocabulary” (or, in a different terminology but with the same meaning, the incipient shaping of the language of algebra as an *artificial* language).

In the proof that “less times less makes plus” (see above), Dardi speaks of them as “2 meno”/“2 less”, etc., whereas additive numbers are not characterized explicitly as such.

43 This latter presence leads naturally to the question whether the notation in the al-Khwārizmī-redaction from c. 1300 should belong to the same family. This cannot be excluded, but the absence of a fraction line from the notation of the redaction speaks against it. It remains more plausible that the latter notation is either inspired from the Maghreb, or an independent invention.

44 An edition, English translation and analysis of this initial part of the introduction can be found in [Høyrup 2007b: 479–484]. ⟦See also article II.12.⟧

(including binomials containing roots)[45] somehow but indirectly pointing back to al-Karajī: and finally 19 rules for algebraic cases without examples, of which one is false and the rest parallel to those of Jacopo (not borrowed from him but sharing the same source tradition). Everywhere, *radice* is ℞, but “less”, *cosa* and *censo* all appear unabbreviated (*censo* mostly as *zenso*, which cannot have been the Florentine Giovanni’s spelling).

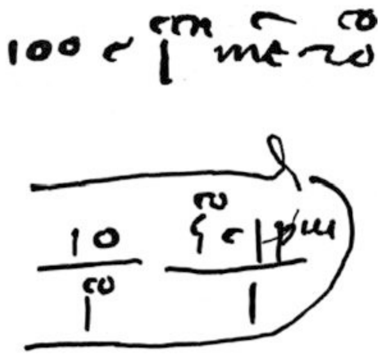


Figure 5. The “equations” from VAT 10488 fol. 37^r (top) and fol. 39^v (bottom)

This introduction comes in the middle of a long section containing number problems mostly solved by means of algebra (many of them about numbers in continued proportion).[46] Here, abbreviations abound. *Radice* is always ℞, *meno* is often ⓜ, m̑ or m̑e (different shapes may occur in the same line). More interesting, however, is the frequent use of *co*, ☐ (occasionally *ce*, both for *census*) and n^o written above the coefficient, precisely as in the Maghreb notation (and quite likely inspired from it). However, these notations are not used systematically, and only used once for formal calculation, namely in a marginal “equation” without equation sign[47] on fol. 39^v – see Figure 5, bottom.[48] In another place (fol. 37^r, Figure 5 top) the running text formulates a genuine polynomial, but this is merely an abbreviation for *100 e* 1 *censo meno 20 cose*. It serves within the rhetorical argument without being operated upon.

Later in the text comes another extensive collection of problems solved by means of algebra (some of them number problems, others dressed as business problems), and inside it another collection of rules for algebraic cases (17 in total, only 2 overlapping

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45 Translation in [Høyrup 2009: 56*f*].

46 Even these are borrowed en bloc, as revealed by a commentary within the running text on fol. 36^r, where the compiler tells how a certain problem should be made *al parere mio*, “in my opinion”. The several hands of the manuscripts are thus not professional scribes copying without following the argument.

47 Two formal fractions are indicated to be equal; the hand seems to be the same as that of the main text and of marginal notes adding words that were omitted during copying.

48 The treatment of the problem is quite interesting. The problem asks for a number which, when divided into 10 yields 5 times the same number and 1 more. Instead of writing “$\frac{10}{\underset{1}{co}} = \overset{co}{5}$ e 1 piu” it expresses the right-hand side as a fraction $\frac{\overset{co}{5}\ e\ 1\,piu}{1}$, thus opening the way to the usual cross-multiplication.

the first collection). In its use of abbreviations, this second cluster of problems and rules is quite similar to the first cluster, the only exception being a problem (fols. 95^{r}–96^{v}) where the use of coefficients with superscript power is so dense that it may possibly have facilitated understanding of the argument by making most of the multiples of *cosa* and *censo* stand out visually.

In the whole manuscript, addition is normally indicated by a simple *e*, “and”. I have located three occurrences of *più*,[49] none of them abbreviated. The expressions *e più* and *e meno* appear to be wholly absent.

It is fairly obvious that this casual use of what could be a symbolism was not invented by the compilers of the manuscript, and certainly not something they were experimenting with. They used for convenience something which was familiar, without probing its possibilities. If anybody else in the abbacus environment had used the notation as a symbolism and not merely as a set of abbreviations (and the single case of an equation between formal fractions suggests that this may well have been the case), then the compilers of the present manuscript have not really discovered – or they reveal, which would be more significant, that the contents of abbacus algebra did not call for and justify the effort needed to implement a symbolism to which its practitioners were not accus–tomed.[50] They might *almost* as well have used Dardi’s quasi-fractions – only in the equation between formal fractions would the left-hand side have collided with it by meaning simply “10 *cose*”.

Though not really using the notation as a symbolism, the compilers of Vat. lat. 10488 at least show that they knew it. However, this should not make us believe that every abbacus algebra from the same period was familiar with the notation, or at least not that everybody adopted it. As an example we may look at two closely related manuscripts coming from Bologna, one (Palermo, Biblioteca Comunale 2 Qq E 13, *Libro merchatantesche*) written in 1398, the other (Vatican, Vat. lat. 4825, Tomaso de Jachomo Lione, *Libro da razioni*) in 1429.[51] They both contain a list of 27 algebraic cases with examples followed by a brief section about the arithmetic of roots (definition, multiplica-tion, division, addition and subtraction). The former has a very fanciful abbreviation for *meno*, namely , which corresponds, however, to the way *che* and various other non–algebraic words are abbreviated, and is thus merely a personal style of the scribe; the other writes *meno* in full, and none of the two manuscripts have any other abbreviation whatsoever of algebraic terms – not even ℞ for *radice* which they are likely to have

[49] In a marginal scheme and the running text of a problem about combined works (fol. 90^{r}), and once in an algebra problem (fol. 94^{r}). There may be more instances, but they will be rare.

[50] The latter proviso is needed. For us, accustomed as we are to symbolic algebra, it is often much easier to follow a complex abbacus texts if we make symbolic notes on a sheet of paper.

[51] More precisely, 7 March 1429 – which with year change at Easter means 1430 according to our calendar, the date given in [Van Egmond 1980: 223].

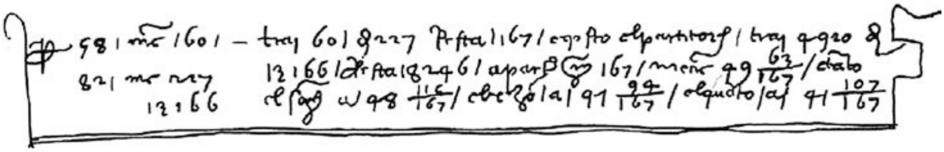

Figure 6. Non-algebraic scheme from Palermo, Biblioteca Comunale 2 Qq E 13, fol. 38^{v}.

known, which suggests but does not prove that the other abbreviations were also avoided consciously.

Maybe we should not be surprised not to find any daring development in these two manuscripts. In general, they offer no evidence of deep mathematical insight. In this perspective, the manuscript Florence, Bibl. Naz. Centr., fondo princ. II.V.152 (*Tratato sopra l'arte della arismetricha*) is more illuminating. Its algebraic section was edited by Franci and Pancanti [1988].[52] It was written in Florence in c. 1390, and offers a clear discussion of the sequence of algebraic powers as a geometric progression as well as sophisticated use of polynomial algebra in the transformation of equation types – see [Høyrup 2008: 30–34] ⟦translated as article I.12.⟧.

In the running text, there are no abbreviations nor anything else which foreshadows symbolism. However, inserted to the left we find a number of schemes explained by the text and showing multiplication of polynomials with two or three terms (numbers, roots and/or algebraic powers), of which Figure 7 shows some examples – four as rendered by Franci and Pancanti, the last of these also as appearing in the manuscript (redrawn for clarity).

Those involving only binomials are easily seen to be related to what we find in the *Trattato dell'alcibra amuchabile* and in Dardi's *Aliabraa Argibra* – but also to schemes used in non-algebraic sections of other treatises, for instance the Palermo-treatise discussed above, see Figure 6, which should warn us against seeing any direct connection.

The schemes for the multiplication of three-term polynomials are different. They emulate the scheme for multiplying multi-digit numbers, and the text itself justly refers to multiplication *a chasella* [ed. Franci and Pancanti 1988: 9]. The *a casella* version of the algorithm uses vertical columns, while the scheme for multiplying polynomials used in the Jerba manuscript [ed. Abdeljaouad 2002: 47] follows the older algorithm *a scacchiera* with slanted columns; none the less inspiration from the Maghreb is plausible, in particular because another odd feature of the manuscript suggests a pipeline to the Arabic world. In a wage problem, an unknown amount of money is posited to be a *censo*,

[52] I have controlled on a scan of a microfilm, but since it is almost illegible my principal basis for discussing the treatise is this edition.

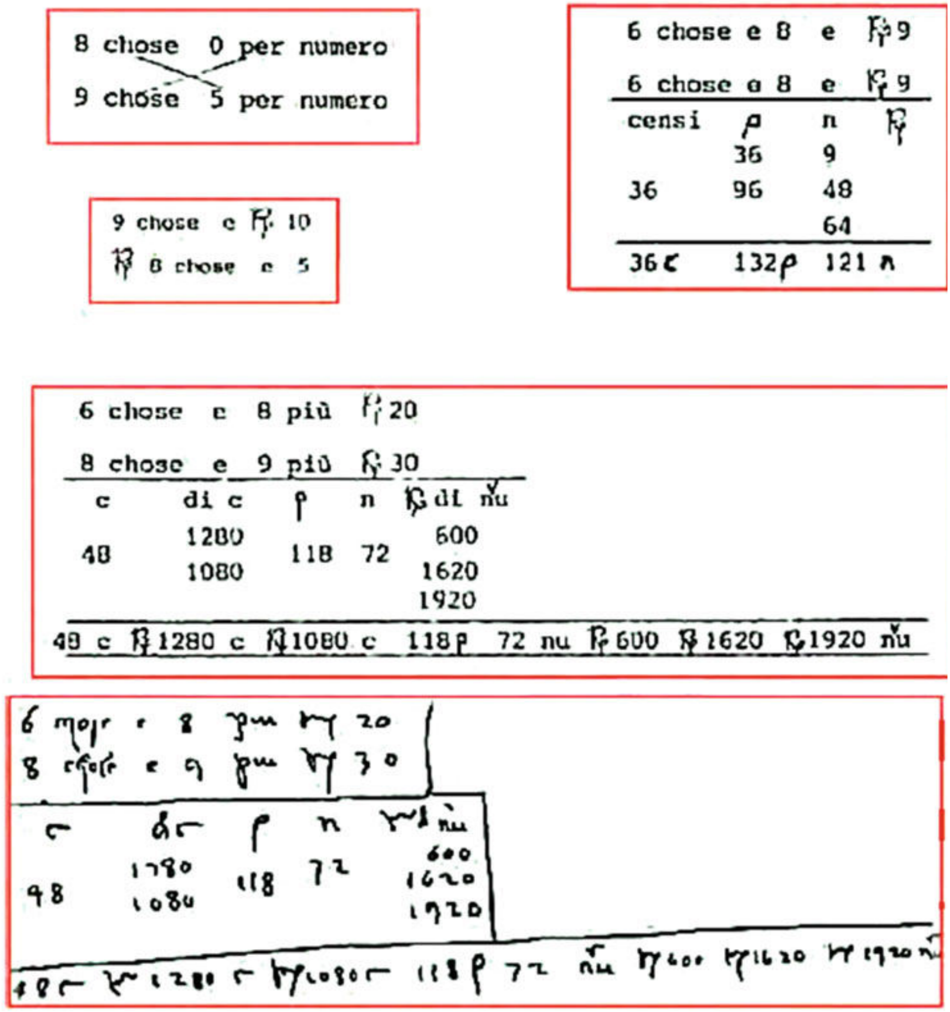

Figure 7. Schemes for the multiplication of polynomials, from [Franci and Pancanti 1988: 8–12], and from the manuscript, fol. 146ᵛ.

whereas Biagio *il vecchio* [ed. Pieraccini 1983: 89*f*] posits it to be a *cosa* in the same problem in a treatise written at least 50 years earlier. But the present author does not understand that a *censo* can be an amount of money, and therefore feels obliged to find its square root – only to square it again to find the solution. He thus uses the terminology without understanding it, and therefore cannot have shaped the solution himself; nor can the source be anything of what we have discussed so far.

Schemes of this kind (and other schemes for calculating with polynomials) turn up not only in later abbacus writings (for instance, in Raffaello Canacci, see below) but also in Stifel's *Arithmetica integra* [1544: fols. 3^{v}*ff*], in Jacques Peletier's *L'Algèbre* [1554: 15–22] and in Petrus Ramus's *Algebra* [1560: fol. A iii^{r}].

Returning to the schemes of the present treatise we observe that the *cosa* is represented (within the calculations, not in the statement lines) by a symbol looking like ρ, and the *censo* by *c*. *Radice* is ℞ in statement as well as calculation. The writing of *meno* is not quite systematic – whether it is written in full, abbreviated *me* ⌒ or as ⓜ rendered "m." in the edition) seems mostly to depend on the space available in the line. Addition may be *e* or *più* (*più* being mostly but not always nor exclusively used before ℞); when space is insufficient, and only then, *più* may be abbreviated *p*.[53] All in all, the writer can be seen to have taken advantage of this incipient symbolism but not to have felt any need to use it systematically – it stays on the watershed, between facultative abbreviation and symbolic notation.

6. The mid-15th-century abbacus encyclopediae

Around 1460, three extensive "abbacus encyclopediae" were written in Florence. Most famous among these is, and was, Benedetto da Firenze's *Trattato de praticha d'arismetrica* – it is the only one of them which is known from several manuscripts.[54]

Earliest of these is Siena, Biblioteca Comunale degli Intronati, L.IV.21, which I have used together with the editions of some of its books.[55] According to the colophon (fol. 1^{r}) it was "conpilato da B. a uno suo charo amicho negl'anni di Christo MCCCCLXIII". It consists of 495 folios, 106 of which deal with algebra.

The algebra part consists of the following books:

- XIII, Benedetto's own introduction to the field, starting with a 23-lines' excerpt from Guglielmo de Lunis's lost translation of al-Khwārizmī (cf. note 21). Then follows a presentation of the six fundamental cases with geometric proofs, built on al-Khwārizmī; a second chapter on the multiplication and division of algebraic powers (*nomi*, "names") and the multiplication of binomials; and a third chapter containing rules and examples for 36 cases (none of them false);

[53] The phrases *e più* and *e meno* occur each around half a dozen times, but apparently in a processual meaning, "and (then) added" respectively "and (then) subtracted". Nothing suggest a use of *più* and *meno* as attributes of numbers, even though the author does operate with negative (not merely subtractive) numbers in his transformation of cubic equations – see [Høyrup 2008: 33].

[54] On Benedetto and his historical setting, see the exhaustive study in [Ulivi 2002].

[55] [Salomone 1982]; [Pieraccini 1983]; [Pancanti 1982]; [Arrighi 1967]. All of these editions were made from the same Siena manuscript, which is also described in detail with extensive extracts in [Arrighi 2004/1965]. ⟦At closer inspection, this manuscript turns out to be Benedetto's autograph, see article I.12.⟧

– XIV, a problem collection going back to Biagio *il vecchio* († c. 1340 according to Benedetto);
– XV, containing a translation of the algebra chapter from the *Liber abbaci*, provided with “some clarifications, specification of the rules in relation to the cases presented in book XIII, and the completion of calculations, which the ancient master had often neglected, indicating only the result” [Franci and Toti Rigatelli 1983: 309]; a problem collection going back to Giovanni di Bartolo (fl. 1390–1430, a disciple of Antonio de’ Mazzinghi); and Antonio de’ Mazzinghi’s *Fioretti* from 1373 or earlier [Ulivi 1998: 122].

The basic question regarding this manuscript is to which extent we can rely on Benedetto as a faithful witness of the notations and possible symbolism of the earlier authors he cites. A secondary problem is whether we should ascribe to Benedetto himself or to a later user a number of marginal quasi-symbolic calculations.

Regarding the first problem we may observe that there are no abbreviations or any other hints of incipient symbolism in the chapters borrowed from Fibonacci and al-Khwārizmī. This suggests that Benedetto is a fairly faithful witness, at least as far as the presence or absence of such things is concerned. On the other hand it is striking that the symbols he uses are the same throughout;[56] this could mean that he employed is own notation when rendering the notations of others, but could also be explained by the fact that all the abbacists he cites from Biagio onward belong to his own school tradition – as observed by Raffaella Franci and Laura Toti Rigatelli [1983: 307] the *Trattato* is not without “a certain parochialism”.

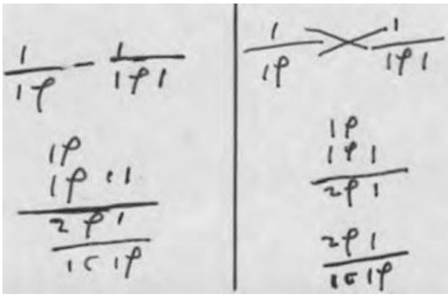

Figure 8. A marginal calculation accompanying the same problem from Antonio’s *Fioretti* in Siena L.IV.21, fol. 456r and Ottobon. lat. 3307, fol. 338v.

Marginal calculations along borrowed problems can obviously not be supposed a priori to be borrowed, and not even to have been written by the compiler. However, the marginal calculations in the algebraic chapters appear to be made in the same hand as marginal calculations and diagrams for which partial space is made in indentions in book XIII, chapter 2 as well as in earlier books of the treatise. Often, the irregular shape of the insertions even shows these earlier calculations and diagrams to have been written before

[56] One partial exception to this rule is pointed out below, note 59.

the main text, cf. fol. 263^{v} as shown in Figure 9.[57] This order of writing shows that the manuscript is Benedetto's original, and that he worked out the calculations while making it – in particular because the marginal calculations are never indented in the algebra chapters copied from earlier authors.

Comparison (Figure 8) of the marginal calculations accompanying a problem in the excerpt from Antonio's *Fioretti* and the same problem as contained in the manuscript Vatican, Ottobon. lat. 3307 from c. 1465 (on which below) shows astonishing agreement, proving that these calculations were neither made by a later user nor invented by Benedetto and the compiler of the Vatican manuscript. In principle, the calculations in the two manuscripts *could* have been added in a manuscript drawn from the *Fioretti* that had been written after Antonio's time and on which both encyclopedias build; given that the encyclopedias do not contain the same selection it seems reasonable, however, to assume that they reflect Antonio's own style – not least, as we shall see, because we are not far from what can be found in the equally Florentine *Tratato sopra l'arte della arismetricha* c. 1390, discussed on p. 842.

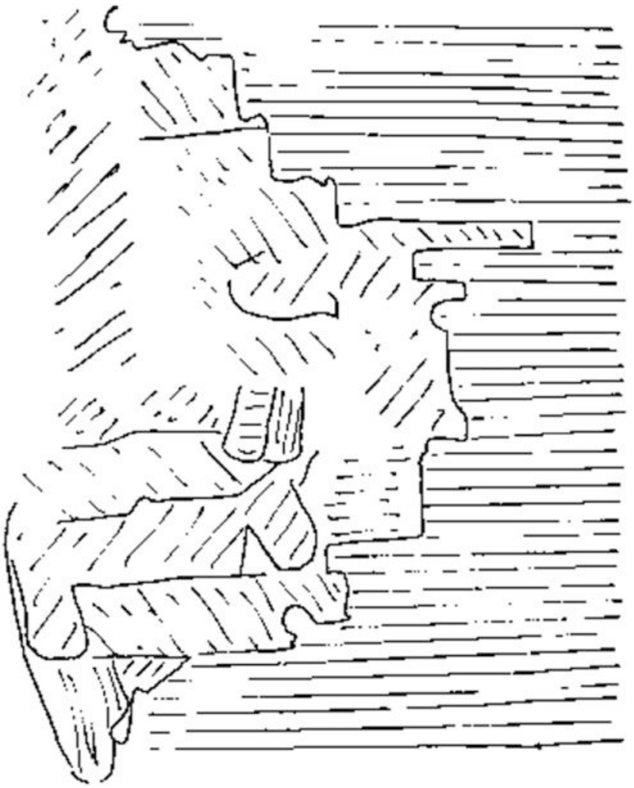

Figure 9. The structure of Siena, L.IV.21, fol. 263^{v}. To the right, the orderly lines of the text proper. Left a variety of numerical calculations, separated by Benedetto by means of curved lines drawn ad hoc. Different hatchings indicate distinct calculations

57 Figure 10 shows a particularly striking case, and contains calculations for a very complicated problem dealing with two unknowns, a *borsa*, "[the unknown contents of] a purse", and a *quantità*, the share received by the first of those who divide its contents.

What Benedetto does when he approaches symbolism can be summed up as follows: He uses ρ (often a shape more or less like ℘) and (much less often) *c* and *c*° for *cosa* respectively *censo* (and their plurals), but almost exclusively within formal fractions.[58] Even in formal fractions, *censo* may also be written in full. *Meno* is mostly abbreviated $\widehat{me}$ in formal fractions.[59] Radice may be abbreviated ℞ in the running text, but often, and without system, it is left unabridged; within formal fractions, where there is little space for the usual abbreviation, it may become *r* or *ra.* Both when written in full and when appearing as ℞, it may be encircled if it is to be taken of a composite expression. In later times (e.g., in Pacioli's *Summa*, see below) this root was to be called *radice legata* or *radice universale;* the use of the circle to indicate it goes back at least to Gilio of Siena's *Questioni d'algebra* from 1384 [Franci 1983: xxiii], and presumably to Antonio, since Gilio's is likely to have been taught by him or at least to have known his works well (*ibid.* pp. iv*f*). The concept itself, we remember, was expressed by Dardi as "℞ *de zonto … con* …", close in meaning to *radice legata.*

All of this suggests that the "symbolism" is only a set of facultative abbreviations, and not really an incipient symbolism. However, in a number of marginal calculations it does serve as carrier of the reasoning. One example was shown in Figure 8, another one (fol. 455^{r}, see Figure 10) performs a multiplication which, in slightly mixed notation, looks as follows:

$$(1\rho\ \widehat{me}\ \text{℞}[13\tfrac{1}{2}\ \widehat{me}\ 1\ c])\times(1\rho\ \text{p[iù]}\ \text{℞}[13\tfrac{1}{2}\ \widehat{me}\ 1\ c])$$

Formal fractions *without* abbreviation are used in the presentation of the arithmetic of algebraic powers in Book XIII (fols. 372^{r}–373^{r}). At first in this piece of text we find

> Partendo chose per censi ne viene rotto nominato da chose chome partendo 48 chose per 8 censi ne viene $\frac{6}{1\,\mathit{chosa}}$.

in translation

> Dividing *things* by *censi* results in a fraction denominated by *things*, as dividing 48 *things* by 8 *censi* results in $\frac{6}{1\,\mathit{chosa}}$.

---

[58] Outside such fractions, I have noticed ρ three times in the main text of the *Fioretti, viz* on fols. 453^{r}, 469^{r} and 469^{v} (of which the first occurrence seems to be explained by an initial omission of the word *chosa* leaving hardly space for the abbreviation), and c° once, on fol. 458^{r}. Arrighi [1967: 22] claims another c° on fol. 453^{r}, but the manuscript writes *chosa* in the corresponding place.

[59] Additively composite symbolic expressions are mostly constructed by juxtaposition (in running text as well as marginal computations); in rhetorical exposition, *e* or (when a root and a number are added) an unabbreviated *più* is used. A few marginal diagrams in the section copied from Bartolo mark additive contributions to a sum by *p*, and all subtractive contributions by *m.*

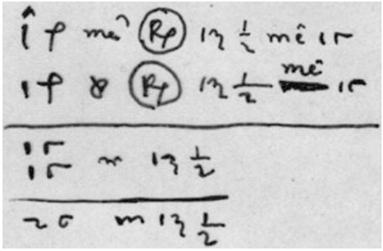

Figure 10. The multiplication of $(1\rho-\surd[131\,{}^{1}/_{2}-1c])$ by $(1\rho-\surd[13\,{}^{1}/_{2}-1c])$

Afterwards we find denominators "1 *censo*", "1 *cubo*", "1 *cubo di censo*", etc. When addition of such expressions and the division by a binomial are taught, we also find denominators like "3 *cubi* and 2 *cose*".[60]

Long before we come to the algebra, namely on fols. 259[v]–260[v], there is an interesting occurrence of formal fractions in problems of combined works, involving not a *cosa* or a *censo* but a *quantità* – *such as* $\frac{8}{1\ \mathit{quantit\acute{a}}}$ and $\frac{1\ \mathit{quantit\acute{a}\ meno}\ 8}{1\ \mathit{quantit\acute{a}}}$.[61] These fractions are written without any abbreviation.[62] Together with the explanation of the division of algebraic powers they demonstrate (as we already saw it in the *Trattato dell'alcibra amuchabile*) that the use of and the argumentation based on formal fractions do not depend on the presence of standard abbreviations for the unknown (even though calculations involving products of unknown quantities become heavy without standard abbreviations).

The manuscript Vatican, Ottobon. lat. 3307, was already mentioned above.[63] It was also written in Florence, dates from c. 1465, and is also encyclopedic in character but somewhat less extensive than Benedetto's *Trattato de pratícha d'arismetrica*, of which it is probably independent in substance.[64] It presents itself (fol. 1[r]) as *Libro di praticha*

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60 This whole section looks as if it was inspired by al-Karajī or the tradition he inaugurated; but more or less independent invention is not to be excluded: once the notation for fractions is combined with interest in the arithmetic of algebraic monomials and binomials things should go by themselves.

61 Benedetto would probably see these solutions not as applications of algebra but of the *regula recta* – which he speaks of as *modo retto/repto/recto* in the *Tractato d'abbaco*, ed. [Arrighi 1974: 153, 168, 181], everywhere using *quantità* for the unknown.

62 However, in the slightly later problem about a *borsa* and a *quantità* mentioned in note 57, these are abbreviated ⟦*b* and *q*⟧ in the marginal computations – perhaps not only in order to save space (already a valid consideration given how full the page is) but also because it makes it easier to schematize the calculations.

63 Description with extracts in [Arrighi 2004/1968].

64 The *idea* of producing an encyclopedic presentation of abbacus mathematics may of course have been inspired by Benedetto's *Trattato* from 1463 – unless the inspiration goes the other way, the dating "c. 1465" is based on watermarks [Van Egmond 1980: 213] and is therefore only approximate. If the present compiler had emulated Benedetto, one might perhaps expect that he would have indicated it in a heading, as does Benedetto when bringing a whole sequence of problems borrowed from Antonio. In consequence, I tend to suspect that the Ottoboniano manuscript precedes Benedetto's *Trattato.*

d'arismetrica, cioè fioretti tracti di più libri facti da Lionardo pisano – which is to be taken *cum grano salis*, Fibonacci is certainly not the main source.

Judged as a mathematician (and as a Humanist digging in his historical tradition), the present compiler does not reach Benedetto's level. However, from our present point of view he is very similar, and the manuscript even presents us with a couple of innovations (which are certainly not of the compiler's own invention).

Even in this text, margin calculations are often indented into the text in a way that shows them to have been written first, indicating that it is the compiler's autograph.[65] Already in an intricate problem about combined works (not the same as Benedetto's, but closely related) use is made of formal fractions involving an unknown (unabbreviated) *quantità*. Now, even the square of the *quantità* turns up, as *quantità di quantità*.

When presenting the quotients between powers, the compiler writes the names of powers in full within the formal fractions, just as done by Benedetto. The details of the exposition show beyond doubt, however, that the compiler does not copy Benedetto but that both draw on a common background. In the present treatise, the first fractional power is introduced like this (fol. 304^{v}):

> Partendo dramme per chose ne viene un rocto denominato da chose, chome partendo 48 dramme per 6 chose ne viene questo rotto cioè $\frac{48\,\mathit{dramme}}{1\,\mathit{chosa}}$.

The second example makes the same numerical error. From the third example onward, it has disappeared. The fourth one looks as follows (fol. 305^{r}):

> Partendo chose per chubi ne viene rotto nominato da chubi, come partendo 48 chose per 6 chubi, ne viene questo rotto, cioè $\frac{8\,\mathit{chosa}}{1\,\mathit{chubo}}$.

Only afterwards is the reduction of the ratio between powers (*schifare*) introduced, for instance, that $\frac{8\,\mathit{chose}}{1\,\mathit{chubo}}$ is $\frac{8\,\mathit{dramme}}{1\,\mathit{censo}}$.

Abbreviations for the powers are absent not only from this discussion but also from the presentation of the rules. When we come to the examples, however, marginal calculations with binomials expressed by means of abbreviations abound. That for *cosa* changes between ρ and , that for *censo* between *c* (written) and σ (actually); in both cases the difference is simply the length of the initial stroke; since all intermediate shapes are present, a single grapheme is certainly meant for all abbreviations for *cosa* and similarly for *censo*. c° appears to be absent. In the marginal computations, *più* may

[65] This happens seven times from fol. 48^{v} to fol. 54^{v}. On fols. 176^{v} and 211^{v} there are empty indentions, but these are quite different in character, wedge-shaped and made in the beginning of problems, and thus expressions of visual artistry and not evidence that the earlier indentions were made as empty space while the text was written and then filled out afterwards by the compiler or a user.

appear as *p*, whereas *meno* may be may be *m* or *mê*.[66] However, addition may also be indicated by mere juxtaposition. The marginal calculations mostly have the same character as those of Benedetto, cf. Figure 8. In the running text abbreviations are reserved for formal fractions; otherwise they are as absent as from Benedetto's *Trattato*.

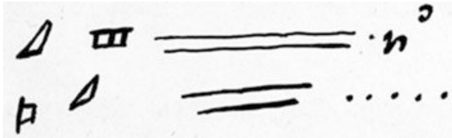

Figure 11. The marginal note from Ottobon. lat. 3307 fol. 309r

On two points the present manuscript goes slightly beyond Benedetto. Alongside a passage in the main text which introduces cases involving *cubi* and *censi di censi* (fol. 309r), the margin contains the note shown in Figure 11. *n*° being *numero* and the superscript square being known (for instance from Vat. lat. 10488, cf. above) to be a possible representative for *censo*, it is a reasonable assumption (which we shall find fully confirmed below) that the triangle stands for the cube and the double square for *censo di censi*, the whole diagram thus being a pointer to the equation types "*cubi* and *censi di censi* equal number" and "*censi* and *cubi* equal number". We observe that equality is indicated by a double line.[67] As we shall see imminently, the compiler and several other 15th-century writers indicate equality by a single line. This, as well as the deviating symbols for the powers, suggests that this particular note was made by a later user of the manuscript.

The other innovation can be safely ascribed to the hand of the compiler if not (inasfar as innovation) to his mind. It is a marginal calculation found on fol. 331v, alongside a problem $\frac{100}{1\rho}+\frac{100}{1\rho+7}=40$ (these formal fractions, without + and =, stand in the text). The solution follows from a transformation $\frac{100\rho+100\cdot(\rho+7)}{(1\rho)\cdot(1\rho+7)}=\frac{100\rho+(100\rho+700)}{(1\sigma+7\rho)}=40$; whence 200ρ+700ρ = 40σ+280ρ; in the margin, the same solution is given schematically:

$$\begin{array}{llll} 100\rho & & & \\ \underline{100\rho} & \underline{700} & & \\ \underline{200\rho} & \underline{700} & & \\ 1\sigma & \underline{7\rho\qquad} & & 40 \\ & & & \\ 200\rho & 700 & \text{——} & 40\sigma\ \langle 280\rho\rangle \end{array}$$

(the omitted ⟨280ρ⟩ in the last line is present within the main text). The strokes before

66 Both *m* and mê appear in the same calculation on fol. 312v – by the way together with *p*.

67 The double line is also used for equality in a Bologna manuscript from the mid-16th century reproduced in [Cajori 1928: I, 129]; whether Recorde's introduction of the same symbol in 1557 was independent of this little known Italian tradition is difficult to decide. In any case, the combination with the geometric symbols indicates that the present example (and thus the Italian tradition) predates Recorde.

40 and 40σ appear to be meant as equation signs. It might be better, however, to understand them as all-purpose "confrontation signs" – in the margin of fol. 338^{r}, ———— means that one commercial partner has $\frac{3000}{1\rho\,5000}$, the other $\frac{4000}{1\rho\,6000}$ (see Figure 12).[68]

This is one of Antonio's problems. In Benedetto's manuscript, we find the same problem and the same diagram on fol. 456^{r} – with the only difference that the line is replaced by an X indicating the cross-multiplication that is to be performed. The "confrontation line" is thus not part of the inheritance from Antonio (nor, in general, from the inheritance shared with Benedetto). Though hardly due to the present compiler, it *is* an innovation.

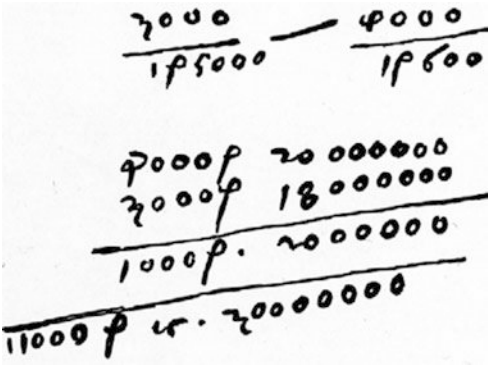

Figure 12. The confrontation sign of Ottobon. lat. 3307 fol. 338^{r}

The reason to doubt the innovative role of our compiler is one of Regiomontanus's notes for the Bianchini correspondence from c. 1460 [ed. Curtze 1902: 278]. For the problem $\frac{100}{1\rho}+\frac{100}{1\rho+8}=40$, he uses exactly the same scheme, including the "confrontation line";

$$\frac{100}{1\ \rho} \qquad \frac{100}{1\ \rho\ et\ 8}$$

```
        100 ρ et 800
        100 ρ
        ───────────
        200 ρ et 800
        ───────────
         1 ρ et 8 σ   ── 40
40 σ et 320 ρ ── 200 ρ et 800
40 σ et 120 ρ ── 800
 1 σ et   3 ρ ── 20
```

(Regiomontanus extends the initial stroke of ρ even more than our compiler, to ; his variant of σ, *census*, is , possibly a different extension of c[69]).

68 As we shall see, Raffaello Canacci also uses the line both for equality and for confrontation; so does Widmann [1489: fols. 12^{r}, 21^{r-v}, 23^{r}, 27^{r}, 38^{v}] when confronting the numbers 9 and 7 with the schemes for casting out nines and sevens.

69 Curtze does not show these shapes in his edition, but they are rendered in [Cajori 1928: I, 95].

A third Florentine encyclopedic abbacus treatise is Florence, Bibl. Naz. Centr., Palat. 573.[70] Van Egmond [1980: 124] dates it to c. 1460 on the basis of dates contained in problems. However, the compiler refers (fol. 1^r) to Benedetto's *Trattato* (from 1463) as having been made "already some time ago" (*già è più tenpo*), for which reason a date around 1470 seems more plausible. This is confirmed by the watermarks referred to by Van Egmond – even this manuscript can be seen from marginal calculations made before the writing of the main text to be the compiler's original, whose date must therefore fit the watermarks.

As regards algebraic notations and incipient symbolism, this treatise teaches us nothing new. It does not copy Benedetto (in the passages I checked) but does not go beyond him in any respect; it uses the same abbreviations for algebraic powers, in marginal calculations and (sparingly) in formal fractions within the main text – including the encircled *radice* and ℞. In the chapter copying Fibonacci's algebra it has no marginal calculations (only indications of forgotten words), which confirms that the compilers of the three encyclopedic treatises copied the marginal calculations and did not add on their own when copying – at least not when copying venerated predecessors identified by name.

7. Late 15th-century Italy

The three encyclopediae confirm that no systematic effort to develop notations or to extend the range of symbolic calculation characterizes the mid-century Italian abbacus environment – not even among those masters who, like Benedetto and the compiler of Palatino 573, reveal scholarly and Humanist ambitions by including such matters as the Boethian names for ratios in their treatises and by basing their introduction of algebra on its oldest author (al-Khwārizmī).[71] The experiments and innovations of the 14th century – mostly, so it seems, vague reflections of Maghreb practices – had not been developed further.[72] In that respect, their attitude is not too far from that of 15th-century mainstream Humanism.

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70 Described with sometimes extensive extracts from the beginnings of all chapters in [Arrighi 2004/1967].

71 Benedetto [ed. Salomone 1982: 20] gives this argument explicitly; the compiler of Palatino 573 speaks of his wish that "the work of Maumetto the Arab which has been almost lost be renovated".

72 It is true that we have not seen the quotients between powers expressed as formal fractions in earlier manuscripts; however, the way they turn up independently in all three encyclopaediae shows that they were already part of the heritage – *perhaps* from Antonio. The interest in such quotients is already documented in Giovanni di Davizzo in 1339, who however makes the unlucky choice to identify negative powers with roots – see [Høyrup 2007c: 478–484] (and cf. above, before note 44).

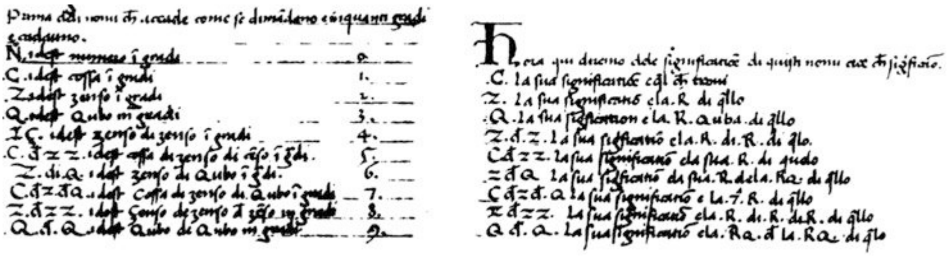

Figure 13. The two presentations of the algebraic powers in Bibl. Estense, ital. 578.

Towards the end of the century we have evidence of more conscious exploration of the potentialities of symbolic notations. A first manuscript to be mentioned here is Modena, Bibl. Estense, ital. 578 from c. 1485 (according to the orthography written in northern Italy – e.g., *zonzi* and *mazore* where Tuscan normal orthography would have *giongi* and *magiore*).[73] It contains (fols. 5^r–20^r) an algebra, starting with a presentation of symbols for the powers with a double explanation, first with symbols and corresponding “degrees”, *gradi* (fol. 5^r), next by symbols and signification (fol. 5^v) – see Figure 13.

As we see, the symbol for the *cosa* is the habitual *c*. For the *censo, z* is used, in agreement with the usual northern orthography *zenso* – however, in a writing which is quite different from the *z* used in full writing of *zenso* (z respectively ʒ, see also Figure 14); the *cubo* is *Q*, the fourth power is *z di z*. The fifth power is *c di zz*, obviously meant as a multiplicative composition (as the traditional *cubo di censo*), the sixth instead *z di Q*, that is, composed by embedding. The seventh degree is *c di z di Q*, mixing the two principles, the eight again made with embedding as z *di zz*. So is the ninth, *QQ*.

Then follow the significations. *c* is “that which you find”, *z* “the root of that”, *Q* “the cube root of that”, and *z di z* “the root of the root of that”. Already now we may wonder – why “roots”? I have no answer, but discuss possible hints in [Høyrup 2008: 31] ⟦translated as article I.12⟧, in connection with the *Tratato sopra l’arte della arismetricha* (above, p. 842), from where these “root-names” are known for the first time.[74] It is reasonable to assume a connection – this *Tratato* has the same mixture of multiplicative and embed-

73 [Van Egmond 1986] is an edition of the manuscript. It has some discussion of its symbolism but does not go into details regarding the written shapes, for which reason I base my discussion on the manuscript.

74 Van Egmond [1986: 21] “explains” them

$$\mathrm{Z} = \mathrm{R} \qquad x^2 = \mathrm{n} \mathrel{->} \mathrm{x} = \sqrt{\mathrm{n}}\,,$$

etc., which however is contradicted by the words of the text.

ding-based formation of the names for powers, though calling the fifth degree *cubo di censo*, and the sixth (like here) *censo di cubo*.[75]

The root names go on with "root of this" for the fifth power – which is probably meant as "5th root of this", since the seventh power is "the 7th root of this". The names for the sixth, eighth and ninth degree are made by embedding.

Figure 14. Three graphemes from Bibl. Estense, ital. 578. Left, *z* abbreviating *zenso* in the initial overview; centre, *z* as written as part of the running text; right, the digit 3.

After explaining algebraic operations and the arithmetic of monomials and binomials the manuscript offers a list of algebraic cases followed by examples illustrating them. Here the same symbols are used within the text (there are no marginal calculations) – with one exception, instead of *z* a sign is used which is a transformed version of Dardi's *ç* – ʓ, with variations that sometimes make it look like a *z* provided with an initial and a final curlicue.[76]

The problems are grouped in *capitoli* asking for the same procedure in spite of involving different powers – chapter 14, for instance, combines "*zz* and *z di zz* equal to n°" and "*c di zz* and *QQ* equal to *c*". The orderly presentation of the powers in a scheme and the concept of numerical *gradi*, "degrees" (our exponents) has facilitated this further ordering. This is clear from the presentation – in chapter 14, "When you find three names of which one is 4 degrees more than the other …". Beyond this, the abbreviations seem to serve as nothing but abbreviations, though used consistently.

Raffaello Canacci's use of schemes for the calculation with polynomials (including multiplication a *casella*) in the *Ragionamenti d'algebra*[77] from c. 1495 [ed. Procissi

[75] This difference may tell us something about the spontaneous psychology of embedding: it seems to be easier to embed within a single than within a repeated multiplication – that is, to grasp *censo* of P as $(P)^2$ than to understand *cubo* of R as $(R)^3$.

[76] There are a few slips. In the initial list, *zenso di zenso* is written z ʓ instead of zz and within the list of cases and the examples a few instances of *zenso* abbreviated *z* (written ʒ, not z) occur. Van Egmond [1986: 23] reads these as "3", and takes this as evidence that the manuscript was made by a copyist who did not really understand but had a tendency to replace a *z* used in the original by 3. However, even though the writings of *z* and 3 are similar, the magnification allowed by a scanned microfilm shows them to be different, and that the copyist did *not* write 3 where he should have written *z* (see Figure 14). Other errors pointed out by Van Egmond show beyond doubt that the beautifully written manuscript *is* a copy. However, the almost systematic distinction between the abbreviations z and ʓ, as well as the general idea of applying stylized shapes of letters when used as symbols, is likely to reflect the ways of the original – an unskilled copyist would hardly introduce them.

[77] Florence, Bibl. Naz. Centr., Palat. 567. I have not seen the manuscript but only Angiolo Procissi's diplomatic transcriptions.

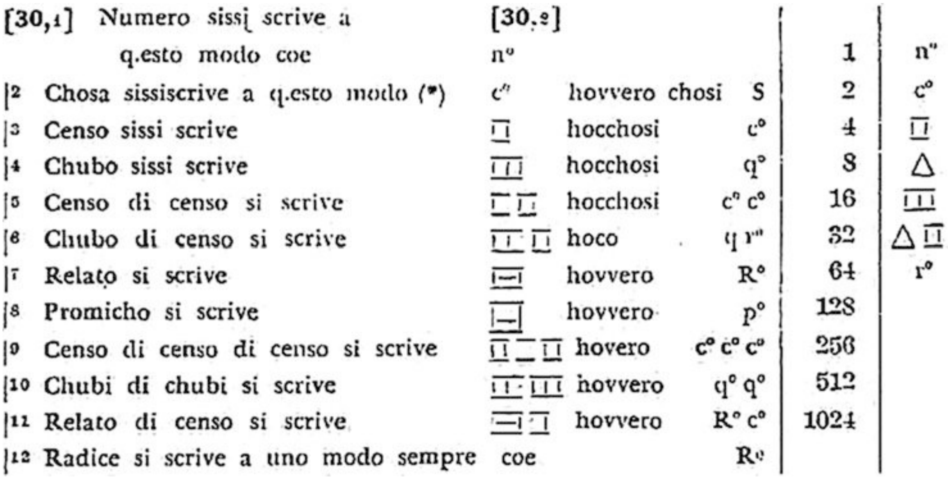

Figure 15. Canacci's scheme with the naming of powers, after [Procissi 1954: 432].

1954: 316–323] was mentioned above. In a couple of these he employs geometric signs for the powers, but mostly he writes *s* for *cosa* and *censo* in full. Addition may be indicated by juxtaposition, by *e*, by *più* or by *p*, subtraction by $\widetilde{m}$ or *me*.[78] Later Canacci presents an ordered list, with three different systems alongside each other – see Figure 15. To the right we find an extension of a different "geometric" system – namely the one which was found in a (secondary) marginal note in the Ottoboniano encyclopaedia. Next toward the left we find powers of 2 corresponding to the algebraic powers (an explanatory stratagem also used by Pacioli in the *Summa*); then letter abbreviations; and then finally, just to the right of the column with Canacci's full names, his own "geometric" system (not necessarily invented by him, but the one he uses in the schemes) – better planned for the economy of drawing than as a support for operations or algebraic thought. According to Cajori [1928: I, 112*f*] the system turns up again in Ghaligai's *Pratica d'arithmetica* from 1552 (and probably in the first edition from 1521, entitled *Summa de arithmetica*), where their use is ascribed to Ghaligai's teacher Giovanni del Sodo. ⟦[79]⟧

78 However, "p n" and "p n°" stand for "per numero". In schemes showing the stepwise calculation of products (pp. 313*f*), *m* stands for multiplication. In one scheme p. 318), a first *p* stands for *più*, a second in this way for *per*.

79 ⟦This can now be confirmed, they are described and ascribed to del Sodo in [Ghaligai 1521: fol. 72r].⟧

Canacci uses these last geometric signs immediately afterwards in an brief exposition of the rules for multiplying powers – and then no more. In a couple of marginal notes to the long collection of problems [ed. Procissi 1983: 58, 62–64] he uses the letter abbreviations (only *s* and *c*°) – but also the line as an indication, once of equality, twice of confrontation or correspondence not involving equality. The running text, including formal fractions, writes the powers unabridged (except *numero*, which once is *n*°); even *più* and *meno* are mostly written in full, but *meno* sometimes (pp. 21–23) with a brief stroke "–" – the earliest occurrence of the minus sign in Italy I know of.[80]

Three works by Luca Pacioli are of interest: his Perugia manuscript from 1478, the *Summa de arithmetica* from 1494, and his translation of Piero della Francesca's *Libellus de quinque corporibus regularibus* as printed in [Pacioli 1509].

Since there is only one brief observation to make on the latter work, I shall start by that. According to the manuscript Vatican, Urb. lat. 632 as edited by G. Mancini [1916: 499–501], Piero uses the familiar superscript square for *censo* when performing algebraic calculations, or he writes words; for *res* he uses a horizontal stroke over the coefficient, but mostly also keeps the word.[81] Pacioli [1509: fols. 3^{v}–26^{r}, *passim*] instead uses a sign ◈ for the *cosa* and ⧈ for the *censo* (or, in the old unsystematic way, words). *Censo di censi* is ⧈⧈ on fol. 4^{r} and ⧈ *de* ⧈ on fols. 4^{r} and 11^{v}. These geometric signs are absent from Pacioli's other works, and they must rather be considered a typographic experiment – given that their use is not systematic, they can hardly be understood as an instance of mathematical exploration beyond what Pacioli had done before. It is difficult to agree with Paola Manni [2001: 146] that they should represent "progress of mathematical symbolism" with respect to the more systematic use of letter abbreviations in the Perugia manuscript and the *Summa* (see imminently; and cf. the quotation from Woepcke after note 12). Indeed, the *Libellus* is an appendix to Pacioli's *Divina proportione*, in which Pacioli [1509: fol.3^{v}] explains that various professions, among whom *le mathematici per algebra*, use specific *caratheri e abreviature* "in order to avoid prolixity in writing and also of reading".[82]

80 As well known, "–" is already used in the *Deutsche algebra* from 1481 [ed. Vogel 1981: 20]. Whether this is part of the very mixed Italian heritage of this manuscript (see below, note 89 and surrounding text) or a German innovation eventually borrowed by Ghaligai is undecidable unless supplementary evidence should turn up.

81 The same (lack of) system is found in his abbacus treatise, see [Arrighi 1970: 12].

82 That Pacioli really thinks in terms of abbreviations is confirmed by a list of examples given in the manuscript of the treatise (Milano, Biblioteca Ambrosiana, Ms. 170 Sup., written in 1498), see [Maia Bertato 2008: 13]: it mixes the abbreviations for *radice, più, meno, quadrato* (*cosa* and *censo* are absent) with others for, *inter alia, linea, geometria* and *arithmetica.*

The 1478 Perugia manuscript *Suis carissimis disciplis* ... (Vatican, Vat. lat. 3129) has lost the systematic algebra chapters listed in the initial table of contents,[83] but it does contain a large amount of algebraic calculations. Everywhere here – in the main text as well as in the margin, and in the neat original prepared in 1478 as well as in fols. 350^{r}–360^{v}, added at a later moment and obviously very private notes – we find the signs from Canacci's right-hand column (Figure 15) written superscript and to the right – on fol. 360^{v} extended until ⊞, *censi di censi di censi. Meno* is ⓜ and *più* (both when signifying addition and as a normal word) a corresponding encircled *p*. This is thus the system which Pacioli used when calculating for himself, at least at that moment.[84] He also uses the equality line in the margin (but also the same line indicating confrontation/correspondence, e.g., fol. 130^{r}).

Most important (in the sense that it was immensely influential and the other two works not) is of course the *Summa* [Pacioli 1494]. Typographic constraints are likely to have caused Pacioli to give up his usual notation. In ordinary algebraic explanation and computation, he now uses *.co.* and *.ce.* written on the line, and *più* and *meno* have become p̃ and m̃ (*meno* sometimes mẽ) – both as operators and as indicators of positivity and negativity (not only additivity and subtractivity).[85] However, he also has more systematic presentations. The first, in the margin of fol. 67^{v}, shows how the sequence *.co.-.ce.* is to be continued, namely (third power) *cubo*, (4th) *censo de censo*, (5th) *primo relato*, (6th) *censo de cubo/cubo de senso*, (7th) *secundo relato*, (8th) *censo de censo de censo*, (9th) *cubo de cubo*, (10th) *censo de primo relato*, (11th) *terzo relato*, etc. until the 29th power. As we see, the embedding principle has now taken over completely, creating problems for the naming of prime-number powers. For each power the "root name" is indicated, number being "℞ *prima*", *cosa* "℞ 2^{a}", *censo* "℞ 3^{a}", etc.[86] As we see, the "root number" is *not* the exponent, but the exponent augmented by 1. This diminishes the heuristic value of the concept: it still permits to see directly that "6th roots and 4th roots equal 2nd roots" must be equivalent to "5th roots and 3rd roots equal 1st roots", but it requires as much thinking as in Jacopo's days almost 200 years earlier to see that

83 See the meticulous description in [Derenzini 1998], here p. 173. Since all abbreviations except the superscript symbols are expanded in the edition [Calzoni and Gavalzoni 1996], I have used a scan of the manuscript.

84 This restriction is probably unnecessary. At least the encircled *p* and *m* and the square are in the list offered by the 1498 manuscript, cf. note 82.

85 E.g., on fol. 114^{r}, "a partir . m̃.16.p m̃.2.ne ven.p̃.8", and the proof that "meno via meno fa più" on fol. 113^{r}, which is characterized as "absurda" and referred to the concept of a debt – if only subtractive numbers were involved, as in Dardi's corresponding proof, nothing would be absurd.

86 Pacioli believes (or at least asserts) that these names go back to "the practice of algebra according to the Arabs, first inventors of this art". Could he have been led to this belief by the equivalence of "root" and *thing/cosa* in al-Khwārizmī's algebra?

this is a biquadratic problem that must be solved as “3rd roots and 2nd roots equals 1st roots”.

After this list comes a list of symbols for “normal” roots: ℞ meaning *radici;* ℞℞ meaning *radici de* radici*;* ℞u. meaning *radici universale* or *radici legata*, that is, root of a composite expression following the root sign (encircled in Benedetto’s *Trattato* and spoken of as “℞ *de zonzo*” by Dardi, we remember); and ℞ cu., cube root.

On fol. 143^{r} follows a scheme that deals with the first 30 powers (*dignità*), and with how they are brought forth as products (*li nascimenti pratici o li 30 gradi de li caratteri algebratici*). It runs in four tangled columns and 30 rows. The first column has the numbered “root name” of the power, the second formulates in Pacioli’s normal language or in abbreviations that number times this power gives the same power. The third, written inside the second, indicates the corresponding power of 2. The fourth, finally, repeats the second column, now translated into root names – see Figure 16.

On the next page follow further schemes, expressed in roots names, for the products of the nth root with all roots from the nth to the (31–n)th (meaning that all products remain within the range defined by the 30th root), $2 \leq n \leq 15$.

All in all, we may say that Pacioli explored existing symbolic notations to a greater extent (and used them more consistently) than for example Benedetto, thus offering those of his readers who wanted it matters to chew; but he hardly gave them many solutions they could build on. Even in this respect, subsequent authors could easily have found reasons to criticize him while standing on his shoulders (as they did regularly), if only their own understanding of the real progress they offered had been sufficient for that. Tartaglia, for instance, gives the list of *dignitates* until the 29th in *La sesta parte del general trattato* [Tartaglia 1560: fol. 2^{r}], with names agreeing with Pacioli’s .co.-.ce.-list and indication of the corresponding exponents (now *segni* – indeed exponents as in the Modena manuscript, not Pacioli’s exponents+1), alongside a text that explains how multiplication of *dignitates* corresponds to addition of *segni;* that, however, was well after Stifel’s *Arithmetica integra*, which Tartaglia knew well.

8. Summary observations about the German and French adoption

Regiomontanus shows familiarity with algebraic practice, not only in the notes for the Bianchini-correspondence (cf. above) but also elsewhere – several articles in [Folkerts 2006] elucidate the topic in detail. Not only the calculation before note 69 but also some of his abbreviations (and the variability of these) are evident borrowings from Italian models [Høyrup 2007c: 134]. It might seem a not impossible assumption that Regiomontanus was the main channel for the adoption of Italian abbacus algebra into German areas, in spite of his purely ideological ascription of the algebraic domain to Diophantos and Jordanus (above, text before note 24).

An influence cannot be excluded, even though those of Regiomontanus’ algebraic notes we know about may not have circulated widely. However, those of his symbolic

℞.prima n°.uia	n°..fa numero.	℞.p².u².℞.p².fa.℞p²
℞.2ª.no°uia	2.co fa cosa.	℞.p².v².℞.2ª.fa.℞.2ª
℞.3ª.n°.uia	4.ce.fa censo.	℞.p².via.℞.3.fa.℞.3.
℞.4.n°.uia	8.cu.fa cubo.	℞.p².v².℞.4.fa.℞.4.
℞.5.n°.uia	16.ce.ce.fa censo de censo.	℞.p².via.℞.5.fa.℞.5.
℞.6.n°.uia	32.p°.r°.fa primo relato.	℞.p².via.℞.6.fa.℞.6.
℞.7.n° uia	64.ce.cu.uel cu.ce.fa ce cu.uel cu.ce.	℞.p².via.℞.7.fa.℞.7
℞.8.n°.uia	128.2°.r°.fa.2°.r°.	℞.p².via.℞.8.fa.℞.8.
℞.9.n°.uia	256.ce.ce.ce.fa ce.	℞.p².via.℞.9.fa.℞.9.
℞.10.n°.uia	512.cu.cu.fa cu.	℞.p².v².℞.10.fa.℞10
℞.11.n°.uia	1024.ce.p°.r°.fa ce.p°.r°.	℞.p².v².℞.11.fa.℞.11
℞.12.n°.uia	2048.3°.r°.fa.3°.r°.	℞.p².v².℞.12.fa.℞12
℞.13.n°.uia	4096.cu.ce.ce.uel ce.ce.cu.fa cu.ce.ce.ce.ce.cu.	℞.p².v².℞.13.fa.℞13
℞.14.n°.via	8192.4°.r°.fa.4°.r°.	℞.p².v².℞.14.fa℞14
℞.15.n°.via	16384.ce.2°.r°.fa ce.2°.r°.	℞.p².v².℞.15.fa.℞15
℞.16.n°.via	32768.cu.p°.r°.fa cu.p°.r°.	℞.p²v².℞.16.fa.℞.16
℞.17.n°.via	65536.ce.ce.ce.ce.fa.ce.ce.ce.cē.	℞.p².v².℞.17.fa.℞17
℞.18.n°.via	131072.5°.r°.fa.5°.r°.	℞.p².v².℞.18.fa.℞18
℞.19.n°.via	262144.cu.ce.cu.vel.ce.cu.cu.fa quello.	℞.p².v².℞.19.fa.℞19
℞.20.n°.via	524288.sexto relato fa.6°.r°.	℞.p².v².℞.20.fa.℞20
℞.21.n°.via	1048576.primo.r°.fa ce.ce.primo r°.	℞.p².v².℞.21.fa.℞21
℞.22.n°.via	2097152.cu.2°.r°.fa cu.2°.r°.	℞.p².v².℞.22.fa.℞22
℞.23.n°.via	4194304.ce.3°.r°.fa ce.3°.r°.	℞.p².v².℞.23.fa.℞23
℞.24.n°.via	8388608.7°.r°.fa.7°.r°.	℞.p²v²℞.24.fa℞.24
℞.25.n°.via	16777216.cu.ce.ce.ce.uel ce.ce.ce.cu.fa q̄llo.	℞.p².v².℞.25.fa℞.25
℞.26.n°.via	33554432.8°.r°.fa.8°.r°.	℞.p².v²℞.26.fa.℞.26
℞.27.n°.via	67108864.ce.4°.r°.fa ce.4°.4°.	℞.p²v².℞.27.fa℞.27
℞.28.n°.via	134217728.cu.cu.cu.fa cu.cu.cu.	℞.p².v²℞.28.fa.℞.28
℞.29.n°.uia	268435456.ce.ce.2°.r°.fa ce.ce.2°.r°.	℞.p².v²℞.29.fa.℞.29
℞.30.n°.uia	736870912.9°r°.fa.9°.°.	℞.p².v²℞30.fa.℞.30

Figure 16. Pacioli's scheme [1494: fol. 143r] showing the powers with root names.

notations or abbreviations which are not to be identified as Italian are already present in a section of a manuscript possessed by Regiomontanus but not written by him [Folkerts 2006: V, 201*f*], cf. [Høyrup 2007c: 136*f*].[87].

That Regiomontanus was at most one of several channels can also be seen from the so-called *Deutsche Algebra* from 1481 [ed. Vogel 1981]. Its symbols[88] for *number* (*denarius*, replaces earlier *dragma*), *thing* and *census* coincide with those of the Robert-Appendix,[89] that for the *cube* with the one Regiomontanus employs for *census* – hardly evidence for inspiration from Regiomontanus. A token of Italian inspiration certainly *not* passing through Regiomontanus is occasional use of the quasi-fraction notation for powers and of 1^c for *cosa* [Vogel 1981: 10] – all in all, as Kurt Vogel observes, evidence that a number of sources flow together in this manuscript.

I shall not consider in detail German algebraic writings from the 16th century (Rudolff, Ries, Stifel, Scheubel), only sum up that with time German algebra tends to be more systematic and coherent in its use of symbolism (for notation as well as calculation) than any single Italian treatise.[90] But what the German authors do is to combine and put into system ideas that are all present in *some* Italian work. They never really go beyond the Italian inspiration *seen as a whole*, and never attain the coherence which appears to have been reached by the Maghreb algebraists of the 12th century.[91]

I shall also be brief on what happened in French area. Scrutiny of Nicolas Chuquet's daring exploration of the possibilities of symbolism in the *Triparty* from 1484 [ed. Marre 1880] would be a task of its own; his parenthesis (an underlining[92]) and his complete

87 The *thing* symbol in the appendix to Robert of Chester's translation of al-Khwārizmī (above, note 20) is the same as Regiomontanus's transformation of ρ; the *census* symbol is a *z* provided with a final curlicue and *could* be derived from the ȿ which we find in the Modena-manuscript but is much more likely to correspond to its initial use of *z* in this function.

88 Listed in [Vogel 1981: 11].

89 With ∂ as an alternative for *thing*, standing probably for *dingk.*

90 The use of schemes for polynomial arithmetical calculation by Stifel [1544] and Scheubel [1551] was mentioned above. They also appear in Rudolff's *Coss* [1525].

91 Quite new as far as I know, and awkwardly related to the drive toward more systematic use of notations (but maybe more closely to the teaching of Aristotelian logic), is the idea to represent persons appearing in commercial problems by letters A, B, C, …. I have noticed it in Magister Wolack's Erfurt lecture from 1467, apparently the earliest public presentation of abbacus mathematics in German land [ed. Wappler 1900: 53*f*], and again in Christoff Rudolff's *Behend und hübsch Rechnung durch die kunstreichen Regeln Algebra* #128 [1525: fol. N v^{r-v}.]

92 The only parentheses Italian symbolic notation had made use of were those marked off by the fraction line and the ℞ *de zonzo/legata/universale.* The latter, furthermore, was ambiguous – how far does the expression go that it is meant to include? A parenthesis as good and universal as that of Chuquet had to await Bombelli [1572], even though Pacioli [1494] uses brackets containing *textual*

arithmetization of the notation for powers as well as roots certainly goes beyond what can be found in anything Italian until Bombelli, and (as far as the symbols for powers and roots are concerned) even beyond the Maghreb notation. However, his innovations were historical dead ends; Etienne de la Roche, while transmitting other aspects of Chuquet's mathematics in his *Larismetique* from 1520, returned to more familiar notations [Moss 1988: 120*f*]. What later authors learned (or, like Buteo, refused to learn, *ibid.*, p. 123) from de la Roche could as well have been Italian.[93]

As a representative of the French mid-16th century I shall choose Jacques Peletier's *L'algebre* from [1554] – interesting not least because his orthographic reform proposal [1555; 1554, final unpaged note] shows him to have reflected on notation. Peletier knows Stifel's *Arithmetica integra*, cites it often and learns from it. But he must be acquainted with the Italian abbacus tradition, and not only through Pacioli and Cardano, whom he cites on p. 2: he speaks of the powers as *nombres radicaus* (p. 5), and uses ℞ for the first power (this, as well as the *nombres radicaus, could* at a pinch be inspired by Pacioli) and the stylized ç (ʓ) which we know from the Modena-manuscript for the second power (following Stifel for higher powers). That certainly does not help him go beyond the combination of the most developed elements of Italian symbolism we know from the German authors – and like Stifel he does not get beyond.

9. Why should they?

As we have seen, Italian abbacus algebra makes use of a variety of elements that might have been (and in the main probably were) borrowed from the Maghreb, most of them already present in one or the other manuscript from the 14h century. But the abbacus masters do not seem to have been eager to use them consistently, to learn from each other or to surpass each other in this domain (to which extent they wanted to avoid to *teach* symbolism is difficult to know – it would not have the same value in the competition for jobs and pupils as the ability to solve intricate questions); Benedetto and the compilers of the Ottoboniano and Palatino encyclopaediae were quite satisfied with repeating a heritage that may reach back to Antonio, and did not care about the schemes for polynomial arithmetic that had been in circulation at least since Dardi's times. Only with the Modena manuscript, with Canacci and with Pacioli's *Summa* do we find some effort to be encyclopedic (if not systematic) also in the presentation of notations.

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parentheses (e.g., on fol. 3^r). As we remember from note 12, even Descartes eschews general use of the parenthesis

[93] The question to which extent the Provençal tradition which Chuquet draws upon was independent of the Italian tradition (to some extent it certainly was) is immaterial for the present discussion; no surviving earlier or near-contemporary Provençal writings offer as much incipient symbolism as the Italian abbacus writers.

Our meeting is about the "philosophical aspects of symbolic reasoning", and about "early modern science and mathematics". The philosophical question to raise to the material presented above is whether the abbacus masters of the 14th and 15th centuries, and even the algebraic writers of the early and mid-16th century, had any *reason* to develop a coherent symbolic approach. The answer seems to be that they had none (cf. also note 50 and preceding text). The kind of mathematics they were engaged in (even when they applied their art to *Elements* X, as do for instance Fibonacci and Stifel) did not ask for that. They might sometimes extrapolate their technique further than their mathematical practice asked for – 29 algebraic powers is an example of that, as is of course the creation of never-used symbols for these powers. But without a genuine practice there was nothing which could force these extrapolations to merge into a consistent conceptual and operational framework. Even those abbacus authors that had scholarly ambitions – as Benedetto and his contemporary encyclopedists, and later Pacioli and Tartaglia – did not encounter anything within the practice of university or Humanist mathematics which asked for much more than they did. To the contrary, the aspiration to connect their mathematics to the Euclidean ideal made them re-attach geometric proofs to a tradition from which these had mostly been absent, barring thereby the insight that purely arithmetical reasoning could be made as rigorous as geometric proofs – barring it indeed to such an extent that Ries and Scheubel rejected Jordanus' arithmetical rigour and borrowed only his problems, as we have seen.

That changed in the outgoing 16th century. By then (if I may be allowed some concluding sweeping statements), Apollonios and Archimedes were no longer mere names (or at most authors of difficult texts to be assimilated) but providers of problems to be worked on, and trigonometry had become an advanced topic.[94] This was probably what created the pull on the development of symbolic reasoning and of those notations that symbolic reasoning needed if it was to go beyond simple formal fractions; the reaction to this pull (which at first created a complex of new mathematical developments) was what ultimately transformed symbolic mathematics into a factor that could (eventually) push the development of (some constituents of) early modern science.

94 It may perhaps be allowed to give a frivolous illustration of a sweeping statement: the problems which the 16–17 years old Huygens investigated by means of Cartesian algebra under the guidance of Franz van Schooten. Quite a few of them deal with matters from Archimedes or Apollonios [Huygens 1908: 27–60]. The problems he dealt with 4–5 years later (pp. 217–275 in the same volume) are derived from Pappos, and even they make extensive use of Descartes' technique. It is difficult to image that they could have been well served by cossic algebra, with or without the abbreviations that had been standardized in the mid-16th century.

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Chapter 31 (Article II.14) Embedding – Another Case of Stumbling Progress

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧

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Abstract

At an earlier occasion I have argued that the development toward full algebraic symbolism in Europe was a case of "stumbling progress", before Viète never really intentional. Here I shall concentrate on a particular aspect of algebraic symbolism, the one that allowed Cartesian algebraic symbolism to become the starting point not only for theoretical algebra but for the whole transformation of mathematics from his times onward: The possibility of embedding, that is, of making a symbol or an element of a calculation stand not only for a single number, determined or undetermined, but for a whole expression (which then appears as an algebraic parenthesis).

From the Italian beginning in 14th century, and also in ibn al-Yāsamin's (?) first creation of the Maghreb letter symbolism, the possibility of embedding was understood and explained in the simple case where a fraction line offered itself as defining a parenthesis; Diophantos, without a line, did something similar on at least one occasion. However, only Chuquet and Bombelli would explore some of the possibilities beyond that, and Viète still less. Even Descartes did not take full advantage.

A final section argues, from the character of the mathematical practice in which medieval and Renaissance algebra participated, why this stumbling character of development should not bewilder us.

Contents

Im memory of Ivor Grattan-Guinnes
friend and wise colleague
master of polemics

Approaches to algebraic symbolism[1]

As is well known, Georg Nesselmann's *Algebra der Griechen*[2] suggested a classification of algebra types into three groups: rhetorical, syncopated, and symbolic [Nesselmann 1842: 302]. In "rhetorical algebra", everything in the calculation is explained in full words. "Syncopated algebra" uses standard abbreviations for certain recurrent concepts and operations, while "its exposition remains essentially rhetorical".[3] In "symbolic algebra" (as known to us as well as to Nesselmann), "all forms and operations that appear are represented in a fully developed language of signs that is completely independent of the oral exposition". He also characterized these types as "stages" (*Stufen*), a term that normally indicates ascent and thus chronology; but it is clear from his examples that this division into chronological stages is at most meant locally, not as steps of some universal history.

According to Nesselmann, the rhetorical stage is represented by Iamblichos, by "all so far known Arabic and Persian algebraists", and by all Christian-European writers on algebra until Regiomontanus. Diophantos, and the later Europeans until well into the 17th century are classified as syncopated,

> although already Viète has sown the seeds of modern algebra in his writings, which however only sprouted some time after him

In the following pages, Nesselmann mentions Oughtred, Descartes, Harriot and Wallis as creators of this modern, symbolic algebra.

> However, we Europeans since the 17th century are not the first to have attained this level; indeed, the *Indian* mathematicians anticipate us in this domain by many centuries.

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1 The first version of the paper was presented at the workshop "Mathematics in the Renaissance: Language, Methods, and Practices", ETSEIB, Barcelona, 23 January 2015. I thank for the invitation, and also thank Maria Rosa Massa Esteve for commentaries.

2 The first volume of his *Versuch einer kritischen Geschichte der Algebra. Nach den Quellen bearbeitet* – and, as it turned out, the only volume to appear. In 1843 he published an edition and translation of an Arabic practical arithmetic, after which followed work on Baltic languages (an Indo-European group which he named) and a Sanskritist and Arabist chair. As many Orientalists of his day, he was thus versed in all the languages required for the topic as it could be studied at the time – Latin, Greek, Arabic, Sanskrit, as well as modern European languages. We may envy him.

3 Here and in the followings, all translations into English are mine if nothing else is indicated.

Probably because they are used by most of those historians who refuse to see *every* use of algebraic abbreviations as a "symbolism", Nesselmann's categories have often been criticized – obvious mistakes or platitudes from the 1840s would have been forgotten long ago. What follows may be read as an attempt to elaborate, substantiate and revise what Nesselmann says in a couple of pages.

As if we all knew and agreed upon what symbolic algebra is, Nesselmann's central observation about what characterizes the symbolic level has mostly been neglected: namely that symbolization allows operations directly on the level of the symbols, without any recourse to thought carried by spoken or internalized language – indeed, almost without recourse to reflective thought. In Nesselmann's words

> We may execute an algebraic calculation from the beginning to the end in fully intelligible way without using one written word, and at least in simpler calculations we only now and then insert a conjunction between the formulae so as to spare the reader the labour of searching and reading back by indicating the connection between the formula and what precedes and what follows.

That is exactly what we do when we reduce an equation by additions, divisions, differentiations, and whatever else we may need to apply. We can of course speak *about* the operations we perform, just as we may speak about the operations we perform when changing the tyre on a bicycle or preparing a sauce; but in all three cases *the operations themselves are outside language.*

To illustrate this we may look at two instances of incipient symbolic operation – one from Diophantos, the other from the Italian 14th century.

In the *Arithmetic*, Diophantos uses abbreviations (spoken of as "signs" [σημεῖον]) for the unknown number (the *arithmós*) and its powers.[4] The unknown itself is written with a simple sign, something like ς; for the higher powers (*dynamis* = $ς^2$, *kybos* = $ς^3$, *dynamodynamis* = $ς^4$, etc.), phonetic complements are added (Δ^{Y}, K^{Y}, etc.); similarly, complements are added to the sign for the monad ("power zero"), and for numbers occurring as denominators in fractions,[5] except in the compact writing of fractions where $^{5}_{16}$ means $\frac{16}{5}$. Addition is implied by juxtaposition, subtraction and subtractivity are denoted by the abbreviation ⩚ (λεῖψις, "missing" etc.). Only one sign occasionally serves direct operation: the designation of the "part denominated by" n (better, indeed, since n is not always integer, the reciprocal of n); the introduction explains it to be indicated by a sign $^{\times}$ for powers of the unknown. In III.xi [ed. Tannery 1893: I, 164] we see that a number

[4] Manuscripts do not agree about when and when not to use an abbrevation, but all use them; Diophantos' introduction leaves no doubt that they are really his, and no later scribal invention.

[5] In I.23 [ed. Tannery 1893: I, 92], $\frac{50}{23}$ appears as $\bar{\nu}\,\kappa\gamma^{\omega\nu}$, "50 of 23rds", and $\frac{150}{23}$ slightly later as "150 of the said part".

which was posited to be $\varsigma^{\times}$ is stated immediately to be $\frac{41}{77}\left(\frac{77}{41}\right)$ when ς itself turns out to be $\frac{77}{41}$. This would hardly have been possible if Diophantos had not known at the level of symbols (and supposed his reader to recognize) that $(\varsigma^{\times})^{\times} = \varsigma$, and that $\left(\frac{q}{p}\right)^{\times} = \frac{p}{q}$. But this, as far as I have noticed without having worked systematically on the text, is the only instance of genuine symbolic operation.

Let us next look at a Tuscan *Trattato dell'alcibra amuchabile,* a compound in three parts from c. 1365.[6] In the third part we find [ed. Simi 1994: 41*f*] the request to divide 100 first by a "quantity" and then by the "quantity" plus five. The sum of the quotients is told to be 20. So, you should first divide 100 by a *cosa* ("a thing"), and next by "a *cosa* and 5", and join the two quotients. Similar problems (though with subtraction) are found in al-Khwārizmī's and Abū Kāmil's algebras [ed. Hughes 1986: 255; ed. Rashed 2013: 352–354], and again in Fibonacci's *Liber abbaci* [ed. Boncompagni 1857: 413]. Al-Khwārizmī gives a purely numerical (but sensible) prescription for the initial, difficult steps – obviously, what he did went beyond his technical vocabulary; Abū Kāmil uses a geometric diagram; and Fibonacci applies proportions. The 14th-century treatise, however, comes close to what we would do:

> Now I want to show you something similar so that you may well understand this addition, and I shall say thus: I want to join 24 divided by 4 to 24 divided by 6, and you see that it should make 10. Therefore write 24 divided by 4 as a fraction, from which comes $\frac{24}{4}$. And posit similarly 24 divided by 6 as a fraction. Now multiply in cross, that is, 6 times 24, it makes 144; and now multiply 4 times 24, which is above the 6, it makes 96, join it with 144, it makes 240. Now multiply that which is below the strokes, that is, 4 times 6, it makes 24. Now you should divide 240 by 24, from which 10 should result. […]

Then follows the application:

> Now let us return to our problem. Let us take 100 divided by a *cosa* and 100 divided by a *cosa* and 5 more, and therefore posit these two divisions as if they were fractions, as you see hereby.
>
> $$\frac{100}{\text{per una cosa}} \qquad \frac{100}{\text{per una cosa e più 5}}$$
>
> And now multiply in cross as we did before, that is, 100 times a *cosa,* which makes 100 *cose.* And now multiply along the other diagonal, that is, 100 times a *cosa* and five, it makes 100 *cose* and 500 in number; join with 100 *cose,* you get 200 *cose* and 500 in

6 The first part contains the sign rules and teaches operations with roots and binomials; the second gives the rules, mostly provided with examples, for the basic "cases" (equation types) until the fourth degree (some of them false); the third, finally, is a problem collection. ⟦A more detailed description can be found in article I.12.⟧

> number. Now multiply that which is below the strokes, one with the other, it makes a *censo*[7] and 5 *cose* more. Now multiply the results, that is, 20 against a *censo* and 5 *cose* more, it makes 20 *censi* and 100 things more, which quantity equals 200 things and 500 in number. …

This text, we see, is purely rhetorical – everything is written out in full words. On the other hand, the solution proceeds by means of formal operations, in a way we are accustomed to in symbolic algebra; rhetorically expressed polynomials are dealt with *as if* they were the numbers of normal fraction arithmetic. We may say that the *lexicon* of the text is rhetorical, but its *syntax* (in part) symbolic.[8]

Characteristic for this syntax is the phenomenon of *embedding:* the insertion of something possibly complex in the place of something simpler. We know the phenomenon from ordinary language making use of subordinate clauses: *I go now* → *I go when it pleases me*. In contemporary symbolic mathematics indefinitely nested embedding is possible – for instance, in continued fractions, or in the graphically simpler expression

$$1+\tfrac{x}{2}\cdot\left(1+\tfrac{x}{3}\cdot\left(1+\tfrac{x}{4}\cdot\left(1+\tfrac{x}{5}\cdot\left(1+\tfrac{x}{6}\cdot(\ldots)\right)\right)\right)\right).$$

In ordinary language, the same possibility is present, restricted only by pragmatic considerations of comprehensibility – "This is the man all tattered and torn / That kissed the maiden all forlorn / That milked the cow with the crumpled horn / That tossed the dog / That worried the cat / That chased the rat / That ate the cheese / That lay in the house / that Jack built".

We may now turn back to Nesselmann. As we remember, he ascribed to the Indian mathematicians a symbolic algebra that precedes that of Europe by many centuries.

We may look at an example, borrowed from Bhaskara II (b. 1115) via [Datta & Singh 1962: II, 31*f*]. What we would express

$$5x+8y+7z+90 = 7x+9y+6z+62$$

is written by Bhāskara as a scheme

$$\begin{array}{llll} y\hat{a}\ 5 & k\hat{a}\ 8 & n\hat{\imath}\ 7 & r\hat{u}\ 90 \\ y\hat{a}\ 7 & k\hat{a}\ 9 & n\hat{\imath}\ 6 & r\hat{u}\ 62 \end{array}$$

7 The *censo* is the square on the *cosa.*

8 (An aside:) And why not? As pointed out by André Weil in a famous polemical note [1978: 92] that deserves to be read for much more than its venomous concluding paragraph, "words, too, are symbols". *We,* when reducing "3 *things* and two added equal 17" into "3 *things* equal 15" probably use our training in letter algebra, that is, use the syntax of symbolism, stepping outside the framework of grammatical language and forgetting for a while to think of that which the words stand for. A genuinely rhetorical solution would follow the principles of Euclid's common notions (if only at the intuitive level): "But then, since removing equals from equals gives equals, 3 *things* alone must equal 17 with 2 removed", etc. Whether an algebraic text becomes truly rhetorical or hiddenly symbolic depends in part on the reader.

while our

$$8x^3+4x^2+10y^2x = 4x^3+0x^2+12y^2x$$

appears as

$$\begin{array}{llllllll} y\hat{a} & gha\ 8 & y\hat{a} & va\ 4 & k\hat{a} & va & y\hat{a}.bh\hat{a}\ 10 \\ y\hat{a} & gha\ 4 & y\hat{a} & va\ 0 & k\hat{a} & va & y\hat{a}.bh\hat{a}\ 12 \end{array}$$

Datta and Singh quote David Eugene Smith [1923: II, 425*f*] for the stance that this notation is "in one respect […] the best that has ever been suggested", namely because it "shows at a glance the similar terms one above the other, and permits of easy transposition".

However, the Indian schemes do not permit direct multiple embedding – for instance the replacement of *yâ* by a polynomial. Nor are they meant for that, they serve exclusively for reducing one side of an equation to zero. The rest of the argument (the initial part that precedes the scheme as well as that based on the reduced equation) is as syncopated as that of Diophantos, albeit with a more systematic use of the abbreviations (and operating with several unknowns) – see the chapter "Varieties of Quadratics" in Bhāskara's *Vijā-gaṅita* [ed. trans. Colebrooke 1817: 245–267]. Replacing a simple by a composite expression requires the same amount of thinking in the Indian notation as in a rhetorically expressed algebra. It is not impossible in either case.

Indian schemes allow certain direct operations, and in this sense they clearly constitute a symbolism, as claimed by Nesselmann. However, Smith is right that this notation is the best "in one respect" only – namely for linear reductions within the restricted framework of problem types actually dealt with by Bhāskara. It allows operations directly at the level of symbols, but only a rather limited, non-expandable set of operations.

Stumbling progress toward algebraic symbolism

On an earlier occasion [Høyrup 2010] ⟦= article II.13⟧ I have described the slow development of algebraic symbolism, from the first introduction in late 12th-century Maghreb to the final unfolding around Viète and Descartes – not only "hesitating", as my title said, but stumbling. A summary will be useful for the following.

At some moment mathematicians in the Islamic West (the Maghreb, in the general sense including also al-Andalus) invented not only the writing of fractions with a stroke (taken over in the Latin *Liber mahameleth*, plausibly from the 1160s) but also notations for composite fractions, most important the notation for ascending continued fractions such as $\frac{e}{f}\frac{c}{d}\frac{a}{b}$ meaning $\frac{a}{b}+\frac{c}{bd}+\frac{e}{bdf}$ (they are used in Fibonacci's *Liber abbaci,* and almost certainly already in the lost first version from 1202).

Probably towards the very end of the century (Fibonacci seems not to know about it), an algebraic symbolism was created, with symbols for powers zero to three of the unknown, and signs for subtraction, inverse, square root and equality; ibn al-Yāsamin († 1204) may have been the inventor. It was first described by Franz Woepcke in [1854]

on the basis of its use by al-Qalaṣādī (15th c.), that is, well after Nesselmann's perspicacious reference to "all so far known Arabic and Persian algebraists". Already Woepcke suspected from ibn Khaldūn's report that the notation might go back to the 12th century, as now confirmed by scattered occurrences in writings of ibn al-Yāsamin – see [Abdeljaouad 2002: 20, 24*f*]; from these early traces it is not clear whether the full system we know from later centuries was there from the beginning. In this full system, signs for the powers are written above their coefficient, the root and inverse signs above their argument. The signs are derived from the initial letters of the corresponding words but provided with tails enabling them to cover composite expressions, that is, to delimit algebraic parentheses; the notation served to write polynomials and equations, and even to operate on the equations.

The phrase "algebraic parentheses" asks for two observations. Firstly, a parenthesis is not a (round, square or curly) bracket nor a pair of brackets but an expression that is marked off, *for example* by a pair of brackets; in spoken language, pauses may mark off a parenthesis in the flow of words, and in written prose these are often rendered as a pair of dashes. An *algebraic parenthesis* is an expression marked off as a single entity that can be submitted as a whole to operations; in calculation it has to be determined first. When division is indicated by a fraction line, this line delimits the numerator as well as the denominator as parentheses if they happen to be composite expressions (for instance, polynomials). Similarly, the modern root sign $\sqrt{}$ marks off the radicand as a parenthesis.

Secondly, it is to be observed that the Maghreb notation, though possessing the parenthesis function, does not exploit it fully. More on this below.

The early evidence is accidental, but later extant Maghreb writings are sometimes systematic in their use of the notation, showing that at least its fully developed form can be regarded as a genuine symbolism at the Indian level (though so different in character that influence one way or the other can be safely disregarded).

In these later writings, the symbolic calculations are as a rule made separately from the running text (as can be seen in Woepcke's translation of al-Qalaṣādi), usually following after a phrase "its image is" and thus illustrating the preceding rhetorical exposition. They can also stand as marginal commentaries, as in the "Jerba manuscript" (written in Istanbul in 1747) of ibn al-Hā'im's *Šarh al-Urjūzah al-Yasminīya*, "Commentary to al-Yāsamin's *Urjuza*" (originally written in 1387) [ed. Abdeljaouad 2004]. Such marginal calculations probably correspond to what was written on a *takht* (a dustboard, in particular used for calculation with Hindu numerals) or a *lawha* (a clayboard used for temporary writing) – see [Lamrabet 1994: 203] and [Abdeljaouad 2002: 27, 34*f*].

Fibonacci, as stated, does not know the Maghreb notation (his copious use in non-algebraic contexts of rectangular schemes rendering what would be written on a *lawha* makes it almost certain he would have used it if he had known about it). Nor does the earliest generation of abbacus algebra as represented by Jacopo da Firenze's *Tractatus*

algorismi [ed. Høyrup 2007a].[9] Even algebraic abbreviations are absent in this earliest phase, although abbreviations are of course used profusely in the writing of current words.

Soon, however, some traces of symbolic operation turn up. Paolo Gherardi's *Libro de ragioni* from 1328 [ed. Arrighi 1987: 101] describes operations on a diagram (itself missing in the copy, which also has a defective text on this point,[10] but which is found in a parallel text[11]):

```
100   1 cosa
100   1 cosa piu 5
```

The context is the same problem as discussed above, just after note 6. It is clear that the same operations are thought of, even though the diagram is more rudimentary.

In the first part of the *Trattato dell'alcibra amuchabile,* schemes are used to teach the multiplication of binomials – for example (we now observe the abbreviation ℞ for *radice,* "root"):

```
5 e   piu  ℞ di 20
via
5 e   meno ℞ di 20
```

The binomials are numerical, but since al-Khwārizmī irrational roots had been used so to speak as pedagogical stand-in for algebraic roots (square roots of the *censo,* that is, *cose*).

The *Trattato dell'alcibra amuchabile* was written in c. 1365, but even this part of its material is probably older. In Dardi of Pisa's *Aliabraa argibra* from 1344,[12] we find something similar though more elaborate:

9 There are strong reasons to suppose that this algebra, present in only one of the three manuscripts, belongs to Jacopo's original work; but even if it should be a secondary insertion, its closeness to the second section of the *Trattato dell'alcibra amuchabile* (above, note 6) and the way the two texts are reflected in Paolo Gherardi's *Libro de ragioni* from 1328 shows that it must antedate the latter treatise – see [Høyrup 2007a: 23–25, 163*f*] and hence all other extant vernacular algebra texts.

10 Unless, of course, Gino Arrighi copies the manuscript badly. However, I doubt that Arrighi would first read *parto* as *porto*, then omit *e poi parto 100 in più 5 che prima* (or something similar), and finally also omit a diagram spoken of in the text.

11 Florence, Ricc. 2252, see [Van Egmond 1978: 169].

12 I use the manuscript Vatican, Chigi M.VIII.170, written in Venetian in c. 1395, checking with Van Egmond's personal transcription of a manuscript from 1429 actually held by Arizona State University Temple, for access to which I am grateful. In some of the details, the Arizona manuscript appears to be superior to the others, but at the level of overall structure the Chigi manuscript is demonstrably better – see [Høyrup 2007a: 169*f*]. Considerations of consistency suggests it to be better also in its use of abbreviations and other quasi-symbolism, for which reason I build my presentation on this manuscript (cross-checking with the transcription of the Arizona-manuscript – differences on this account are minimal); for references I use the original foliation.

3 m̃ ℞ de 5
                    14 m̃ ℞ de 180
3 m̃ ℞ de 5

Here we find a supplementary abbreviation, $\widehat{m}$ for *meno*, “less”. Dardi indeed uses abbreviations systematically: *radice* is always **℞**, *meno* (“less”) is *me*, *cosa* is *c*, *censo* is *Ç*,[13] *numero/numeri* are *nũo/nũi. Cubo* is unabridged, *censo de censo* (the fourth power) appears not as *ÇÇ* but as *Ç de Ç* (an expanded linguistic form which we may take as an indication that Dardi thinks in terms of abbreviation and nothing more). Roots of composite entities are written by a partially rhetorical expression, for instance (fol. 9^v) “**℞** *de zonto* $\frac{1}{4}$ *cô* **℞** *de* 12” (meaning $\sqrt{\frac{1}{4}+\sqrt{12}}$; *zonto* corresponds to Tuscan *gionto,* “joined”; that a root is “of joined *a* with *b*” thus means that it is taken of the composite *a*+*b*).

Algebraic monomials are written in a way which we might be tempted to see as an inversion of the Maghreb notation – for instance, “4 *cose*” is written $\frac{4}{c}$. The same notation is used in the original manuscript of the *Trattato di tutta l’arte dell’abbacho* from 1334.[14] Closer inspection of the use reveals, however, that the notation must be understood as a mere reflection of the spoken form, in analogy with the frequent writing of the ordinal *il terzo* as “il $\frac{1}{3}$” (for example number three of “three men”) − that is, the fraction notation itself is not understood as an indication of division but as a way to write the ordinal form of the numeral. Even though Dardi was indubitably the best abbacus mathematician of his days and the first to write a treatise dealing solely with algebra, and more consistent in his use of algebraic standard abbreviations than anybody else in his century, he saw no point in exploring the possibilities of symbolic operations.

All in all, until the mid-14th century the only symbolic operations we find are those on formal fractions and the multiplication of binomials in schemes – both rather rudimentary, the former plausibly inspired from Maghreb practices, the latter perhaps an independent development. Algebraic abbreviations remained abbreviations and nothing more, and only Dardi used them systematically.

In the early 15th century, the use of standard abbreviations (*co* and *ce*) for *cosa* and *censo* become common (but more often used in marginal annotations than in the running text, rarely very systematically, and very rarely for symbolic operations); they are often written above the coefficient, which might suggest inspiration from Maghreb ways. The first trace of such recent interaction is the algebra section of a *Tratato sopra l’arte della*

13 Dardi probably thinks of the spelling *çenso,* which corresponds to orthographic habits of his times in north-eastern Italy. In the 15th century it was to become *zenso*, which explains the terms and abbreviations of German cossic algebra.

14 Florence, Bibl. Naz. Centr., fond. princ. II.IX.57. For the dating and for reasons *not* to ascribe the work to Paolo dell’Abbaco, see [Cassinet 2001].

arismetricha written in Florence around 1390.[15] Probably indirect contact of some kind with the Arabic world is suggested by the use of *censo* for an amount of money which the compiler (in spite of being apparently an extraordinary mathematician) does not understand – after having found the *censo* he takes its square root, believing it has to be an algebraic square, and then has to multiply it by itself in order to find the unknown amount. Beyond sophisticated use of polynomial algebra in the transformation of equation types, we find here a clear discussion of the sequence of algebraic powers as a geometric progression, to which we shall have to return.

The running text contains no abbreviations and certainly nothing foreshadowing symbolic operations. Inserted to the left, however, we find a number of schemes explained by the text and showing multiplication of polynomials with two or three terms (numbers, roots and/or algebraic powers).

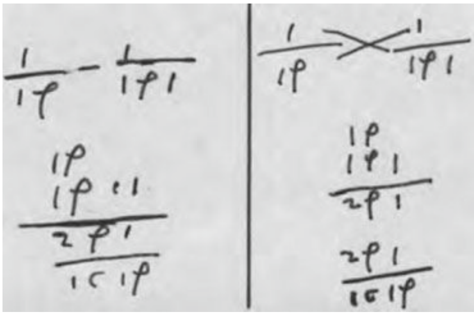
Those involving only binomials are related to those of the *Trattato dell'alcibra amuchabile* and Dardi. The schemes for the multiplication of three-term polynomials are of a different kind. They emulate the scheme for multiplying multi-digit numbers, and the text itself justly refers to multiplication *a chasella* as the model [ed. Franci and Pancanti 1988: 9]. The *a casella* algorithm (roughly identical with ours) solely differs from the older *a scacchiera* algorithm, used in the Maghreb multiplication of polynomials (see the "Jerba manuscript" [ed. Abdeljaouad 2002: 47]), by using vertical instead of slanting columns.

Such schemes (and other schemes for calculation with polynomials) turn up not only in later abbacus writings (for instance, in Raffaello Canacci's *Ragionamenti d'algebra* [ed. Procissi 1954: 319 and *passim*], on which more below) but also in numerous 16th-century algebras – for example, Stifel's *Arithmetica integra* [1544: fols. 3^{v}*ff*], Jacques Peletier's *L'Algèbre* [1554: 15–22] and Petrus Ramus's *Algebra* [1560: fol. A iii^{r}]. On the other hand, schemes *of this type* are absent from the three major "abbacus encyclopediae" from c. 1460, all three Florentine and in the tradition reaching back via Antonio de' Mazzinghi (c. 1353 to c. 1391 [Ulivi 1996: 110*f*]) to Paolo dell'Abbaco and Biagio "il vecchio" (respectively mid- and early-mid-14th c.). Most famous and known from many copies is Benedetto da Firenze's *Trattato de praticha d'arismetrica.* The other two (both known only from the autograph) are Florence, Palatino 573, and Vatican, Ottobon. lat. 3307 – the compilers of the latter two being both pupils of a certain Domenico d'Agostino *vaiaio*.

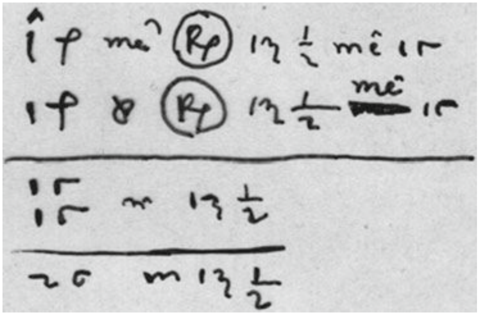
On the other hand, here we find marginal schemes of this type:[16]

15 Bibl. Naz. Centr., fondo princ. II.V.152. Its algebraic section was edited by Raffaella Franci and Marisa Pancanti [1988].

16 A marginal calculation accompanying the same problem from Antonio's *Fioretti* in Siena L.IV.21, fol. 456^{r} and Ottobon. lat. 3307, fol. 338^{v}.



The appearance of the scheme in similar shape in the different encyclopediae suggests that it goes back to Antonio (from whom the problem itself is borrowed). We also find an abundance of formal fractions, and schemes of a different kind for the multiplication of binomials (ρ stands for *cosa, c* for *censo*):[17]



Formal fractions *without* abbreviation are used in the presentation of the arithmetic of algebraic powers in Book XIII (fols.

All in all, as I summarized the matter in [Høyrup 2010: 39] ⟦= article II.13⟧:

> The three encyclopediae confirm that no systematic effort to develop notations or to extend the range of symbolic calculation characterizes the mid-century Italian abbacus environment − not even among those masters who, like Benedetto and the compiler of Palatino 573, reveal scholarly and Humanist ambitions […]. The experiments and innovations of the fourteenth century – mostly, so it seems, vague reflections of Maghreb practices – had not been developed further. In that respect, their attitude is not too far from that of mid-fifteenth-century mainstream Humanism.

As Humanism, the character and use of notations underwent some changes toward the end of the century – and not only as a consequence of printing (the notational innovations are also found in manuscripts, and sometimes they are more thorough there).

17 Benedetto's multiplication of $(1\rho - \sqrt{[13\frac{1}{2} - 1c]})$ by $(1\rho + \sqrt{[13\frac{1}{2} - 1c]})$. Redrawn after the autograph Siena, Biblioteca Comunale degli Intronati, L.IV.21, fol. 455r.

Firstly, the use of abbreviations becomes more systematic, and there is some exploration of alternative systems; secondly, the character of the sequence of powers as a geometric series is taken note of more often, and the sequence of powers is linked to the natural numbers. Sometimes the numbering coincides with our exponents, but the most influential work – Luca Pacioli's *Summa* – makes the unfortunate choice to count *number* as level 1, and *cosa* as level 2 (etc.). In consequence, it still asks for thinking to see that an equation involving (for example) *censi di censo, censi* and *numero* is simply a quadratic equation with unknown *censo*.[18]

We still find schemes for multiplication of binomials, sometimes like those of Dardi, sometimes similar to Benedetto's, and also symbolic marginal calculations similar to what Benedetto and his contemporaries had offered – but hardly anything that goes beyond them.

We may jump – in time as well as socially, namely to a scholar treating in Latin of abbacus mathematics *von höheren Standpunkt aus* – to Cardano's *Practica arithmetice, et mensurandi singularis* from [1539]. Here, we find not only indented marginal schemes (in Benedetto style) but also compact writings in the running texts – a very simple case is the statement (C vii^r) that "ducendo ℞.8 ad ℞℞. fit ℞℞64" ("reducing root of 8 to root of root makes root of root of 64"); somewhat more complex (D i^r) "1.co.p̃. $\frac{1.men.\ 1.co.}{1.ce.piu.1.}$", meaning "1 $cosa + \frac{1-cosa}{1censo+1}$". "Plus", we observe, may appear both as p̃ and as *piu.* As we shall see below, the use of the parenthesis function is even less systematic in Cardano's *Ars magna* from 1545. It is doubtful whether this can have assisted symbolic operations, and even whether it has supported thought better than full writing (as the marginal schemes indubitably do, but only for the addition, subtraction and multiplication of binomials, which they had always served).

Tartaglia's *Sesta parte del general trattato* from [1560] is not very different in its use of notations: there are schemes for the operations on binomials, still in Benedetto's style (trinomials are treated stepwise, the *a casella* scheme for polynomials from the *Tratato sopra l'arte della arismetricha* seems to have been forgotten). In the running text, too, we find formal fractions like $\frac{240ce.\ men\ 48000}{1ce.\ p\ 14co.\ p\ 60}$ (fol. 23^r)[19] and other expressions using abbreviations – but nothing with suggests thought supported by symbolic operations.

Michael Stifel, in the *Arithmetica integra* [1544], as already Christoph Rudolff in the *Coss* [1525], use the modern symbols +, – and $\sqrt{}$, but makes no further changes.

Noteworthy innovations are to be found in the works of Chuquet and Bombelli, but since these innovations are central to our topic we shall deal with them below.

[18] In contrast, the manuscript Modena, Bibl. Estense, ital. 578 (a copy from c. 1485 of an earlier but probably not much earlier original), whose numerical *gradi* coincides with exponents, classifies higher-degree equations according to the quadratic equations to which they correspond, and apparently feels no need to waste words on the matter.

[19] Actually, the *p* standing for *piu* is encircled.

Powers

Let us now return, not so much to embedding as a mere fact as to the willingness to *think in terms of* embedding. This willingness is revealed by the ways in which higher powers were named.

Diophantos introduces these terms for the powers of the unknown [ed. Tannery 1893: I, 2–6]:[20]

ἀριθμός (first power)
δύναμις (second power)
κύβος (third power)
δυναμοδύναμις (fourth power)
δυναμόκυβος (fifth power)
κυβόκυβος (sixth power)

Obviously, juxtaposition here means multiplication. Nothing in the grammatical construction would suggest otherwise, the nouns are glued together in the standard way to make compositions.

Arabic algebra is very similar. A systematic exposition was given by al-Karajī in the *Fakhrī* [Woepcke 1853: 48]:

jidhr or *šay*’ (first power)
māl (second power)
kaʿb (third power)
māl māl (fourth power)
māl kaʿb (fifth power)
kaʿb kaʿb (sixth power)
māl māl kaʿb (seventh power)
kaʿb kaʿb (eighth power)
kaʿb ka’b kaʿb (ninth power)

“and so on, until infinity”; indeed, the system allows naming of all powers. Juxtaposition once more stands for multiplication. Grammatically, the connection between the nouns is a genitive, but the Semitic genitive does not, like its Indo-European namesake, necessarily imply a subordination. As we shall see, the use of the Latin and Italian genitive was in the long run to enforce a reading of “the cube *of* the cube” as the ninth power – that is, in modern terms, an understanding of the cube as *a function*, not as an entity.

Only in the long run, however. The Latin translations of al-Khwārizmī’s algebra have no names for powers beyond the second (various biquadratics and other easily reducible

20 Hippolytos refers to the same sequence and names in his *Refutation of all Heresies,* I.2.10 and IV.51.8 ed. [Wendland 1916: 6, 75]. Since Diophantos speaks of the terms as “having been approved” (ἐδοκιμάσθη), this is hardly astonishing.

higher-degree equations are reduced without names being given to the higher powers, and such names therefore do not turn up in the problems). The *Liber mahameleth* refers twice to the *cubus,* explains the first time that the *cubus* is the product of the *census* and its root [ed. Vlasschaert 2010: 338, 363], but goes no further in the sequence.

Fibonacci, however, does. He does not explain the names nor *a fortiori* the whole sequence, but in the *Liber abbaci* he makes use of those which he needs [Boncompagni 1857: 447*f*, 450*f*, and *passim*]. Here we see that the sixth power may be *cubus cubi* as well as *census census census* (this equivalence *is* stated on p. 447, and the latter expression seems to be his standard; we may guess that he follows an Arabic model), while the eighth power is *census census census census.* As we see, he uses the Latin genitive in the Arabic way.[21]

The early abbacus algebras – for instance, Jacopo – go no further than the fourth power, which is *censo di censo* (while the third power is *cubo).* Since 2+2 = 2×2, they can tell us nothing about conceptualizations.

The earliest abbacus writer known to go beyond this boundary is a certain Giovanni di Davizzo. A manuscript written in 1424 (Vatican, Vat. lat. 10488) contains seven pages claimed to be copied from a treatise written in 1339 by him; since the style (use of abbreviations, etc.) is wholly different from what comes before or after, we can probably trust the faithfulness of the copying. The interesting part for our present discussion [ed. Høyrup 2007b: 479–481] first gives rules for the multiplication of powers, some of which show the thinking to be multiplicative in spite what might be suggested by the grammar:

> and thing times censo makes cube
> and cube times cube makes cube of cube
> and censo times cube makes censo of cube.

Then follows something which will wring the bowels of any modern mathematician – a daring but mistaken attempt to express negative powers, namely confounding them with roots (the first negative power stated to be "number"):[22]

> And know that dividing number by thing gives number
> and dividing number by censo gives root
> and dividing thing by censo gives number
> and dividing number by cube gives cube root
> and dividing thing by cube gives root
> and dividing censo by cube gives number

[21] In the *Pratica geometriae* [ed. Boncompagni 1862: 207] *census census* and *cubus cubi* are used in the same way, pp. 214–216 *census census census* and *census census census census*.

[22] Along with a number of false solutions to cubics and quartics, this system survived until Bento Fernandes' *Tratado da arte de arismetica* from 1555, see [do Céu 2008]. Maria do Céu's attempt (p. 9) to save the system mathematically is ingenious but disagrees completely with the words and the structure of the various texts that state these rules (and obviously never use them).

and dividing number by censo of censo gives root of root
and dividing thing by censo of censo gives cube root
and dividing censo by censo of censo gives root
and dividing cube by censo of censo gives number
and dividing number by cube of cube gives cube root of cube root
and dividing thing by cube of cube gives root of cube root
and dividing censo by cube of cube gives root of root
and dividing cube by cube of cube gives cube root
and dividing censo of censo by cube of cube gives root
…

It may not be warranted to look for anything in the text beyond a play with words, but we can still try to take it as seriously meant, and suppose that Giovanni's "roots" in this context are intended to be the same as those he speaks about in the first part of the excerpt (which are those of everybody else). Under these conditions we see that even his roots are supposed to be composed "multiplicatively" (whatever can have meant by that) – for instance, that the cube root of the cube root is the sixth, not the ninth root. Similarly, the *Trattato dell'alcibra amuchabile* [ed. Simi 1994: 48] takes *radicie de radicie chubica* to be the fifth − but since this is once again in a messy context where no calculation is performed, the compiler has no reason to discover that his rule is absurd.

Dardi knows better. His names for the *powers* are still in the Arabic style, and he even explains like Fibonacci that *Ç di Ç di Ç* is the same as *cubi di cubi* (fol. 43^{r}). Since most of his problems involve radicals (in the style of "roots of cubes"), he gives us the occasion to observe that he understands roots as functions, and that repeated root taking thus involves embedding – expressing for example (fol. 95^{r}) the 12th root as ℞ *cuba de* ℞ *de* ℞ *overo* ℞ *de* ℞ *de* ℞ *cuba* ($\sqrt[3]{\sqrt{\sqrt{a}}}$ or $\sqrt{\sqrt{\sqrt[3]{a}}}$), while his term for the 12th power would be *cubo di cubo di cubo di cubo.* But this terminological insight and innovation has a price: Dardi has no name for the fifth and the seventh root, and once replaces the former by ℞ *cuba* (fol. 97^{v}), and once the latter by ℞ *dela* ℞ (fol. 98^{r}), thus ending up with mistaken rules – cf. [Van Egmond 1983: 417]. In spite of his manifest command of the sequence of powers, he is at the frontier of what he can express.

Toward the end of the 14th century, the frontier had moved, and the consequences of the genitive construction made themselves felt – but as yet inconsistently.

The manuscript Palatino 573 (one of the three "abbacus encyclopedia" mentioned above) quotes Antonio de' Mazzinghi for the following [ed. Arrighi 2004: 191]:

> *Cosa* is here a hidden quantity; *censo* is the square of the said *cosa; cubo* is the multiplication of the *cosa* in the *censo; censo di censo* is the square of the *censo* [*quadrato del censo*], or the multiplication of the *cosa* in the *cubo.* And observe that the terms of algebra are all in continued proportion; such as: *cosa, censo, cubo, censo di censo, cubo relato, cubo di cubo,* etc.

As we see, Antonio avoids speaking of the fifth power as *cubo di censo* or *censo di cubo,* introducing instead a neologism; but his naming of the sixth power is still multiplicative, and when he suggests an understanding of the fourth power through embedding the name for the function is *square,* not *censo*. The name for the fifth power may have been inspired by his term for the fifth root, appearing in a problem about composite interest [ed. Arrighi 1967: 38] as *radice relata.*

The extensive algebra section of the *Tratato sopra l'arte della arismetricha* (Florence, c. 1390, see above) – also from the hand of a highly competent algebraist – starts by explaining how the powers are produced one from the other, and that they are in continued proportion [ed. Franci & Pancanti 1988: 3–5]. One particularity is an extra identification of these as "roots", namely (as explained) as the roots which they have.[23] Taking this into account, the sequence is

cosa (first power)
censo or *radice* (second power)
cubo or *radice cubica* (third power)
censo di censo or *radice della radice* (fourth power)
cubo di censi or *una radice che nascerà d'una quantità quadrata chontro*
  a una quantità chubicata or (some say) *radice relata* (fifth power)
censo di cubo (sixth power)

For the sixth power it is stated (but not properly given as a name) that one may take the root, and of this quantity take the cube root. The author thus recognizes the embedding of the taking of roots, and transfers this to the name *censo di cubo,* corresponding to our $(x^3)^2$; this, however, does not force him to give up the multiplicative name for the fifth power, also identified as "a root born from a squared root multiplied against a cubicated quantity". The name *radice relata* ascribed to "some", we observe, coincides with the name for the fifth root used by Antonio.

Benedetto as well as the compiler of Palatino 573, both of whom copy long extracts from Antonio, also take over his naming in their independent chapters. The third encyclopedia instead (Ottobon. lat. 3307) uses both *cubo di censo* and *censo di cubo* about the fifth power; the intervening 70 years have thus not witnessed any steps beyond the inconsistencies of the late 14th century.

There is some – though not yet really exhaustive – change toward the end of the 15th century. Above, the algebra of the Modena manuscript Bibl. Estense, ital. 578 was mentioned for its use of *gradi* coinciding with our exponents. It also uses the "root names" for the powers, and the abbreviations *C*, *Z* and *Q* for *cosa, censo* (thought of in the North Italian orthography *zenso*) and *cubo* – in the running text (and once in the scheme below),

[23] "tanto vol dire uno censo quanto dire una quantità ch'à radice" (p. 3); "questa quantità di nome che produce radice relata" (p. 5).

however, *censo* is represented by a variant of Dardi's *Ç*.[24] The whole sequence (fol. 5^{r}) is then abbreviated

N (power zero)
C (first power)
Z (second power)
Q (third power)
ZÇ (fourth power)
$C\bar{d}ZZ$ (fifth power)
ZdiQ (sixth power)
$C\bar{d}ZdQ$ (seventh power)
$Z\bar{d}ZZ$ (eighth power)
$Q\bar{d}Q$ (ninth power)

Since, as always, 2+2 = 2×2, we cannot decide the principle according to which the name for fourth power is formed; the fifth, however, is clearly formed from the fourth as a multiplication, whereas the 6th is based on embedding. The seventh is based on mixed principles, the eighth and the ninth on pure embedding.

On fol. 5^{v}, a new scheme gives the corresponding root significations:

C: *egli che trovi* ("that which you find").
Z: *la R. di quello* ("the root of that").
Q: *la R. quba di quello* ("the cube root of that").
ZÇ: *la R. di R. di quello* ("the root of the root of that").
$C\bar{d}ZZ$: *la sua R. di quello* ("its root of that").
ZdiQ: *la sua R. de la Rq. di quello* ("its root of the root of that").
$C\bar{d}ZdQ$: *la 7^{a} R. di quello* ("the 7th root of that").
$Z\bar{d}ZZ$: *la R. di R. di R. di quello* ("the root of the root of the root of that").
$Q\bar{d}Q$: *la RQ di la RQ di quello* ("the cube root of the cube root of that").

As we see, there is a strong coupling between the roots that are expressed via embedding and the corresponding powers. The seventh, irreducible root is referred to with this name, whereas the fifth root is unspecified.[25] All in all, a preliminary conclusion suggests itself: Namely that the much more obvious embedding of roots is what started enforcing also the view of power-taking as an embedding (that is, as a function or an operation).

Raffaello Canacci's *Ragionamenti d'algebra* from c. 1495 has idiosyncratic names for some of the higher powers:

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24 Namely, ç with a much enlarged cedilla – of interest only because Jacques Peletier also uses it in [1554], which shows him to know not only Stifel, Pacioli and Cardano but also at least part of the manuscript tradition.

25 It is possible that the copyist has misread "5^{a}" as "sua".

numero (power zero)
chosa (first power)
censo (second power)
cubo (third power)
censo di censo (fourth power)
chubo di censo (fifth power)
relato (sixth power)
promico (seventh power)
censo di censo di censo (eighth power)
chubi di chubi (ninth power)
relato di censo (tenth power)

The fifth power is thus named according to the multiplicative principle, but the eighth and ninth by embedding (the Modena manuscript does the same, but not in the same way for the fifth power). The name for the sixth power is the one others use for the fifth power, and that for the seventh is even more astonishing, and in absolute conflict with normal usage.[26] The name for the tenth power falls outside both systems (but see imminently).

Canacci also experiments with graphic notations for the powers – *censo* is a square, *cubo* a vertically divided rectangle, *censo di censo* two separate squares, his *relato* a horizontally divided rectangle, his *promico* a horizontally divided square. Their compositions emulate those of the names. ⟦The system can be seen in article II.13.⟧

According to Francesco Ghaligai [1521: 71ʳ], almost the same names and graphic signs had been used by Giovanni del Sodo (Canacci's teacher) in his algebra, with the extension that the 11th power was *tromico*, and the 13th was *dromico*. But del Sodo, according to Ghaligai, used *relato* about the fifth power, and named the sixth power with embedding, as *cubo di censo*. In this system, *relato di censo*, understood as embedding, is really the tenth power. Del Sodo's system is thus consistently based on embedding, although his graphic notation must be characterized as unhandy. Canacci's inconsistencies, it turns out, must be traced back to deficient understanding of his model (his rules for multiplication of powers [ed. Procissi 1954: 433] confirms this). However, such misunderstandings on the part of an otherwise competent abbacus writer shows that del Sodo's way to think was not yet commonplace, nor central to mathematical practice – concepts that are appropriated through use are not mixed up like this by trained practitioners.

26 *Pronic numbers* are numbers of the form $n \cdot (n+1)$, and the pronic root is related in other authors to this concept, though not always in the same way. According to Pacioli [1494: I, 115v], the pronic root of 84 is 9, because $(9^2+\sqrt{9}) = 84$, while Gilio [ed. Franci 1983: 18*f*] as well as Muscharello [ed. Chiarini et al 1972: 163] state that it is 3. Benedetto [ed. Pieraccini 1983: 26] suggests without being quite clear that the pronic root of 18 is 4, which would agree with Pacioli $(4^2+\sqrt{4} = 18)$.

In Pacioli's Perugia manuscript from 1478 [ed. Calzoni & Gavazzoni 1996], those 25 sheets are missing where a systematic presentation of the powers would be expected according to Pacioli's own table of contents. Since the problems do not deal with powers beyond the fourth, we can only see that *cosa, censo, cubo* and *censo di censo* are represented by superscript ^{co}, $^{\square}$, $^{\triangle}$ and $^{\square\square}$, from which we can conclude nothing.

We may look instead at his *Summa* from [1494], which uses a different notation (plausibly because superscripts were not possible or acceptable for his printer, who seems to have counterbalanced Pacioli's loquacity by reducing line distances to an absolute minimum). Here, fol. 67^{v} [27] lists the 30 *gradi* of the "algebraic characters" or *dignità* (as he says they are called):

℞ 1^{a} n^{o}. *numero* (power zero)
℞ 2^{a} *co. cosa* (first power)
℞ 3^{a} *ce. censo* (second power)
℞ 4^{a} *cu. cubo* (third power)
℞ 5^{a} *ce.ce. censo de censo* (fourth power)
℞ 6^{a} $p^{o}.r^{o}$ *primo relato* (fifth power)
℞ 7^{a} *ce.cu. censo de cubo e anche cube de censo* (sixth power)
℞ 8^{a} $2^{o}.r^{0}$. *secundo relato* (seventh power)
℞ 9^{a} *ce.ce.ce. censo de censo de censo* (eighth power)
. . .
℞ 29^{a} *ce.ce.2°.*r^{o}. *censo de censo de secundo relato* (twenty-eighth power)
℞ 30^{a} *[9°]r°. nono relato* (twenty-ninth power)

Everywhere, composition means embedding, and the prime powers are designated as 1st, 2nd, 3rd, 4th, … 9th *relato.*

So, with del Sodo and Pacioli, embedding-composition has become the sole principle. Taking a power, in other words, has become *an operation,* and the power itself more or less a function. And in the Modena manuscript as well as Pacioli, the members of the sequence are identified arithmetically.

Chuquet's *Triparty des nombres* [ed. Marre 1880] from 1484 was more radical. Dropping all names, Chuquet simply wrote the exponent of the power superscript after the coefficient. This was *too* radical, at least in the opinion of Étienne de la Roche, whose *Larismethique nouvellement composee* from [1520], based to a large extent on Chuquet's work and the only channel through which Chuquet's ideas reached the wider world, turns instead to the notations that were becoming current in German algebra at the time, for instance in Rudolff's *Coss* (ultimately going back to the Florentine notations of the 15th century) − see [Moss 1988], in particular the comparison between Chuquet's manuscript and de la Roche's corresponding text on p. 122.

[27] Repeated on fol. 143r within a more complicated structure.

Rudolff [1525: D ii^{v}] offers this sequence:

dragma oder numerus (power zero)
radix (first power)
zensus (second power)
cubus (third power)
zensdezens (fourth power)
sursolidum (fifth power)
zensicubus (sixth power)
bissursolidum (seventh power)
zenszensdezens (eighth power)
cubus de cubo (ninth power)

– also based on embedding, but with new terms for the prime powers, obviously invented in a Latinizing environment (*sursolidum/supersolidum* might be related to Antonio's *cubo relato* – a cube, after all, *is* a solid). ⟦[28]⟧

The same powers and graphic symbols are given by Stifel in the *Arithmetica integra* [1544: 234^{v}−235^{r}] − but Stifel goes on until the 16th power (after *zensocubicus* only with graphic symbols). Similarly, Tartaglia, in the *Secunda parte del general trattato de numeri e misure* [1556: 73^{r}] repeats Pacioli's list – and again in the *Sesta parte* [1560: 1^{r}], though stopping here at the 14th power because one very rarely needs so high powers (but pointing out in both volumes that one may go on *in infinito*).

Bombelli, in the manuscript of his *L'algebra,* uses an arithmeticized notation with indication of the power written above the coefficient – for instance, $\overset{\underset{\smile}{1}}{30}$ for "30 *cose*" [Bortolotti 1929: 21], which in the printed version would become $30\underset{\smile}{1}$. In the beginning of book I [Bombelli 1572: 1–3], however, he explains the terms which we know from Pacioli and Tartaglia – though only until *numero quadrocubico, over cubicoquadrato.* As Tartaglia in the *Sesta parte* he obviously sees no purpose in discussing, for the sole reason that they can be given a name, powers that are of no use in his work.

All in all, the insights in this domain that had been reached by del Sodo and Pacioli in the late 15th century were conserved and systematized but not superseded during the following century.[29] But how could they be without an explicit parenthesis function

28 ⟦His corresponding graphical symbols look as follows:

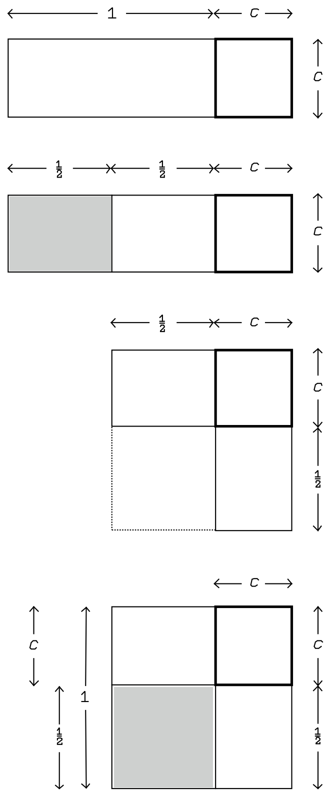
⟧

29 Nor had they totally superseded the multiplicative understanding. Viète's *In artem analyticen isagoge* [1591: 4^{v}] still speaks of the fifth power as *quadrato-cubus* and of the sixth as *cubo-cubus*. The way the compositions are formed, with *-o-*, suggests that this was a conscious return to Diophantine manners, away from the "filthy jargon" of current algebra (*à barbaris defoedata et conspurcata*, as formulated by Viète in the dedicatory letter. p. A ii^{v}). ⟦Actually, the terms are used in Xylander's translation of Diophantos [1575: 2], although Xylander also uses

allowing the automatization and apparent trivialization[30] of such insights as $x^{(mn)} = (x^m)^n = (x^n)^m$? We shall therefore now turn our attention to the algebraic parenthesis.

The parenthesis before and until the brackets

Once upon a time there was a "Babylonian algebra". It was discovered (or invented) around 1930, but over the last three decades I believe I have managed to convinced most of those who work seriously on the topic that the numbers found on the tablets and supposed to reflect algebraic operations correspond instead to the measures of geometric entities manipulated in a cut-and-paste technique (whether this technique can then be characterized as "algebraic" is a matter of taste or definition). For instance, let us look at a literal translation of the very simplest second-degree example – the first problem on the tablet BM 13901, a "theme text" from around 1700 BCE about squares:[31]



1. The surface and my confrontation I have heaped: $\frac{3}{4}$ is it. 1, the projection,

2. you posit. The moiety of 1 you break, $\frac{1}{2}$ and $\frac{1}{2}$ you make hold.

3. $\frac{1}{4}$ to $\frac{3}{4}$ you join: by 1, 1 is equal. $\frac{1}{2}$ which you have made hold

4. from the inside of 1 you tear out: $\frac{1}{2}$ the confrontation.

A "confrontation" is the side of a square (which "confronts" its equal), the "moiety" is a "natural half', that is, a half whose role could not be filled by any other fraction. To "make *a* and *b* hold" stands for the construction of a

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quadratiquadratun.⟧

Oughtred [1648: 35*f*] takes over this multiplicative understanding, now also in symbols (*quadrato-cubun* becoming qc ⟦even this is found in Xylander, though in a scholion⟧). Since he uses juxtaposition in the modern way, as multiplication, this in also unobjectionable. Still, none of the two has the idea that powers could be functions.

30 Apparent! Cf. [Weil 1978: 92], where exactly this is discussed.

31 Borrowed from [Høyrup, forthcoming]. Since the Babylonian sexagesimal place value system is immaterial for the present discussion, I translate the numbers.

rectangle with sides a and b, and that s "is equal by" A means that s is the side of the area A laid out as a square. The "projection" gives the clue to the method: at first the side or "confrontation" c is provided with a "projection", a breadth 1, which transforms it into a rectangle with area $1 \times c = c$. Then, according to the statement, this rectangle, together with the square $\square(c)$, has a total area $\frac{3}{4}$. Breaking it into two equal parts and moving one of them around we get a gnomon, still with area $\frac{3}{4}$, which is completed by a square of area $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$. The completed square has an area $\frac{3}{4} + \frac{1}{4} = 1$ and therefore a side $\sqrt{1} = 1$. Removal of the part which was added below leaves us with the original side, which must hence be $1 - \frac{1}{2} = \frac{1}{2}$.

This technique seems to leave no space for anything like a parenthesis, and at this level the immediate impression holds true. However, the technique may be used for "representation", that is, the sides of square and rectangular areas may themselves be areas, volumes, numbers of working days or bricks produced during these days, prices, etc. In the particular case where the sides of a rectangle are two square areas, we may describe the solution as making use of an *implicit* parenthesis – as when Fibonacci [ed. Boncompagni 1857: 447] solves a bi-biquadratic problem by treating the *census census census census* as a square area and the *census census* as its side.[32]

The Babylonian texts also present us with an *explicit* parenthesis function, though (in the likeness of the parenthesis demarcated by a fraction line, see imminently) only used in very specific contexts. They make use of two different subtractive operations, *removal* and *comparison*.[33] For an entity b to be removed from another entity B (for instance, but there are synonyms and almost-synonyms, by being "torn out"), b has to be a part of B. An entity A that is no part of B obviously cannot be removed from it, but instead the text may state by how much B exceeds A. In the former case, the operation produces an entity that can be subjected to the usual geometric operations. In the latter, however, only few texts see the excess as an independent quantity that can be directly manipulated, for instance by constructing a square with the excess as side (making the excess "confront itself"). The majority would instead make "*so much as* that by which B exceeds A" confront itself. The phrase "so much as" (translating *mala,* a single word) thus defines a parenthesis.[34] However, the use of this parenthesis is not general, and

[32] However, Fibonacci takes care here not to identify this square with another *census,* but uses *Elements* II.6 – he is not quite as close to an implicit parenthesis as is the Babylonian text. On p. 422, it is true, a *census* is re-baptized *res* – but it is not clear whether that *census* is meant as an algebraic second power or just renders the original Arabic meaning of *māl,* an amount of money.

[33] This is actually a simplification – [Høyrup 1993] provides some shades; but it is a close approximation, and sufficient in the present context.

[34] It is also used in slightly different ways – for example, "So much as I have made confront itself, and 1 cubit exceeding, that is the depth" (namely of an excavation with square base). ⟦Here, the

like implicit parentheses (Babylonian, or Fibonacci's) it cannot be nested without strain on thought. Within the kind of mathematics that was practised (by the Babylonian calculators, or by Fibonacci) it is also dubious whether any use for such nesting would easily present itself.[35]

Because of the tails with which the superscript symbols for powers and root were provided in the Maghreb notation, these symbols may serve to delimit parentheses – see examples in [Abdeljaouad 2002: 23–46]. The argument of a root sign may be a complex expression, and may even itself contain roots (nesting). Inverse taking may have an algebraic monomial as its argument, and it may even be repeated; but the notation is ambiguous as regards the coefficient (will it produce $5 \cdot x^{-1}$ or $(5 \cdot x)^{-1}$?). Division written fraction-wise may contain algebraic polynomials in the numerator as well as the denominator.

The situation concerning the symbols for powers is different. Here, the argument (which is actually the coefficient) may be an integer or a broken number or even an arithmetical composite, but nothing else. Number, *šay'*, *māl* and *ka 'b* have individual signs; higher powers are written as composites either horizontally or vertically, but the meaning will always be multiplicative (as in al-Karajī's verbal list of their names) − the sign for *māl* and *ka 'b* written together will always stand for *māl ka 'b* (the fifth power), never for $(x^3)^2$. In other words: *šay'*, *māl* and *ka 'b* are *entities,* not functions or operations, as are the root sign and the division written fraction-wise.

All in all: at least in its mature phase the Maghreb notation comprised a fairly well developed parenthesis function – certainly more fully developed than anything that can be found in Europe before and even including Viète; but like Viète it stopped short of the point where it could be used for free symbolic manipulation.[36]

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parenthesis is rather linguistic.⟧

[35] One example comes to my mind – problem #3 of the text TMS IX [Høyrup 2002: 91–95]. Here, the entities at the first level are given new names (the original sides of a rectangle augmented by 1); at the second level, no such names are introduced, the unknown at this level are simply understood to be 3 respectively 21 times those of the first level. All in all, two-level embedding is thus eschewed.

[36] As we have seen, even Viète's powers are entities, not functions allowing nesting; his copious use of proportion techniques would also make the use of nested expressions almost as difficult as in the Indian notation. And like Bhaskara II, he steps outside symbolic calculation when needing to operate with complex expressions, as for instance in *Ad logisticen speciosam notae priores,* prop. 41 [ed. van Schooten 1646: 32]:

> Sit radix binomia A+B, sublaterale coëfficiens D planum. Effingendum sit solidum sub A+B, &D plano, adfectum multa cubi ex A+B. Ducatur A+B in D planum multatum A+B cubo. Orientur solida, A in D planum, +B in D planum, – A cubo, – A quadrato in B 3,– A in B quadratum 3,– B cubo.

As translated by Witmer [1983: 64]:

In Latin (that is, Romance and Germanic) Europe, as we have seen, powers remained entities until the mid-15th century; even for del Sodo, Pacioli etc., who consistently named higher powers by embedding, it was still impossible to use their names as operations on other entities than powers of the unknown.

Formal fractions carrying a binomial in the denominator were in use from the 14th century, as we have seen; in the 15th century, trinomials also appear occasionally. In the beginning, however, this development was stymied by the predominant understanding of the fraction line as an indication of ordinality and not of division; no wonder, perhaps, that this borrowing from the Maghreb took a long time to get established.

For roots, the sign **R̸** came in use before 1340 (Giovanni di Davizzo used it in 1339, and Dardi in 1344). The cube root, however, was written **R̸** *cubo,* and roots of composite expressions also had to be designated "**R̸** *de zonto*" (Dardi), ℞ by Gilio (who may have taken it over from his master Antonio [Franci 1983: xxiii]), and also by Benedetto; and **R̸** *legata* or **R̸**u (*u* for *universale* or *unita)* by Pacioli and Cardano. Mostly, but not consistently, this root was to be taken of a binomial; Cardano, moreover, might use **R̸**u of a binomial as the sum of the two roots (**R̸**u(a+b) = $\sqrt{}a+\sqrt{}b$) – see the survey of his notations in [Tamborini 2011: 57]. That is, **R̸**u is no symbol proper but only an abbreviation, whose meaning must be understood from context (as current in manuscript abbreviations, where a stroke over a vowel might mean that either *m* or *n* was to follow, and where the same abbreviation might stand for *phisice* as well as for *philosophice*).

So, in spite of the original access to inspiration from the Maghreb and to the enduring use of the algebraic parenthesis defined by the fraction line, the obvious *need* for an unambiguous way to take roots of polynomials, that is, for a delimitation of the radicand as a parenthesis, was only answered by Chuquet, who used the simple trick to underline the radicand – see for example [Marre 1880: 734 and *passim*]. As far as I have noticed, he does not use the notation for other purposes, and it is never nested. De la Roche may have found the innovation superfluous.

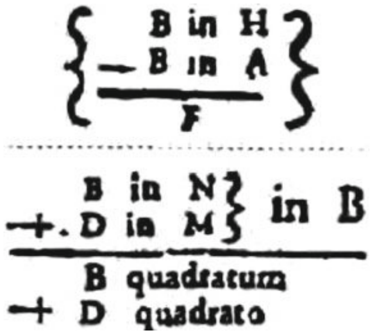
For the root of binomials, Bombelli still uses *Radice legata* or *Radice universale,* as he explains [1572: 98*f*]. Longer radicands (and sometimes also binomials, for instance on p. 106), are delimited by an initial L. and a final inverted ⅃; sometimes, the system is nested (but always with each parenthesis being a radicand). The lack of system indicates that the purpose is disambiguation and nothing more. Bombelli's manuscript, however, goes somewhat further: the whole radicand is underlined, and the beginning and the end of the line are marked by vertical strokes – see [Bortolotti 1929: 6].

Let $A + B$ be a binomial root and D^p the coefficient of its first power. The solid from $A + B$ and D^p, and affected by the subtraction of the cube of $A + B$, is to be constructed. Multiply $A + B$ by D^p minus the cube of $A + B$. There then arise these solids: $AD^p + BD^p - A^3 - 3A^2B - 3AB^2 - B^3$.

In *La seconda parte del general trattato,* Tartaglia had already used round brackets occasionally to delimit universal roots without explaining – but only when it turns out to be needed for disambiguation, namely in the exposition of how to operate with several universal roots, or with universal roots and numbers [1556: 167$^{\mathrm{v}}$]. Occasionally, only the initial bracket is present. Beyond that, Tartaglia makes generous use of round brackets as punctuation (as already Pacioli had done, yet without getting the idea to borrow them for algebraic purposes).

So, where does the *general* algebraic parenthesis begin? Not yet with Viète. In *Ad logisticen speciosam notae priores*, prop. 53 [ed. van Schooten 1646: 38], what we would write $B^2+(Z+D)^2$ is expressed with a rhetorical parenthesis as "B quadrato, + quadrato abs Z+D" – but in the accompanying diagram it appears as "$Z+D^q+B^q$", where ^{q} stands for *quadratum.* This is at least as ambiguous and just as context-dependent as Cardano's ℞u.[37]

What then about Descartes?



Viète's suggested parentheses
see note 37

[37] In *Zeteticorum libri V* [Viète 1593], a notation is occasionally used which certain 17th century readers would understand as an algebraic parenthesis. Inspection of the text shows that this was not Viète's intention. Curly brackets, or a single brace, serve to indicate that an expression going over several lines is meant to kept together typographically rather than mathematically. So, on p. 3$^{\mathrm{r}}$ we find the upper expression in the adjacent diagram (translated $\frac{BH-BA}{F}$ in [Witmer 1983: 93]); van Schooten [1646: 45] sees that there is no need for specification of a parenthesis – the fraction line suffices – and writes $\frac{B\,in\,H,=B\,in\,A}{F}$. Vasset [1630: 50] and Vaulezard [1630: 38] offer something very similar in their translations.

In [Viète 1593: 18$^{\mathrm{r}}$] we see that a sole right brace can be used in the same function, and once again van Schooten (p. 70), Vasset (p. 141) and Vaulezard (p. 166) simply write numerator and denominator on a single line each. In [Viète 1593: 17$^{\mathrm{r}}$] we see that a single right brace may also stand along the numerator alone – and even here, van Schooten (p. 69), Vasset (p. 138) and Vaulezard (p. 162) simply write the numerator in one line.

In [Viète 1593: 15$^{\mathrm{r}}$], on the other hand, we find something which the 17th-century editors and translators would see differently, even though nothing in Viète's original text suggests he saw any difference – namely the lower expression in the diagram. In this case, van Schooten (p. 65) and Vaulezard (p. 139) conserve the bracket, seeing that it has a function. Vasset (p. 125) conserves the bracket but locates it after the fraction (containing both numerator and denominator), where it is actually superfluous; but he must understood it as having a function in Viète's text.

It is tempting to see this reinterpretation of Viète's text as a reflection of a "cognitive pull" produced by the development of 17th-century mathematics: once the need for a more general parenthesis function was there, it made Vasset and Vaulezard read it into the text under their eyes (van Schooten is a different and less significant case: he was close to Descartes [van Randenborgh 2012], and he wrote after 1637).

Firstly, of course, Descartes has the modern, long square root $\sqrt{}$, which can also be nested – for instance $\sqrt{-\frac{1}{2}a+\sqrt{\frac{1}{4}aa+bb}}$ [ed. Adam & Tannery 1897: VI, 375]. Next, he uses complex expressions involving multiple parentheses, as in this equation (p. 398):

$$yy \propto \frac{\left.\begin{array}{l}-dekzz\\+cfglz\end{array}\right\} y \quad \left.\begin{array}{l}-dezzx\\-cfgzx\\+bcgzx\end{array}\right\} y \quad \left.\begin{array}{l}+bcfglx\\-bcfgxx\end{array}\right\}}{ezzz-cgzz}$$

As we see, the parentheses are not enclosed in pairs of brackets, but written vertically and kept together by a brace to the right; but that is immaterial as long as they are unambiguous.[38] We also notice that Descartes prefers to write second powers as *yy*, even though he writes y^3 (etc.), as in this expression (p. 420):

$$y^6-2by^5\left.\begin{array}{l}-2cd\\+bb\\+dd\end{array}\right\}y^4\left.\begin{array}{l}+4bcd\\-2ddv\end{array}\right\}y^3\left.\begin{array}{l}-2bbcd\\+ccdd\\-ddss\\+ddvv\end{array}\right\}yy-2bccddy+bbccdd$$

but that, again, is a different question (we too, when dealing with angles, may write $2°23'12''25^{(3)}$, and similarly differentiate sequentially as f(x), f'(x), f''(x), $f^{(3)}(x)$, …).

Descartes does not use these parentheses very much, but they are there. And as Engels [1962: 496] states in *Dialektik der Natur,* "100,000 steam engines [prove the principle] no more than one"; or, at least, in a formulation ascribed to Ulrich von Wilamowitz-Moellendorf, "according to the philologists, once is never, twice is always". Nor did mathematicians of the following generation use Descartes' invention very much.[39] In the *Arithmetica infinitorum* from [1656], for instance, Wallis has many complex expressions

[38] It is also immaterial that the brace had already been used by Viète in a different function – but perhaps more significant that Vaulezard has changed the use of Viète's notation into something close to what Descartes was doing.

[39] In Descartes' own generation there were many who saw no great promises in the expanded use of symbols.

[Oughtred 1648] was already mentioned. Inasfar as embedding is concerned, Oughtred does not go beyond Dardi or Benedetto (Oughtred's only parentheses are the fraction line, and the "universal root", which he writes $\sqrt{}$: .

Since neither Oughtred nor others go beyond what was done in earlier centuries with parentheses, there is no reason the go into depth with them in the present context, nor to discuss whether what they did would be classified as systematic syncopation or as genuine symbolization.

kept together by fraction lines; but when fractions are written with a slash on a single line and his modern translator [Stedall 2004: 140] writes

$$(\ell^5 + 10\ell^4 + 35\ell^2 + 50\ell^2 + 24\ell)/5,$$

there Wallis himself (p. 149) has

$$\ell^5 + 10\ell^4 + 35\ell^2 + 50\ell^2 + 24\ell/5.$$

In *Mechanica, sive, De motu* [1670: 394], he even uses an explanatory parenthesis inside a formula:

$$\frac{6fvR^2 - 3fs^2R(=3fv^2R) - 4as^2 - 3s^3v}{-3eR^2 + 12avR + 9svR - 6as^2 - 4s^3}$$

(similarly p. 427). When continuing a numerator or a denominator over two lines (for instance, p.411) he sees no need to indicate (as had done Viète) that the whole belongs together as one expression. This is not out of ignorance: in his correspondence with John Collins, he uses both the Cartesian brace (namely when presenting something as Descartes' solution to a problem) and round brackets when it fits [ed. Rigaud 1841: 574, 585]. But his mathematics asks for no systematic use.

A similar picture is offered by Newton. His Mathematical Notebook from c. 1664/65[40] uses few parentheses beyond the traditional types kept together by fraction lines – but one does find others (thus fol. 152^{v}), which are delimited by a vinculum. This notation is still used in his *Arithmetica Universalis* [1722: 6 and *passim*] as well as his writings on fluxions [1723: 23–25; 1736: 18] – but sparsely used, and for simple purposes.[41]

In the longer run, however, the road was open after Descartes to such fabulous calculations (to mention but this example) as Euler's development [1748: I, 257] of the infinite fractional infinite product

$$\frac{1}{(1-xz)(1-x^2z)(1-x^3z)(1-x^4z)(1-x^5z)\,\&c.},$$

as the sum

[40] MS Add. 4000, Cambridge University Library, Cambridge, UK, transcription http://www.newtonproject.sussex.ac.uk/view/texts/normalized/NATP00128 (accessed 30 June 2018).

[41] The manuscript of Newton's Cambridge lectures on algebra (1673–1683) as polished by his successor [ed. Whiteside 1972] uses the same system (at times also a Cartesian brace (thus pp. 82–86, dealing with polynomial division). Already as a young student still under Wallis's strong influence he must have seen the general parenthesis function to be important though of only occasional use, and he choose his own way, to which he stuck forever after.

$$\begin{array}{l}
1+z\,(x+x^2+x^3+x^4+x^5+x^6+x^7+x^8+x^9+\&c.)\\
+z^2(x^2+x^3+2x^4+2x^5+3x^6+3x^7+4x^8+4x^9+5x^{10}+\&c)\\
+z^3(x^3+x^4+2x^5+3x^6+4x^7+5x^8+7x^9+8x^{10}+10x^{11}+\&c.)\\
+z^4(x^4+x^5+2x^6+3x^7+5x^8+6x^9+9x^{10}+11x^{11}+15x^{12}+\&c.)\\
+z^5(x^5+x^6+2x^7+3x^8+5x^9+7x^{10}+10x^{11}+13x^{12}+18x^{13}+\&c.)\\
+z^6(x^6+x^7+2x^8+3x^9+5x^{10}+7x^{11}+11x^{12}+14x^{13}+20x^{14}+\&c.)\\
+z^7(x^7+x^8+2x^9+3x^{10}+5x^{11}+7x^{12}+11x^{13}+15x^{14}+21x^{15}+\&c)\\
+z^8(x^8+x^9+2x^{10}+3x^{11}+5x^{12}+7x^{13}+11x^{14}+15x^{15}+22x^{16}+\&c.)\\
\qquad\qquad\qquad\qquad\&c.,
\end{array}$$

without any intermediate argument – thus expecting the reader to know how to transform $\frac{1}{1-x^n z}$ into an infinite sum, and to be able to grasp how the product of this infinite product of infinite sums could be reduced to an infinite sum of infinite sums. Half a century later (and almost certainly before – I have not searched this period and level systematically) the full use of parentheses could even be presupposed at much the more elementary level represented by the general examination at Saint John's College, Cambridge, whose students were confronted in 1797 with this problem [ed. Rotherham 1852: 3]:

$$\frac{123+41\sqrt{x}}{5\sqrt{x}-x}=\frac{20\sqrt{x}+4x}{3-\sqrt{x}}-\frac{2x^2}{(5\sqrt{x}-x)(3-\sqrt{x})}.$$

Why? Why not?

Why was progress so slow, much more marked by stops than by goes, at times even by regressions? Indeed, why not?

Metaphysical absolute progress is nothing but an illusion, mistaking Ivor Grattan-Guinness's famous polemical "royal road to me" for *the* road. Within the broader practice of ocean trade, colonization and warfare, improved mathematical navigation certainly constituted progress − but from the point of view of the human chattel brought over the Atlantic or dying on the way, the characterization can be disputed. Even Nunez and Dee, however, had little use for algebra when working on navigational techniques.[42] Until their time, algebra had no social uses outside the environment of those who lived from teaching mathematics. Mathematics, of course, also has its *internal* constraints, and those (like Dardi, Antonio and Benedetto) who understood the subject well would not stoop to the false solutions of irreducible cubics and quartics or Giovanni di Davizzo's advertising of roots as inverse powers. Even they, however, used and developed algebra

[42] Regiomontanus does use some algebra in his *De triangulis* – but he needs nothing beyond simple second-degree Florentine abbacus techniques.

in view of treating *a particular kind* of problems, and for this kind of problems they had no need to develop neither symbolic operations nor embedding and parenthesis function. Personally (but this is already counterfactual history running wild), they might perhaps have enjoyed it if they had been able to foresee that developing such techniques would have made it possible for them to discover Euler's theorems about the partition of numbers (or just Descartes geometrical results). However, in the competition for pupils and prestige within the environment of abbacus teaching such things would not have been understood and therefore would not have counted, and in any case it is in the nature of dialectic to react to the situation which is already there − nobody gets the idea of deliberately creating tools for the solution of problems which only practice of the tools they create *for purposes they are aware of* will *eventually* cause to emerge. Even when Descartes shaped the tools later used by an Euler, he *did not and could not* foresee what they would make possible. He shaped them more or less accidentally within his particular context, and had no reason to use them more than he did, preparing a future he did not know about.

Already Descartes, however, lived in a mathematical future unknown to Stifel and his abbacist predecessors. Like theirs, *his* mathematical world was one where *problems* served as challenges, and where the ability to solve problems was a ground for prestige; but the problems were no longer those of repeated travels with gain, finding a purse and sharing its contents, buying a horse in common, or finding numbers in given ratio fulfilling conditions corresponding to particular algebraic equations. Descartes, Wallis and their kind were not Humanists – Humanism, in its heyday (the 14th and 15th centuries) had never been interested in mathematics. Petrarch, as I observed long ago in a different context [Høyrup 1994: 211], wrote several biographical notices about Archimedes the servant of his king and the great engineer, and he spelled the name more correctly than the university scholars of his time − but in contradistinction to these he did not know about any of his works. However, as Humanists discovered after 1500 in the wake of the catastrophic *grand tour d'Italie*[43] of the French artillery and after the discovery of the New World, civic utility if restricted to rhetoric and other *studia humanitatis* was useless, civically and in general; civic utility had to encompass technology and even mathematical competence (as reflected in Hans Holbein's *Ambassadors*). In consequence, the Greek mathematicians became interesting, and the *editiones primae* and the first translations of the Greek mathematicians (beyond Euclid and the *Measurement of the Circle*) were produced. ⟦[44]⟧ For French Humanists and post-Humanists like Viète, Fermat and Descartes, worthy problems were therefore those inspired by Archimedes, Apollonios and Pappos. Algebra was available to them, known as the art of solving problems. But it

[43] Cf. [Biagioli 1989]. As Edward Gibbon points out somewhere, the French mercenaries brought greater havoc to Rome than the barbarians of late Antiquity had ever done.

[44] ⟦Moerbeke's translation of an almost full Archimedes was certainly made in the 13th century – but it had virtually no circulation before the 16th century.⟧

need be reshaped (and not only because of its Arabic name);[45] and that was what they did.

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Chapter 32 (Article II.15) Baroque Mind-set and New Science: A Dialectic of 17th-Century High Culture Sarton Chair Lecture, Ghent University, 13 November 2008

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Small corrections of style made tacitly
A few additions touching the substance in ⟦…⟧
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Abstract

The "New Science" of Galileo, Kepler, Harvey, Descartes, Boyle, Steno, etc., and the Baroque in visual arts and literature, are two conspicuous aspects of 17th-century European elite culture. If standard historiography of science can be relied upon, the former of the two was not affected by the latter.

The lecture asks whether this is a "fact of history" or an artefact of historiography. A delimitation of the "Baroque" going beyond the commonplaces of overloading and contortion concentrates on the acceptance of ambiguity and the appurtenance to a "representative public sphere", contrasting with the quest for clarity and the argument-based public sphere of the new science, suggesting that Baroque and New Science were indeed incompatible currents. A close-up looks at Juan Caramuel y Lobkowitz, who was a major Baroque theoretician but also wrote much on mathematics, finding even within his mathematics love for ambiguity. The way his mathematics is spoken about in the Oldenburg correspondence shows that the mainstream of the New Science saw no interest in this.

Contents

The watershed

Modern science – this is generally agreed upon – was inaugurated in the 17th century by characters such as Galileo, Kepler, Harvey, Descartes, Pascal, Huygens, Boyle, Hooke, Steno and Newton. There is also broad consensus that conspicuous 16th-century figures like Copernicus, Tycho, Cardano, Vesalius and Bombelli (not to speak of Dee, della Porta and Paracelsus) opened the way for the breakthrough by carrying the ancient and medieval inheritance beyond the bursting point; but that they left synthesis to a future generation.

Retrospectively, Galileo etc. count as belonging within natural science – the domain which in English in more recent times became *science simpliciter*. However, we do not need to restrict our argument to this domain. The natural law doctrines of Locke and Pufendorf, Hobbes's political theory based on a non-Aristotelian concept of nature, and the *Grammaire générale* of Arnauld and Lancelot are also modern, while (say) Machiavelli forebodes modern political thinking in a way which makes his ancient models crack but does not yet reconstruct.

It is customary to categorize Paracelsus, Copernicus, Vesalius, Cardano, Dee, Brahe, Bombelli and della Porta as "Renaissance scientists", and it is indeed not difficult to point to features of their thought that are widespread within the Renaissance movement. In contrast, there is no tradition for seeing Galileo, Kepler, Harvey, Descartes, Pascal, Huygens, Boyle, Hooke, Steno, Newton, Hobbes, Locke, Pufendorf and Arnauld as exponents of the Baroque, the indubitable general cultural importance of the Baroque for their century notwithstanding.[1]

One may ask – and that is what I am going to do – whether this is a historical or a historiographical conundrum. In other words: is it true that the New Science (or "new philosophy", as it was rather called at the time) and the Baroque represent contemporary but unconnected or perhaps even conflicting cultural currents? Or, have historians of science simply been blind to the relation between the two? Is the Baroque a context without (scientific) texts, or is it simply so much in disrepute among historians of science that they do not wish to associate it with their heroes?

The question dawned to me during teaching the history of the humanities. First I wondered that the Spanish *siglo de oro*, in spite of its importance in the general cultural

[1] The recent "Baroque Science" project of Sydney University should be mentioned as an exception – see http://www.usyd.edu.au/baroquescience. It formulates the contrast in these terms:

> "Baroque" refers to the preoccupation with paradox and contrast, with asymmetry and distortion, with imagery and sensual detail. "Science" is the search for simple, universal structures, eschewing rhetorical embellishment for logical rigor and sense qualities for the austerity of matter in motion

which then allows the project to allow harmony between the two by looking *differently* at 17th-century science.

landscape, seemed not to have left traces calling for the attention of historians of science; then I got the idea that at least the "etymological current" in linguistics might have to be understood within the Baroque framework. In Sweden and Denmark this current is best known through Olaus Rudbeck's *Atlantica* [Rudbeck 1679] – famous in Sweden, notorious in Denmark: charming in Sweden and shocking in Denmark, indeed, the idea that precisely *Swedish* should be the language of Paradise! My starting point was thus not too far removed from that of Gunnar Eriksson's in *The Atlantic Vision: Olaus Rudbeck and Baroque Science* [Eriksson 1994].

Delimitation of the Baroque

This starting point was a mere intuition, and it is not strange that Eriksson and I took different directions when leaving it, Eriksson making a complete portrait of Rudbeck's science, I myself returning to the initial question about the relation between the Baroque and the New Science.[2] In order to make this return fruitful we have to go beyond the everyday understanding of the Baroque as mere "baroque", as mere contrast to the classicist ideal of *edle Einfalt und stille Größe*. Is it possible to define the Baroque, to delimit it, or at least to characterize it?

A first strategy is the chronological approach. It is familiar from the commonsense historiography of music, where everything between Monteverdi and Bach is "baroque music" simply because of its date. This approach is that of Reijer Hooykaas and J. E. Hofmann, among the few historians of science who do mention the Baroque. Hooykaas [1958: 161] speaks of modern science as produced by "scientists of the Renaissance and Baroque periods", whereas Hofmann's ultra-concise *Geschichte der Mathematik* [1953] has the chapter headings "Übergang zum Barock (1450–1580)" (vol. I, p. 100), "Frühbarock (etwa 1550 bis 1650 n. Chr.)" (vol. I, p. 116), "Hochbarock (etwa 1625 bis 1665)" (vol. II, p. 4) and "Spätbarock (etwa 1665 bis 1730)" (vol. II, p. 50).

With this definition, everything is easy. Arnauld is neither more nor less Baroque than Rudbeck. The problem is neither historical nor historiographical but linguistic: the concept is empty, and we may calmly leave it to Occam's razor to dispose of it.

However, according to the same line of thought, Racine is neither more nor less Baroque than Calderón. If we insist that there *is* a difference and do not accept this elimination of the concept of the Baroque from the history of art and literature, then our problem returns. If the Baroque exists as a *particular* current of 17th-century elite culture within which Calderón belongs but to which Racine is in opposition, then it is still legitimate and meaningful to ask whether *this* particular current imprinted the New Science of the 17th century.

[2] My earlier work on the topic is contained in [Høyrup 1997a] and [Høyrup 1997b].

This approach corresponds to René Wellek's reflections on "Baroque in Literature" [1973: I, 195]:

> The term baroque seems […] most acceptable if we have in mind a general European movement whose conventions and literary style can be fixed narrowly, as from the last decades of the seventeenth century to the middle of the eighteenth century in a few countries.

Obviously, a "current" or "movement" cannot be strictly defined. Even a delimitation – the original meaning of the word we translate as "definition" in Euclid's *Elements* – cannot be exact. Yet we may strive to dig out central characteristics, features which distinguish the core of the current but only in weakened form or not all together when we look at its periphery.

In its origin, the Baroque is linked to the Counter-Reformation and the Jesuit order[3] – the latter to such an extent that the [*Grande dizionario Garzanti*] explains "stile gesuitico" as "il barocco, in architettura e in letteratura". In 1563, the Council of Trent issued a decree stating among many other things that ecclesiastical art was to serve the propagation and consolidation of orthodox faith:[4]

> And if any abuses have crept in amongst these holy and salutary observances, the holy Synod ardently desires that they be utterly abolished; in such wise that no images, (suggestive) of false doctrine, and furnishing occasion of dangerous error to the uneducated, be set up. And if at times, when expedient for the unlettered people; it happen that the facts and narratives of sacred Scripture are portrayed and represented; the people shall be taught, that not thereby is the Divinity represented, as though it could be seen by the eyes of the body, or be portrayed by colours or figures.
>
> Moreover, in the invocation of saints, the veneration of relics, and the sacred use of images, every superstition shall be removed, all filthy lucre be abolished; finally, all lasciviousness be avoided; in such wise that figures shall not be painted or adorned with a beauty exciting to lust […].
>
> In fine, let so great care and diligence be used herein by bishops, as that there be nothing seen that is disorderly, or that is unbecomingly or confusedly arranged, nothing that is profane, nothing indecorous, seeing that holiness becometh the house of God.
>
> And that these things may be the more faithfully observed, the holy Synod ordains, that no one be allowed to place, or cause to be placed, any unusual image, in any place, or church, howsoever exempted, except that image have been approved of by the bishop".

That could not and did not determine how and what art should *be*, at most what it should *not* be – the loincloth painted over Michelangelo's naked Christ in the Sistine Chapel is an almost parodic example.

In so far, the emergence of the Baroque can be seen in the perspective of an observation made by Carlo Ginzburg [1976: 146, my translation], regarding

[3] See for instance Arnold Hauser's [1965: 69–72] and Rudolf Wittkower's [1972] discussions.

[4] Translation [Waterworth 1848: 235*f*].

> a problem the significance of which is only now beginning to be recognized: that of the popular roots of a considerable part of high European culture, both medieval and postmedieval. Such figures as Rabelais and Brueghel probably weren't unusual exceptions. At the same time, they closed an era characterized by hidden but fruitful exchanges, moving in both directions between high and popular cultures. The subsequent period was marked, instead, by an increasingly rigid distinction between the culture of the dominant classes and artisan and peasant cultures, as well as by the indoctrination of the masses from above. We can place the break between these two periods in the second half of the 16th century, basically coinciding with the intensification of social differentiation under the impulse of the price revolution. But the decisive crisis had occurred a few decades before, with the Peasants' War and the reign of the Anabaptists in Munster. At that time, while maintaining and even emphasizing the distance between the classes, the necessity of reconquering, ideologically as well as physically, the masses threatening to break loose from every sort of control from above was dramatically brought home to the dominant classes.
>
> This renewed effort to achieve hegemony took various forms in different parts of Europe, but the evangelization of the countryside by the Jesuits and the capillary religious organization based on the family, achieved by the Protestant churches, can be traced to a single current. In terms of repression, the intensification of witchcraft trials and the rigid control of such marginal groups as vagabonds and gypsies corresponded to it.

However, the implementation of the Trent programme was made, and could hardly avoid to be made, on the foundation of existing art, that is, the Mannerist trend, and (since Jesuits were main responsible) with strong regard for Ignazio de Loyola's insight in the importance of the active emotional involvement of the recipient: as explained in §2 of his *Ejercicios espirituales*, the religious message must never be so explicit and direct that the spiritual commitment of the recipient is barred:[5]

> […] if the person who is making the contemplation, takes the true groundwork of the narrative, and, discussing and considering for himself, finds something which makes the events a little clearer or brings them a little more home to him […] he will get more spiritual relish and fruit, than if he who is giving the Exercises had much explained and amplified the meaning of the events. For it is not knowing much, but realizing and relishing things interiorly, that contents and satisfies the soul.

This advice not to tell too explicitly is already in potential conflict with the Trent request that "the people shall be taught, that not [by religious images] is the Divinity represented, as though it could be seen by the eyes of the body, or be portrayed by colours or figures".

The essential point in Loyola's method is not presentation of the religious motif by itself but the motif embedded in a totality of tension, colour and movement. Loyola prescribes thus how to get an "interior sense of the pain which the condemned suffer" (§§65–70, after [Mullan 1914], cf. [de Loyola 2007: 27*f*]:

[5] I follow Elder Mullan's translation [1914], after collating with the edition in [de Loyola 2007: 11].

> The first point will be to see with the sight of the imagination the great fires, and the souls as in bodies of fire. The second, to hear with the ears wailings, howlings, cries, blasphemies against Christ our Lord and against all His Saints. The third, to smell with the smell smoke, sulphur, dregs and putrid things. The fourth, to taste with the taste bitter things, like tears, sadness and the worm of conscience. The fifth, to touch with the touch; that is to say, how the fires touch and burn the souls.

Transferring this principle to the realm of art, Gabriele Paleotti, cardinal and bishop of Bologna, declares in his *Discorso intorno alle imagini sacre e profane* (I, xxv, from 1594; my translation from [Hauser 1965: 71*f*]):

> Telling the martyrdom of a saint, the zeal and constancy of a virgin, the passion of Christ himself, are things that touch the true; but when they are present in live colours, here in front of the eyes the martyred saint, there the virgin assaulted, and on the other side the nailed Christ, this truly increases the devotion and wrings the bowels, so that he who does not feel it is made of timber or marble.

This reveals another aspect of the Baroque: the Baroque work of art is a *Gesamtkunstwerk*, a planned totality where all elements are to fit together – in good agreement also with the connection between the Baroque and court culture. In the terminology of the young Habermas, the Baroque is a "representative public sphere" (*Repräsentative Öffentlichkeit*), the exhibition of "truth" *ad oculos*, beyond possible doubt or debate (though certainly not beyond idiosyncratic personal interpretation).

A strong emotional involvement of the flock impedes criticism and rational doubt and is thus fundamental for the functioning of a representative public sphere; but the clerical insight in its necessity prevented the degeneration of art into one-dimensional didactic, however much the bishops from Trent had aimed at exactly that. The Jesuit Antonio Possevino (1534–1611), friend of Christopher Clavius, thus writes in his *Tractatio de Poësi et Pictura ethnica, humana et fabulosa collata cum vera, honesta et sacra* (1595)[6] that

> the painter should take advantage of the whole of philosophy, in particular of moral philosophy, since the depiction of the soul and the expression of all its sentiments, agitations and other commotions makes the art of painting deserve the highest praise. The soul, indeed, being various, irascible, just, inconstant, and abominable, clement, sweet, compassionate, sublime, vainglorious, humble, proud, and frivolous, he who is able to do that is certainly not lacking in acuteness of mind.

These quotations allow several supplementary observations touching at our topic.

Firstly, we may return to the quotation from Ginzburg and take note of the contrast between Possevino's outlook (which he shared with many Jesuits and with much Jesuit practice) and the one-way moralizing of Puritanism and Lutheran orthodoxy: none of these

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[6] Translated after Paola Barocchi's edition [1978: II, 458].

could accept a similar inextricable conglomerate of good and evil. It may be no accident that witch burning was less common in regions where Jesuit Baroque culture was strong than in Lutheran areas (although, as has been observed, the Spanish inquisition may simply have been too busy burning heretics to bother much about witches).

Secondly, we may notice that erudite Baroque poetry – say, that of Góngora, Donne and Gryphius – is not at all fit to serve "the indoctrination of the masses from above", and in so far not easily related to the Trent decree and its definition of the tasks of (church) art. This kind of poetry can be understood, however, exactly in the context of the way Paleotti, Possevino and others filled out the programme. We may think of this passage from John Donne [1986: 178]:

> I throw myself down in my chamber, and I call in and invite God and his angels thither, and when they are there I neglect God and his angels for the noise of a fly, for the rattling of a coach, for the whining of a door. I talk on, in the same posture of praying, eyes lifted up, knees bowed down, as though I prayed to God; and if God or his angels should ask me when I thought last of God in that prayer, I cannot tell. […] A memory of yesterday's pleasure, a fear of tomorrow's dangers, a straw under my knee, a noise in mine ear, a light in mine eye, an anything, a nothing, a fancy, a chimera in my brain, troubles me in my prayer.

Loyola had also known about such disturbances (*Ejercicios Espirituales* §§346–351 [Mullan 1914] [de Loyola 2007: 84*f*]); but ultimately he ascribed them to "the enemy". The champion of the Counter-Reformation thus could still provide dichotomic simplicity by means of projection and reification; the Baroque poet, like the theoretician Possevino, had come to acknowledge the inherent quiet disorder of the human mind.

On the other hand, and finally, there is a striking contrast between Possevino's words and much of what we find in central representatives of the New Science – Bacon's belief that nature can be reduced to a finite number of forms; Descartes' clear and self-evident truths;[7] the certainty of the geometric method; the conviction of Boyle and others that the *experiment* can establish solid facts; the faith of Descartes, Boyle, Leibniz and others that the mechanized thought of algebra may serve as a general model for the scientific and philosophical method.

General explanations

This latter contrast suggests a first general explanation of the absence of Baroque inspiration in the New Science: the two cultural currents have radically different programmes. We may think of Galileo's vicious remarks about Sarsi alias Orazio Grassi in *Il saggiatore* – the Collegio-Romano mathematician who had dared to suppose a comet

[7] We may also observe that Descartes reproduces Loyola's dichotomy by other means when he separates "the passions of the soul" from the soul itself (which is essentially *thought*).

to be farther away than the moon (and to point out that Galileo could not have performed his experiments too carefully):[8]

> It seems to me that I discern in Sarsi a firm belief that in philosophy it is essential to support oneself on the opinion of some celebrated author, as if when our minds are not wedded to the reasoning of some other person they ought to remain completely barren and sterile. Possibly he thinks that philosophy is a book of fiction created by some man, like the *Iliad* or *Orlando furioso* – books in which the least important thing is whether what is written in them is true.

At first we may believe that Galileo just postulates the incompetence of his opponent – Benjamin Farrington's words from [1938: 437] come to mind:

> There is a phrase that has been much on people's lips in recent times to the effect that science is ethically neutral. It is, no doubt, possible to attach a meaning to this. But it is also surely true that with regard to one, at least, of the cardinal virtues science is not neutral: Science must be true.

However, certain turns in Galileo's assault hint at a more precise aim. Firstly, in the treatise which Galileo attacks, Grassi plays with Baroque rhetoric and metaphors, albeit showing that these *are* metaphors by explaining them; secondly, he permits himself to refer to the testimony of ancient philosophers and even to such poets who – like Ovid and Lucrece – were familiar with mathematics and natural philosophy.

For one reason as well as the other, Galileo can insinuate an identification of Grassi with *probabilism* [Hacking 1975: 23*f*], a doctrine according to which "in matters of faith and morality, it suffices for the assurance of tranquillity of conscience to follow a plausible opinion" [Caramuel 1663a: A3] – where "plausible", that is, *probabilis*, means that an opinion is shared by one of several (possibly discordant) recognized authorities. As observed by the horrified Pascal, the consequence is that most humans will be innocent.[9] That horror may be one of the reasons Pascal and Arnauld created the concept of quantified probability: without quantification, the opposite *probabiliorist* doctrine – that the *most* plausible opinion must be followed – is ultimately meaningless.

Beyond the Baroque acceptance of ambiguity and the tie between Baroque culture and probabilism, we find another global conflict between the Baroque and the New Science as the latter developed in the course of the 17th century. As mentioned, the Baroque was a "representative public sphere" – maybe the most striking deliberate construction of this type of public sphere before the advent of modern advertising. In this respect there is no fundamental conflict with the roots of Modern science in courtly culture, as discussed by William Eamon [1991]. However, from around 1615 the barycentre moved toward

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[8] Translation [Drake & O'Malley 1960: 183].

[9] *Les Provinciales* VI [Pascal 1954: 719].

circles of peers, from the meetings in Mersenne's cell over Gresham College to the creation of the scientific academies (to mention but the emblematic names). Thereby, the ambience of the New Science became an exemplification of the other main type of "public sphere" understood as locus for the creation of collective conviction: the one where "truth" is not displayed but results from discussion based on more or less well-defined shared principles between culturally qualified participants who, with respect to the discussion, are in principle peers[10] – the type of public sphere which the young and still neo-liberalist Habermas believed to have emerged only with (petty) bourgeois society.[11] It is characteristic that striking *displays* of the new truth like those of Otto von Guericke were performed for the Emperor and for the Berlin court [ed. Foley Ames 1994: 168; Krafft 1972: 575]. More characteristic than the display of the Magdeburg hemispheres is what Lorenzo Magalotti, secretary of the Accademia del Cimento, wrote about Leopold, Medici prince and protector of the Academy.[12] Leopold liked

> to act as an Academician, and not as a Prince. He is content to play the second role only on occasions when there is a question of expense, generously supplying the needs of the Academy.

Close-up

Birds eye views are useful. The contrast between the quest for simplicity and clear-cut answers on one hand and the acceptance of and even infatuation with ambiguity on the other is probably a valid contribution to our understanding of why a Baroque influence on the New Science is difficult to discern; the reference to the foundation of the two currents in public spheres of discordant types is also likely to make a cogent point. However, it may be useful to look at these general explanations through the lens of a particular example: a figure whose thinking was deeply rooted in the Baroque mind-set

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[10] This point could evidently be elaborated and modulated. On one hand, the integration of the *Académie des Sciences* in Colbert's state system had as one consequence the introduction of a hierarchy of *pensionnaires, associés* and *élèves*; on the other, printing gave new opportunities for the development of a *republic of letters* encompassing all of those who had received adequate education (in whatever way they had received it). Indeed, the norm that knowledge should be made public (as knowledge that can be *understood*) is already expressed in the 16th century in as unanticipated places as John Dee's *Monas hieroglyphica* from 1564 [ed. Josten 1964] and della Porta's *Magia naturalis* from [1591], cf. [Høyrup 2004: 349*f*, 342*f*], and also Pamela Long's discussion [1991] of the norm of openness as expressed in 16th-century writings on mining and metallurgy]. Since this is not my present theme, I shall restrict myself to these hints.

[11] This expansion of Habermas' conceptual framework is presented in Danish in [Høyrup 1984], together with a discussion of pre-bourgeois instances of an "argument-based public sphere".

[12] Quoted from [Middleton 1971: 56*f*]. Even though the claim may not be fully true (it seems not to be) it illustrates the ideal with which the secretary found it fitting to measure him as an academy member.

and at the same time participated in the unfolding of the New Science, or at least tried to do so – at best participating in its mathematical and natural-science main current.

Two formidable characters propose themselves. One is Athanasius Kircher, the other is Juan Caramuel. Kircher's activity ranges more widely in the natural-scientific field than Caramuel's; Caramuel, on the other hand, is more explicit as a theoretician of the Baroque. I shall concentrate on Caramuel, returning briefly to Kircher, and take up Rudbeck in an aside.

Even though all three are polymaths, we should not necessarily take all polymathy as a characteristic Baroque value: much of it, for instance Alsted's encyclopediae, comes in the wake of Ramism, which in its love for dichotomic simplicity is at least as far removed from the Baroque as is the New Science.

Caramuel's Baroque

Caramuel was born in Madrid in 1606. He studied theology and entered the Cistercian Order at an early age, and died in 1682 after having been bishop, first of Campania[13] and afterwards of Vigevano close to Milan. Many among his more than 70 volumes can be linked to the theory of the Baroque.

One of them is his *Defence of the age-old and universal doctrine, about probabilism. Against D. Prospero Fagnani's new, singular and implausible opinion* [1663a]. The above maxim used to explain "probabilism" was borrowed from the introductory résumé of this work. Further on in the same résumé Caramuel states (exactly one hundred years after the Trent council!) that

> if the theologians will be allowed for another hundred years to constrain consciences with the same force as they have done these last hundred years, then the conversion of the infidels will be made very difficult, and also for the orthodox very great difficulties will most certainly have to be feared.[14]

No wonder that Pascal, convinced of the sinfulness of all men, protests time and again against Caramuel's tolerance in his *Lettres provinciales* (cf. note 9).

Already in [1635], Caramuel had published an *Easy and Clear Explanation of Steganography, or of the Key of the German Solomon, Ioannes Trithemius*. Since Trithemius had introduced it in the early 16th century, "steganography" (the art of concealed writing) was in odour of cabbala and black magic (for which reason Trithemius's book was only printed a hundred years later; cf. [Brann 1999] and [Coulianu 1987: 169]).

[13] Thus in that same depressing area which Christ never reached because he "stopped at Eboli", as Carlo Levi's local interlocutors claimed. For Caramuel no less than for Levi, writing was a way to survive mentally.

[14] *Demonstratur tandem Theologos, ita centum annis ultimis constrinxisse Conscientias, ut si aliis centum eodem impetu pergere permittantur, reddetur difficillissima Infidelium conversio, et apud ipsos Orthodoxos inconvenientia maxima certissimè timeri poterunt.*

Here and everywhere in the following, translations from Caramuel's Latin are mine.

Caramuel exonerated it of all dependency on demonic pact or superstition, understanding the cryptographic technique instead as a way to uncover the secrets of the mind through connotations.[15]

Caramuel's *Metametrica* from [1663b] is an extensive treatment of poetical techniques. Here (p. 1 of the treatise "Apollo analexicus") he phrases the programme that

> The whole machine of the world is full of Proteus. Wherefore let us grasp a Proteic pen, that we may be able to praise Proteus

and he exalts ("Apollo logogriphicus" p. 215) the logogriph as an

> enigmatic song, which digs many significations from the same name, reading backwards, taking away letters or adding others.

If anybody, Caramuel is thus an exponent of Baroque ambiguity, of the use of connotative appeals rather than explicit messages. The word "audacious" (*audax*) recurs in several of his titles – a *Grammatica audax* is the "praecursor logicus" to his *Theologia rationalis* (1554–55), and there is even a *Mathesis audax* from 1644.[16] Even Caramuel's understanding of etymology is, as we shall see, "audacious": It is not necessarily meant to reveal the true historical origin of words but rather, like the logogriph, to reveal concealed possible meanings.[17]

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15 Actually, in a treatise *Cabala, hoc est, secretior interpretatio Sacrae Paginae* (apparently never published but referred to in the initial unpaginated list of Caramuel's publications in [Caramuel 1670]) he did the same to cabala itself, using it to find hidden meanings in the Scripture. In the same place he tells that his *Metametrica* (on which imminently) was nothing but a reinterpretation of cabala, given this new name because of the notoriety of the old one.

16 The full title is nothing less than *Mathesis audax rationalem, naturalem, supernaturalem, divinamque sapientiam arithmeticis, geometricis, catoptricis, staticis, dioptricis, astronomicis, musicis, chronicis, et architectonicis fundamentis substruens exponensque*. I have not been able to see it, but according to the secondary literature it deals with combinatorics (that is, we may observe, the mathematics of the anagram), meant to replace and outdo Aristotle's *organon* as a universal key to all sciences – see [Lüthy 2006: 128; Martínez Gavilán 2001: 118; Pérez de Laborda 2005: 282–284]. ⟦Having finally seen the volume [Caramuel 1644] I can add that combinatorics is only one topic about many dealt with - - the list in the title is indeed quite adequate.⟧

17 On this point, reading of Caramuel may elucidate Rudbeck's programme. In *The Atlantic Vision*, Eriksson [1994: 134] states that for Rudbeck the etymologies of the *Atlantica* have "a rather small degree of credibility", because Rudbeck compares them to "ornaments and paintings" on a building, whose walls and roof are constituted by the ancient written sources, whereas Swedish nature itself makes up the fundament. However, the text which Eriksson quotes continues in a way which shows that something different than mere low credibility is at stake (I quote from Eriksson's translation, repairing an omission):

> Ornaments and paintings do not please all in like measure, for as one person wants green the other wants grey, when the one likes Doric the other likes Jonic. With this I mean the style and the origin of words, for maybe one is more pleased if Neptune has his origin

Caramuel the mathematician

After 1663, the Jansenists and the Dominican probabiliorists got the better of Caramuel, and he was no longer allowed to persist in moral and theological tolerance. Instead he published two huge volumes in [1670] about one of his other interests, namely mathematics. His *Mathesis biceps*, divided into "old mathematics" and "new mathematics", runs over more than 1800 folio pages.

About this work – the only one he mentions in his short "scientific biography" – Juan Vernet [1971] tells that

> although it contains no sensational discovery, [it] presents some original contributions to the field of mathematics. In it is expounded the general principle of the numbering systems of base n (illustrated by the values 2, 3,..., 10, 12, and 60), pointing out that some of these might be of greater use than the decimal. He also proposed a new approximation (although he did not say so) for trisecting an angle. Caramuel developed a system of logarithms of which the base is 10^9, the logarithm of 10^{10} is 0, and the logarithm of 1 is 10. Thus, his logarithms are the complements of the Briggsian logarithms to the base 10 and therefore do not have to use negative characteristics in trigonometric calculations. In these particulars Caramuel's logarithms prefigure cologarithms, but he was not understood by his contemporaries; some, such as P. Zaragoza, raised strenuous objections.

This could make us believe that Caramuel's mathematics is as easily separable from his Baroque poetics as Newton's *Principia* from his "chronology of ancient kingdoms" [1728]. This, however, turns out to be yet another confirmation of Léon Rodet's principle [1881: 205] that "when studying the history of a science, exactly as when one wants to obtain something, one should 'rather ask God himself than his saints'".[18]

> from bathe or depict rather than from ruling the sea, and Hercules rather from being the Honour of Juno (the weather) or etc., than from being a warrior chief.
>
> The walls and the roof are what I call the writings of the ancients with which the building is put together. If they do not tell the truth, neither could I. For I did not live in the time of Troy or before.
>
> The foundation is what I call the country of Sweden, its lakes, mountains and streams and other such things through which the ancients have described Sweden's certain position, all of which features remain undisturbed until the stone, mentioned by Daniel, who himself planted it, falls from heaven crushing everything.

The ancient written sources thus have a lower credibility, compared to the arguments from geographical facts. Etymologies, on the other hand, are a domain which allows audacious subjective choice and where "the least important thing is whether what is written [...] is true".

[18] Pour étudier l'histoire d'une science, tout comme pour obtenir quelque chose, « il vaut mieux avoir affaire au bon Dieu qu'à ses saints ».

⟦With Jed Buchwald and Mordechai Feingold [2013] it may be added that even Newton's chronological work are not as separate from the *Principia* as conventionally assumed.⟧

At first we may look at what Vernet sees as a presentation of “the general principle of the numbering systems of base *n*”. It turns up as a *meditatio prooemialis* before the treatment of arithmetic (proper), and is an answer to the question (p. xliii)

> whether arithmetic be one, or several? If several, which they may be? And how do they differ from each other? Are they practical, or speculative? And are they necessary?

Caramuel’s intention is not to produce a “general principle” but exactly the opposite. After having described place value systems with base 2, 3, 4, 5, 6, 7, 8, 9, 10, 12 and 60 (explaining where each may be useful) and shown how to calculate in base 2, 3 and 4 he concludes (p. lxvi; italics from the original) that

> Firstly, it is thus established *that several arithmetics are possible, which differ from each other:* indeed, as there are various languages in the world, so they can be dissimilar, and varied with respect to the first return of the unit. I intend, 2, 3, 4, etc.,[19] as we have shown above.
>
> Secondly, it is established that *all these arithmetics are analogous:* indeed, as all languages agree analogically in their flow, similarly, or certainly even more strictly, the arithmetics agree.
>
> […].
>
> Thirdly, it is established *that before the operation of the mind there is neither number not arithmetic. Truly, numbers are entities produced by the intellect:*[20] *and that the return of the same numbers depends on human free will; and that these go back to the beginning at so many, and neither by more nor fewer units is because it pleased those who first fashioned arithmetic thus and not otherwise*. […].

We may find the level rather elementary, but Caramuel was none the less the first to publish about different place value systems and describe algorithms for calculating with them.[21] The metamathematical stance is even more original, too original indeed to the taste of mathematicians: only the non-Euclidean geometries of the 19th century led some mathematicians to accept this kind of pluralism; most, even then, only accepted the non-Euclidean variants as genuine geometry when Felix Klein had reduced even this pluralism to a single “general principle”. It was never the prevalent habit of mathematicians to stress free subjective choice.

[19] The meaning is that in the dyadic system, the unit “returns” as 2 (which will be written 10), etc.

[20] In the next paragraph, Caramuel emphasizes that numbers are not chimerical figments of the mind but *formed* by the mind, and that *after* the operation of the mind they truly exist in things. His “relativism” is Einsteinian (no frame of reference is privileged, but all are equivalent and translation is possible), not postmodern in Feyerabend-Latour style.

[21] Harriot had done as much before, but in an unpublished note [Ineichen 2008]. In the early 13th century, Jordanus of Nemore had explained the possibility of place value *fractions* with different bases, speaking of them as “consimilar fractions”, and confronting with “dissimilar fractions”, ascending continued fractions with changing divisors.

In the *Mathesis biceps*, on the contrary, subjective choice turns up even in places where we would expect Caramuel's choice to be anything but free. Time and again he returns to the choice between the Copernican, the Tychonic and the Ptolemaic world system, and as we should expect from a Catholic bishop in 1670 he rejects the Copernican option. His formulations, however, are not as we would expect. He does not say that this system is contrary to Sacred Scripture but (p. 1392b) that "the cardinals have declared it to be contrary to Sacred Scripture" (which indeed they had; the statement is preceded by a list of "famous mathematicians" – Galileo, Kepler and others – who support the Copernican system).[22] Caramuel's own opinion is stated in phrases like "for me, the earth stands still" (p. 1581a) or (p. 1400b)

> *We* have no need for that which the Church has condemned. When hence the Copernican system has been rejected, the two others remain in court. The Ptolemaic system is implausible [*improbabile*]: Nobody can indeed deny that Venus and Mercury move around the sun. Thus the Tychonic system stands.

Algebra!

Caramuel wrote long after Cardano, Bombelli, Viète and Descartes, and it therefore seems adequate that algebra is treated on 108 folio pages. What is immediately striking is that these pages belong to the first volume, "ancient mathematics". However, this location turns out to be well-founded. Nothing of what these four authors had done has indeed left the least trace in Caramuel's algebra.

This does not mean that Caramuel just explains or repeats what can be found in algebraic writings from the earlier Renaissance or the Middle Ages. As far as I know, *no* precursor ever dealt with the material as does Caramuel. His basic idea – a *free choice* if any – is that algebra or "abstract proportion" is an extension of the "false position" and the rule of three.[23] For this reason, his algebra never goes beyond the first degree,

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[22] Elsewhere (p. 105) he declares the stance of the cardinals to be *prudent*, because nothing in the Sacred Scripture suggests the earth to move, but much that it rests.

[23] Both terms may be in need of explanation.

First the "false position", which may be "simple" or "double". A number, to which $^1/_7$ of itself is added, gives 19. In the "simple" variant we make a convenient but probably false guess – for instance, that the number is 7. Adding its $^1/_7$ gives us 8 – but we should have $^{19}/_8$ as much. Therefore, our guess should also be multiplied by $^{19}/_8$. In the "double" variant we make two guesses (for instance, 7 and 21), and find the result as a mean, weighed in inverse proportion to the two resulting errors (the principle of alligation).

Next the "rule of three". 3 sacks of flour cost 17 shillings, what is the price of 4 sacks? The rule, as it is formulated in late medieval abbacus books, prescribes that we multiply [the counterpart of] the things we want to find (that is, 4, namely 4 sacks) by the magnitude which is not of the same kind (17, namely shillings) and divide by the third magnitude (3, sacks).

Both methods, we see, are rather *alternatives* to algebra as we know it from the medieval

even though his presentation of algebraic symbolic notations suggests a notation for higher powers which in principle is related to that of Bombelli (but so different in its concrete shape that borrowing can be excluded).

I shall take up only a few aspects of Caramuel's idiosyncratic algebra. At first (pp. 99–110) comes a philosophical deliberation whether it is possible to derive true conclusions with necessity from false premises. This deliberation is necessary precisely because of the identification of algebra with the "false position". Caramuel rambles widely. He moves through the schemes of Aristotelian logic (not least of course *modus tollens*, the "indirect proof" whose schematic figure called *barocco* may indeed have given rise to the nickname which the Enlightenment gave to 17th-century art[24]); the fictions of legal thought; *theorica planetarum* with its falsely assumed epicycles and crystal spheres; logarithms; the indirect proofs of mathematics; and finally the false position. The conclusion is a denial of the possibility – Caramuel, like Farrington, thinks that "science must be true", his steganography and logogriphs should not be seen as a rejection of the demand for truth; but as Paleotti's way towards piety, the path toward truth may be indirect, poetical rather than through explicitly argued discourse.

This is clearly exemplified in what follows on pp. 117–119: an etymological investigation of the origin of the names of algebra: *algebra*, *cossa* and *almucabala*.

First comes a philological discussion of the proposal to derive *algebra* from the name Geber, a discussion which is at the level of 17th-century standard philology at its best. Caramuel objects that Geber Hispanus (Jābir ibn Aflah) must have lived around the 12th century, which is too late. But then Caramuel goes on, at first with a borrowing from Alsted's *Encyclopaedia* [1630: III, 844a]:

> *Algebra is an arabic word, which means the* doctrine of the excellent man*: AL, indeed, is the article:* GEBER *means* Man*: and it is often a title of honour, as with us* Master*, or* Doctor. *Today this book is much venerated among the erudite nations of the Orient, and by the Indians who are very fond of this art it is called* Aliabra*, or* Alboret*, since they do not know the proper name of its creator.[25]* Certainly ג ב ר, GABAR, in Arabic is *restored. And as the article is* א ל, AL, prefixed*, the restoration of arithmetic was* אל- ג ב רא.
>
> But why do we call the same science *cossic*, and the special numbers which it makes use of, *cossic numbers*? *In Tome* 2, *book* 14, *chapter* 4, § 1 in Alsted: *Moreover, Algebra was called the art of* res, and census *by certain Latin writers; as with Regiomontanus;*

treatises than fundaments for it.

24 The other standard etymology derives the word from Portuguese *barroco*, an imperfect pearl. As far as metaphorical value is concerned, one explanation is as plausible as the other (which certainly would have pleased Caramuel if the word in its current meaning had existed in his times). ⟦Émile Littré [1873: I, 300] takes it for granted that the Portuguese word is borrowed from the scholastic logical term, which would reduce the two etymologies to one.⟧

25 Here the first borrowing from Alsted ends.

by the Italians (read, *by the Spaniards*)[26] Arte de la cosa, *from which* Cossa. *Christoph Rudolph, excellent master of this art, considers that the rule is called* Cossic, *as* Art of things, *because it serves to solve questions about hidden things: after the manner in which arithmetic books usually express themselves in all problems*, We lay down a thing. *Further, by certain Greeks Algebra was called* Analytica. *They also*, etc.[27] […] And there are in Europe two current names, *Regula di tre* [the rule of three], *and Arte de la Cosa*, the former Italian, the latter Spanish, which clearly indicates how much these two nations have promoted, adorned and made illustrious arithmetic.

Further, if you do not want to favour the Spaniards, you shall say that the term *Cossa* comes from the Hebrews or the Arabs to the Greeks and the Latins. Indeed כ ס ר, *Casar*, with the Saracens is *to Break* [*Frangere*], and therefore should mean the science which considers broken numbers [i.e., fractions]. Add to this that one may derive an etymology from the roots ק צ א QAZA, *Judged*, and ק צ ר, QAZAR, *was Brief*: indeed, this science is a kind of arithmetic which is fit for judging, and most sure in matters concerned with numbers. An indication that it solves with utmost security and concision difficulties which ordinary arithmetic is hardly able to solve when moved in roundabout ways and labyrinths.

Johannes Geysius[28] explains the word differently. In *Book* 1 *on the Coß*, chapter 1, he says COSSA *comes from* כ ס ה, CASA, *that is*, Weaved; *it teaches indeed to find a number which has been hidden. Etc*. This *indeed* I do not understand, since "to weave" [*texere*] is not "to reveal" [*detexere*]. Say thus that this ability was named from weaving because it disentangles numbers which have been woven together and intertwined; so that the denomination refers not to the science but to the object.

In Greek it can also be called **ΚΟΣΙΚΗ**, since **ΚΟΣΙΜΒΟΣ** is a *Knot*.[29] And actually, all problems which are treated by this science are knots which you cannot solve if not by breaking (dividing unity). And also, if anybody is audacious, from *Cos*, a Latin word, *Cossica* is almost as saying *Cotica*. The mind actually needs a whetstone [*cos*] in order to be sharpened, and this science sharpens the mind, which is often dulled by badly digested methods. But even the small worms which bore through the hardest tablets are called *Cossi* by the writers on natural history. Also, if anybody is audacious, the name may be drawn from here. Indeed, if the multiplication table is easy and can be penetrated by any mind, others are hard, and cannot be penetrated if not by learning the *Cossic* art.

Further, it follows from Johannes Geysius's *Book* 1 *on the Coß,* chapter 4 No. 4 that *Coß* and *Algebra* are the same thing. There he says, *It is also called* ALMUCABALA, *that is,* Hidden tradition; *and also* ALGEBRA, that is, Magisterial Art. Etc. And Alsted, who

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[26] This correction is inserted by Caramuel, who has not forgotten his Spanish origin even though he is a bishop in southern Italy. According to my work on the beginnings and background of Italian abbacus algebra Caramuel may indeed be right in as far as the first algebraic use of the word is concerned.

[27] Alsted goes on "They also called normal arithmetic synthetic", and explains that with reference to other works from the Ramist tradition.

[28] A parson [Hotson 2007: 220] and, as we see, amateur mathematician at the *Rechenmeister* level, who wrote 10 pages (vol. III, pp. 865–874) on algebra in Alsted's encyclopedia. Alsted refers to him as *cossista*.

[29] The spelling ought to be κοσύμβος, which actually is something made from knots (a hair-net etc.); but Caramuel's translation agrees with that of dictionaries from his century.

in *Tome 2 book* 14 § 1 says, *It is told that there was one remarkable Mathematician, who wrote down his art in Syriac language and sent it to Alexander the Great, and called it* ALMUCABALA, *that is, book on hidden things* (*this Art, indeed, teaches how to find a hidden number*), *the doctrine of which others preferred to call* ALGEBRA. None of them expresses the precise meaning of the word. Indeed, קבלה is Tradition, from the root קבל QABAL, *to transmit*. Since they would not divulge it, they did not transmit it in writing but orally to disciples. מקבלים MAQABALIM are *Cabalists*, and when the article is added it could be called AL-MUCABALA, not in Syriac but in Arabic.

ΕΝΑΡΙΘΜΟΣ is said about the one who is appreciated, a distinguished and extraordinary man: from which ΕΝΑΡΙΘΜΙΚΗ, some noble and distinguished kind of arithmetic, which is appreciated by learned men.

But one may also call this thing ΜΕΤΑΡΙΘΜΙΚΗ which has gone beyond the measure of common arithmetic and traverses the fields that lay beyond it.

It should be obvious that Caramuel does not believe that the etymologies from *Casar* onwards are historical truth. They are propounded for the case "you do not want to favour the Spaniards"; some are "audacious", and repeatedly two alternative explanations are combined into one figure (as *qaza* and *qazar*). As the steganography and the logogriph, these etymologies are meant through poetical play to dig out – or rather display – aspects of the nature of algebra. That these aspects are indeed prior to the etymologies can be seen for instance from the example ΚΟΣΙΚΗ/κοσύμβος: only the one who already knows that he wants to get to broken numbers (that is, to transcend the Greek concept of number as a plurality of units) will find it in *knot*.

The Reception

As we see, Caramuel's *Mathesis biceps* is soaked with ambiguous and poetical Baroque subjectivity – so far removed from the Counter-reformation "constraint of consciences" that only familiarity with the mediating process allows us to discern the connection. Caramuel's Baroque is no external aspect, no mere decoration, as Grassi's poetical references in the treatise about the comets: it inspires the investigation of the "plurality of arithmetics" and allows the understanding of algebra as an abstract version of the false position. Even when writing about mathematics, Caramuel remains a Baroque mind.

Was he a mathematician all the same? The creators of the New Science appear to have nourished some doubts – as Vernet points out, they did not understand the new mathematical ideas contained in the *mathesis biceps* (Leibniz had to reinvent the place-value system), and the rest did not interest them. If we look for references to Caramuel in Oldenburg's correspondence[30] we do not find much. In 1668 John Collins (vol. V, p. 213) lists his *Solis et artis adulteria* as one of those books in a catalogue "which I doe not much desire unless cheape"; in 1669 (vol. VI, p. 228) he asks Oldenburg "how he

30 [Hall & Boas Hall 1965]. Where the letters are in Latin I quote from the stylistically faithful English translation of the editors.

approoves the treatises of John Caramuel Lobkowitz Intituled Ingeniorum crux et Mathesis audax". Oldenburg (vol. VI p. 234) forwards the question to René-François de Sluse, who answers (vol. VI, p. 525) that "I saw the *Mathesis audax* and *Sublimium ingeniorum crux* very many years ago, but saw them only, nor does any memory of them remain". In 1670 Sluse offers (vol. VII, p. 256) to get hold of the *Mathesis biceps* when it becomes available. In the meantime, Oldenburg has received a letter from François Vernon (vol. VII, p. 273), which refers to

> a great Vast Bulke of Caramuel, Able to fill a Library. His Mathesis biceps, speculative & Practicall [*sic*] 2 vol in Folio. His Calamus 2 volumes more [i.e., *Metametrica*] & whc is worse hee is [not] contented with the loade hee hath laid on the world already. but he promiseth to Plague it wth I doe no know how many volumes more.

In consequence, Oldenburg answers (vol. VII, p. 368) that

> As for the two ample volumes of Caramuel Lobkowitz, we understand them to be damned with faint praise, which has cooled our desire to see them.

Sluse, on his part, concludes (vol. VII, p. 484) after getting hold of the volumes that

> I have looked through Caramuel's farrago, and indeed, to speak kindly, its utility does not seem proportionate to its bulk.

This was all – neither much nor very positive.

Historical or historiographical problem?

An examination of Kircher's works and their reception would lead to similar results. Most of his works about nature deal with issues and objects for which it was less easy to judge the validity of new results and proposals than in the case of mathematics – magnetism, the subterranean world, applied acoustics – so the rejection is less absolute. Yet the difference is not significant – when Kircher approached nature as a *Gesamtkunstwerk* or *theatrum* it was difficult to find a perspective which was theoretically fruitful or seen as such by the representatives of the New Science.

In so far we may say that the absence of a Baroque impact on the New Science is a fact of history, no historiographical blind spot. Works which were too close to the Baroque current with its emphasis on ambiguity and poetico-connotative understanding were too far removed from the sensibility of the New Science to gain much influence. When they offered new answers, these were to questions which seemed outdated or irrelevant, or they concerned matters that were too complex to allow the answers to be convincing.

However, once work with the Baroque prototypes (not "ideal types", since Caramuel and Kircher were quite real!) has opened our eyes to characteristic Baroque features, we may find such features elsewhere though as a rule not together. I shall not go into details but just suggest three sketchy examples.

First we may think of Scott Mandelbrote's distinction between two kinds of natural theology in 17th-century England [2007: 451*f*]: on one hand the "Wilkins-Boyle" type which

> stressed the importance of the providential ordering of nature and the consequent lawful operation of the universe as a proof of divine superintendence and of the power of the divine will

on the other that of the Cambridge Platonists, which based its argument on

> appeal to the wondrous activity found in nature, of which regularity was only ever a part, and which required the constant, creative involvement of a hierarchy of spiritual agents

and which

> was ultimately weakened by its association with credulity and with discredited attempts to prove that spiritual agents could be observed at work in the world.

The spiritual agreement of the latter group with the Kircher we know for instance from the *Musurgia universalis* is not perfect, nor is it however totally absent. The reasons for rejection are also fairly alike.

Next, we may turn our attention to the title pages and frontispieces of scientific printed works of the epoch. The point is not that these look very much like other visual art from the epoch, and thus as "Baroque"; this could hardly be otherwise.[31] Significant is that they served to carry a message by indirect, metaphorical means about the trustworthiness and legitimacy of the book under the frontispiece. As stressed by Volker Remmert [2006], however, the message of the frontispiece was not only distinct from the technical argument of the book, which it would indeed be hard to translate into emblematic pictures; it was also largely directed at a different audience, an audience that was hardly able to follow the technical discourse. The text of books was thus directed at the argument-based public sphere of the "republic of letters", whereas the frontispieces – which were indeed so detached from the argument of the book for which they were produced that they might be transferred to quite different books – were directed at a distinct, representative public sphere.

We may finally ask whether the tenacious dedication of certain late-17th- and early-18th-century *virtuosi* to the study of insects, worms and microscopic animals irrespective of the scandalized antagonism of galant society and writers like La Bruyère and Addison [Daston 2002: 29*f* and *passim*] can not be seen as a sympton of Baroque obsession with everything proteic.

[31] Similarly, Eriksson's observation [1994: 164] that "we must admit [the] striking baroque character of Newton's monument in the Westminster Abbey" can never be an argument that Newton alive had been a "Baroque scientist".

If such suggestions of Baroque presence are taken into account, we may conclude that the total absence of the Baroque from the historiography of 17th-century science is also to some extent a historiographical artefact. But this is a different story which I shall not pursue.

A third story – no less important, perhaps, but which I shall not take up even sketchily – is the *modernity* of the Baroque. Not, of course, in relation to modern *science*, but as the starting point for an understanding of the nature and tasks of poetry that was to unfold in the aesthetic theory of the twentieth century (see [Koch 1983] and [1994]) – and (less flattering perhaps for the 17th century but quite to the point if we think of the initial intertwinement of the Baroque with Counter-reformation propaganda) in relation to the contemporary calculated use of emotion, ambiguity and indirect messages in the advertisement industry.

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